Author's response to reviews

Title: Catheter-based distal sciatic nerve block in patients with Charcot-Marie-Tooth disease

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Author's response to reviews: see over
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Version 2 Date: 22 October 2013

Author’s response to reviews: see over


October 22, 2013

The Biomed Central Editorial Team


Thank you for considering our manuscript for publication in your journal. We have revised the above manuscript according to your reviewer's comments.

Reviewer: Xavier Capdevilla

Major Compulsory Revisions

Did the authors do a retrospective data study or a prospective observational study during 7 years. Are the patients consecutive patients?

- This is a prospective observational study. This is and was stated in the last sentence of the Background section. To clarify our study design we added "consecutive" in the first sentence of the Methods section in the Abstract as well as in the first sentence of the Methods section.

The patients not only received a SNB but in fact a multiple bolus SNB (SN catheter), This is really different.

- The reviewer is right; all patients received multiple bolus of ropivacaine. To clarify this we have changed the title of the paper. The title now is “Catheter-based distal sciatic nerve block in patients with Charcot-Marie-Tooth disease”. In addition, we have added the adjunct “catheter-based” in the first sentence of the Methods section of the Abstract as well as in the second sentence of the Methods section.

The reviewer understands that the study (data) began in 2003 but why using a Nerve stimulator in these patients. We know perfectly that all these neuropathic degenerative disease patients suffer from myelin defect or axonal injury which badly interferes with nerve stimulation possibilities (Stines et al. RAPM 2003, Koff et al Anesthesiology 2008...). Ultrasound guidance is definitely better in these patients
• We agree with the reviewer's opinion that ultrasound guidance facilitates placement of a catheter in these patients. Unfortunately at the time we performed the study ultrasound was not available at our institution. Now we have it and use it. We have added the following short section to indicate this in the Discussion section (page 11). "A limitation of this study is the fact that we have not used ultrasound for placement of the catheter, now widely accepted as gold standard. When designing and performing this study, ultrasound was not available at our institution. We are convinced that the use of ultrasound probably may have reduced the number of attempts and shortened the time for catheter placement."

The authors compared three groups totally different, without any randomization: one group with a quite normal nerve stimulation status (10 patients), one group with a very difficult nerve stimulation (probably with a higher neuropathic disease and as such less able to suffer from surgical pain, Gebhard RE et al RAPM 2009) and patient refusing a SNB. We don't know anything about the preoperative status of the patients. The difference in pain relief is not surprising for the reviewer.

• "For ethical reasons randomization of the participants was not possible". This is now stated in the Methods section on page 5. The common clinical finding in all these patients was an increase in walking disability and patients' wish for a surgical intervention to overcome these problems. The preoperative status varied between ASA I to III (table 1). It was not possible to assess patients' pain sensation in advance. It is known that sensitivity to pain varies widely among CM T patients (Padua et al. Neurosci 2008; 29:193, Ribiere et al. Annals of Physical and Rehabilitation Medicine 2012; 55:160). Assignment to groups was based on electrophysiological data at the end of the study. As far as the authors know up to now there is no investigation published documenting a correlation between disease progression and pain sensitivity in CM T.

What kind of surgeries we have. Foot or Ankle surgeries are very different concerning the value of postoperative pain.

• Most surgical procedures were osteotomies to correct the so-called "Charcot foot" (pes equinovarus). We have added a new column in table 1 documenting the surgical procedures.

How can the authors do to realize a lateral approach of the popliteal fossa and a cephalad direction of the needle on 8 to 10 cm ??
• To identify the sciatic nerve several approaches are described. We used the lateral popliteal approach. This approach is outside (lateral) the popliteal fossa between biceps femoris and the tendon of the vastus lateralis muscle. The inserting point was 8-10 cm cephalad of the femoral epicondyle (as stated in the Methods section). The aim of this technique is to place the catheter as near as possible to the sciatic nerve before dividing into the tibial and common peroneal nerve. (see also McLeod and al. Can J Anaesth 1995; 42:765)

Why the authors accept to have in the same group a NS of the tibial nerve and of the common peroneal nerve as a target. It is perfectly known that the characteristics of the nerve stimulated interferes with the result of the final nerve block.

