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## Do continuous 'lumbar plexus' blocks really increase the risk of falls?

Editor—I read with interest the recent review published by Johnson and colleagues<sup>1</sup> on falls after major orthopaedic surgery. The authors should be congratulated for demonstrating that the frequency of falls in orthopaedic patients benefiting from continuous 'lumbar plexus blocks' was similar to the frequency of falls observed in surgical patients. This conclusion was based on an analysis of more than 4000 patients. Their data confirm our previously published findings from a data analysis of falls from our own hospital.<sup>2</sup> Even though the authors were extremely careful in the choices they made, and discussed a number of limitations for their analysis, they failed to acknowledge several selection biases.

It is surprising that the study by Williams and colleagues<sup>3</sup> was included as a randomized trial when the report on falls from this study was published later as a letter to the editor<sup>4</sup> and not reported as an endpoint in the method section of the original paper.<sup>3</sup> This contrasts with the obvious assumption that if a study was published, even if the stated goal was to assess complications, if falls was not a key word, it meant that falls was not included as an endpoint rather than a non-event.<sup>2</sup>

The frequency of falls has been reported to be directly related to a number of pre-, peri-, and postoperative factors including preoperative history of falls,<sup>5</sup> advanced age,<sup>2</sup> and mobilization without supervision.<sup>2</sup> Since none of the studies included in this analysis was controlled for these factors how is it possible to conclude that the difference among the groups was related to the presence or absence of blocks and not due to a difference

in the patient distribution in terms of preoperative history of falls, the number of elderly and very elderly patients, or the number of patients who walked without supervision?<sup>3</sup>

A fall is an established complication of joint replacement. In these conditions, it is surprising that Johnson and colleagues<sup>1</sup> included cohort studies related to blocks in their analysis but did not include studies not involving blocks. This would certainly provide a more balanced evaluation.<sup>4</sup> It is also surprising that the authors did not include the Ackerman and colleagues' study<sup>6</sup> as one of their cohort studies, since the study included 6912 patients with and without blocks.

In conclusion, there is no doubt that a large randomized prospective study focusing on falls would greatly help in determining the role that nerve blocks may play in falls after joint replacement. However, in the current trend including the use of low concentration, low volume of local anaesthetics, it is uncertain that 'lumbar plexus' blocks really affect quadriceps function in patients undergoing total knee replacement, since it is established that the surgery itself reduces quadriceps function by 60%.<sup>7</sup>

## Declaration of interest

None declared.

J. E. Chelly  
Pittsburgh, USA  
E-mail: chelje@anes.upmc.edu

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## Use of the i-gel in unexpected difficult airway

Editor—We support the findings as reported by Theiler and colleagues.<sup>1</sup> In our study, we reported comparable results.

The high fiberoptic score using the i-gel was especially noteworthy. In only 2% of the cases, it was not possible to see the vocal cords.<sup>2</sup> In our hospital, we have a considerable number of difficult airways due to malignancies in the larynx/pharynx. Although awake fiberoptic intubation remains the gold standard in many difficult airway patients, we increasingly use the i-gel for ventilation in patients with an interdental gap of  $\geq 3$  cm. Once the patient's lungs are ventilated, we intubate the trachea using a flexible scope through the i-gel. When lubricated properly, an oral tracheal tube (size 7.0) can be effortlessly pushed along the flexible scope through the i-gel in between the vocal cords. Correct positioning is easily achievable using the scope. If desired, the i-gel can be removed while the tracheal tube is held in the correct place using surgical forceps. This method appears to be so successful that it is increasingly being used in our department in the case of an unexpected difficult airway. The main requirements for this technique are the use of an i-gel 4 (or 5) and an interdental gap of at least 3 cm. By using the i-gel for ventilation before tracheal intubation, one limits the number of manipulations in the larynx, thus reducing the development of swelling, hypersalivation, and potential bleeding.

## Declaration of interest

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M. Sramek\*

Ch. Keijzer

Amsterdam, The Netherlands

\*E-mail: m.sramek@nki.nl

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## Supraglottic airway devices as intubation aids

### Reply from the authors

Editor—We read with interest the letter by Dr Sramek and Dr Keijzer. We are thankful for this valuable contribution. Dr Sramek correctly points out that the i-gel (and many other supraglottic airway devices) provide an excellent back-up device to fiberoptically intubate a patient with a difficult airway. It is especially noteworthy that an awake technique should remain the gold standard in expected difficult airway management. As we have shown in our study cited by Dr Sramek, there is an overlap of risk factors predicting

difficulties with i-gel ventilation and also difficulties with face-mask ventilation.<sup>1</sup> Therefore, unfortunately, when both standard laryngoscopy and face-mask ventilation fail (cannot intubate, cannot ventilate), there is an increased risk for difficulties with supraglottic airway device insertion and ventilation as well.

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L. G. Theiler\*

R. Greif

Bern, Switzerland

\*E-mail: lorenz.theiler@insel.ch

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## In science, all facts, no matter how trivial or banal, enjoy democratic equality

Editor—In the alluring and rapidly expanding field of developmental anaesthetic neurotoxicity, facts have been accumulating at a tremendous rate. The overview of recent work in this area provided by Sanders and colleagues,<sup>1</sup> covering both laboratory and clinical studies, is therefore a welcome addition to the literature.

However, rather than providing a critical and unbiased evaluation of the all-important hypothesis—that a clinically relevant neurotoxic effect of anaesthetics exists—like most other investigators in the field, the authors approach the topic with an implicit assumption that any demonstrable effect must be meaningful. Indeed, already in the introduction, the burden of proof is squarely placed on those questioning the existence of the condition under study when the authors write, 'these accumulating clinical data cannot exclude a clinically important effect... on cognition in later life'. In science, proof of non-existence of anything is notoriously difficult, if not outright impossible. Therefore, one must consider whether current knowledge justifies treating the clinical relevance of anaesthetic neurotoxicity as an established paradigm (and hence the demand to demonstrate its non-existence) as opposed to a hypothesis still awaiting proof.

With respect to rodent data, which constitute the bulk of available information, it is worth pointing out that most experiments were conducted using inbred strains. Inbreeding, by reducing genetic variability, may create both susceptibility to injury and limit compensatory potential, adding to the problems of extrapolating from rodent data to the human condition. Therefore, the authors appropriately highlight the importance of studies in animals other than rodents. On this background, the lenience with which the methodological deficiencies of the piglet studies are downplayed is striking: the lack