• It is correct that accepting a dorsal flexion may not be the best response. However, as stated in the text in some patients it was overall difficult to get any response. So we accepted both a dorsal or plantar flexion.

Why the authors did not use a multimodal postoperative analgesia regimen? Why not using a PCA infusion in the postoperative period? Who injected the bolus of LA? A doctor, a dedicated nurse?

• Indeed this is a pilot study approaching to the important problem of the appropriate use of regional anesthesia in patients with pre-existing neurological diseases. We preferred the administration of single bolus of LA through the catheter for several reasons:
First, we really did not know how pain sensitivity would be after the performed surgery in these patients. Therefore, we administered bolus of LA on demand.
Second, before a bolus of LA was given a short neurological assessment was done to exclude an apparent neurological deficit of the leg. We have stated this now in the Method section on page 7 as follows; "Prior to the bolus was given by an anesthesiologist patients were asked whether motor function and sensitivity of the leg were as "normal.gl"
Third, we used bolus of LA without an additional medication for pain relief to check whether our block has any effect.
In addition, in case pain relief by the LA was not sufficient patients received paracetamol or piritramide. This is summarized in table 2.
The conclusion of the abstract is not in concordance with the results of the study

- The conclusion of the Abstract has been modified as follows;
  In our small series catheter-based distal sciatic block within CMT patients had safely been used for pain relief up to three days. The infusion of local anesthetics via a catheter was not associated with any complication.

Minor Revisions

Some typos (Ropivacaine 0.02 in spite of 0.2% ....)

- Typos of ropivacaine (mg/mL) was changed throughout the manuscript

English editing would be necessary in some parts

- The manuscript has been reviewed by a native speaking colleague.

Please focus the main part of the discussion on the results related research interests

- We have omitted the following sentences in the Discussion section (third subsection formerly page 10): The course of CMT disease is characterised by chronic demyelination and peripheral fibre loss. Our hypothesis is that these degenerative transformations are responsible on the one hand for the difficulty of localising the sciatic nerve by means of electrical stimulation and on the other hand for the reduced analgesic demand.
  In addition, we have also deleted two another sentences in the Discussion section (fourth subsection formerly page 10): The natural history of CMT is characterized by slow progression (mainly decrease in muscle strength) with stable and dynamic periods varying greatly among individuals[11]. Therefore, in our opinion the status reported by the patients is in accordance with a normal natural history. Level of interest: An article whose findings are important to those with closely

Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.
Reviewer: Dan Benham ou

Reviewer's report

Title: Distal sciatic nerve block in patients with Charcot-Marie-Tooth disease

Version: 2 Date: 26 August 2013

Reviewer: Dan Benham ou

Reviewer's report:

Major Compulsory Revisions

There are also several concerns which require modification of the manuscript:

1. The authors have used nerve stimulation (this is understandable given the fact that the series started in 2003) but at any time is ultrasound guidance discussed as a technique that could replace nerve stimulation. The use of ultrasound guidance has already been described in patients with diabetes in whom nerve stimulation can be difficult and sometimes requires high current intensity. This contradicts the notion that the use of nerve stimulation should be maintained to separate patients with high and low current intensity.

   • The reviewer is right regarding ultrasound-guided insertion of a perineural catheter. As the study started ultrasound was not available at our institution. Now we have it and use it. We have added the following short section to indicate this in the Discussion section (page 11). “A limitation of this study is the fact that we have not used ultrasound for placement of the catheter, now widely accepted as gold standard. When designing and performing this study, ultrasound was not available at our institution. We are convinced that the use of ultrasound probably may have reduced the number of attempts and shorten the time for catheter placement.”

   The notion that nerve stimulation should be used was omitted.

2. The conclusion that peripheral nerve block should be avoided in patients in whom high current intensity is needed is somewhat surprising. Moreover, it is suggested that these patients may not benefit from a sciatic block. Nothing in this report suggests such a conclusion. These patients required less analgesics that those with low current intensity which suggests an increased sensitivity to analgesics or an increased pain threshold. Because there was no complication and because doses of local anesthetic and
analgesics were adjusted to each patient's need, it is difficult to see why such a conclusion is drawn. Moreover, stating that when a high intensity is obtained, the block should be abandoned is even more surprising and somewhat unethical. The conclusion certainly needs to be modified both in text and in the abstract to better integrate what is known in other diseases and particularly in diabetic patients. The first sentence of the Discussion section also requires some change.

- According to the reviewers' suggestion, we have completely changed the Conclusion sections; Conclusion Abstract now: "In our small series catheter-based distal sciatic block within CMT patients had safely been used for pain relief up to three days. The infusion of local anesthetics via a catheter was not associated with any complication".

Conclusion Discussion now: "This study shows that in CMT patients a sciatic nerve block can safely be performed and may help to reduce postoperative opioid consumption. The infusion of local anesthetics up to three days via a catheter was not associated with any complication."

We have changed the introduction of the Discussion section as follows: "In this study we investigated the efficacy, safety and patient satisfaction of a catheter-based distal sciatic block in CMT patients. The results of this study suggest that peripheral nerve block can safely be performed in patients suffering from CMT. Application of local anesthetics via a catheter up to three days provided sufficient analgesia without any complication."

3. In the Methods paragraph, the authors state that pain scores were recorded. However, as far as the reviewer is aware, results are not provided in the manuscript while this would have been very useful for better results interpretation.

- The reviewer is right; we have not documented the complete pain scores. The target for pain therapy was to achieve a pain score ≤ 3. If patients had a pain score > 3 LA via the catheter or additional analgesics were given for pain control (as stated in the Method section page 7 line 8). The analgesics requirements are shown in table 2. The total amount of analgesics given correlates well with the efficacy of the nerve block.

4. The authors increased electrical charge by increasing duration of pulse. This is rather unusual since in most cases, it is current intensity which is increased (and duration maintained at the same value). Please comment.
This is an interesting and difficult question. The typical finding in CMT is an overall decreased electrical excitability caused by nerve demyelination. Demyelination and loss of motor axons decrease in addition conduction velocity. Electrophysiological studies show that in an impaired nerve a higher current duration may be necessary to generate a motor response. We have not investigated this problem but based on these rare reports we used a priori a higher current duration (see also Meulstee et al. J Neurol Neurosurg Psychiatry 1997;62:398 as well as Szerb et al. Reg Anesth Pain 2005;9:963). It is also known that unm yelinated fibers need higher stimulus duration.

Minor Essential Revisions

1. Please use consistently US or UK style (as recommended by the Journal).
   - We use now US style throughout the manuscript

2. Abstract: ropivacaine 0.2 % (i.e. 2 mg/m L)
   - % was replaced by mg/m L throughout the manuscript

3. Abstract, L14: analgesic consumption
   - 'drug' was omitted

4. Background: please state more clearly that CMT disease is a demyelinating disease.
   - Background; we have added the following sentence page 4 line 5: "Demyelination of peripheral nerves cause marked slowing of nerve conduction velocity and a decrease in compound motor and sensory nerve action potentials."

5. Methods:
   a. Using a dorsal flexion of the foot is not an ideal response (by contrast searching for a plantar flexion is associated with better block results).
      - It is correct that accepting a dorsal flexion may not be the best response. However, as stated in the text in some patients it was overall difficult to get any response. So we accepted both a dorsal or plantar flexion.
   b. 0.375 % ropivacaine: please use mg/ml all along the text
      - The specification of ropivacaine was changed.

6. Table 2: mA instead of mAm p and piritramide
• .m Amp was replaced; the spelling of piritramide corrected.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests
Suggestions by the Editor

Corrections suggested by the Editor.

Editor's Comment:

P9, L13: replace indicate by suggest.

- Done

P10, L7: Replace Ban by Tsui et al (note Ref 13: The first author's name is Tsui)

- Ref. 13 is now properly cited (Tsui et al).