



Letter to the Editor

Intra-operative fluoroscopy time and radiation dose during suprapatellar tibial nailing versus infrapatellar tibial nailing


Dear Editor

We read with interest the article by M. Williamson et al. regarding the 'Intra-operative fluoroscopy time and radiation dose during suprapatellar tibial nailing versus infrapatellar tibial nailing' [1]. We extend our appreciation to the authors for this study, but wish to provide our opinion on the study conclusions.

1 The study was a retrospective observational study from a prospectively collected database. The authors did not elaborate on the allocation of the patients or the baseline conditions. If a retrospective study design is used, bias can occur due to periodical changes, recall bias, and differential measurement errors. Confounding [2] by indication also occurs when patients are allocated to intervention or control groups on the basis of patient and investigator preferences, patient characteristics, and clinical history [3]. We recommend randomization to reduce this bias through comparison groups that are similar with regards to known and unknown prognostic variables.

2 One of the main indicators for the study was the Dose Area Product (DAP). There was a significant relationship between body mass index (BMI) and intraoperative DAP [4]. If the two groups have different BMIs, the results are biased. Furthermore, the authors did not specify the fluoroscopy machine used during the operation. Were single or multiple fluoroscopy machines used over the 4-year period? and if so were the parameters consistent? As the radiation dose of the examinee is closely related to machine performance and parameters, the same piece of equipment must be used for the consistency, and regularly tested. Finally, the specific operational processes and methods of X-ray fluoroscopy were not specified in detail. For example, the DAP of the tibia in the different segments/positions differed. In addition, scrap films should be removed from the final datasets.

In summary, the number of confounding factors questions the credibility of the data. Though we wish to thank the authors for a very interesting study, we feel that clarification of these queries would allow more confidence in the final conclusions.

Conflict of interest

None.

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Letter to the Editor

Re: Is prehospital blood transfusion effective and safe in hemorrhagic trauma patients? A systematic review and meta-analysis. Lack of clear, objective blood and plasma transfusion criteria after trauma in the prehospital setting


Sir,

We read with great interest the article by Rijnhout et al. [1] which focused on earlier transfusions in the prehospital setting and the improved survival outcomes for hemorrhagic trauma patients.

We would like to discuss with the author the heterogeneity of the inclusion criteria that reflects the difficulty to detect active

bleeding in solid organs and the kinetics of blood coagulation reactions. Some trauma patients probably receive transfusions when then they are not required, for example, due to collapsed systolic blood pressure in a patient with spinal shock or tachycardia in an acute painful context. [2] Regarding blunt and penetrating trauma, it would be interesting to know the authors' precise number of over-transfused patients.

Conflicts of interest

None of the author of this manuscript have any conflicts of interest.

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Letter to the Editor

Letter to the editor Re: The use of dorsal distraction plating for severely comminuted distal radius fractures: A review and comparison to volar plate fixation by R. Perlusa, J. Doyona and P. Henry



I read the work from Toronto, Canada with interest. Dorsal distraction plating is a technique that routinely forms part of my management of complex intra-articular fractures of the distal radius. Anecdotally I find it to be useful for those injuries where standard volar or dorsal locking plate technology would not permit adequate hold of the fracture fragments. I agree with the authors that although external fixation is a viable technique for these injuries, the complication rate – particularly given the necessity for a fixation period of around three months – is excessively high. Complex regional pain syndrome, finger stiffness and superficial radial nerve irritation are all extremely difficult clinical problems to treat.

The paper gives a non-systematic review of the literature to date, demonstrating favourable outcomes with regards to range of movement and radiological parameters. However I have a few questions concerning the data presented. Table 1 and the manuscript text both suggest that eight studies were included in the review, including two authored by Hanel et al. [1,2] in 2006 and 2010. However the latter of these studies [2] does not appear to have been included in table 2. This omission of the largest series of these injuries makes interpretation of the demographics of this overall cohort difficult. This is particularly true when the complication rate is discussed using data from the 2010 paper. The total number of patients in whom results are being reported should be clearly stated in the manuscript.

In the results section no mention is made of the requirement to remove dorsal distraction plates following fracture union. Presumably the included studies all stated this in their methodology describing surgical technique. This should be clearly stated to avoid confusion for readers who may not be familiar with this technique.

The results section includes radiographic parameters and describes the radial inclination measured in millimetres (Table 4). Radial inclination is usually measured in degrees, with radial height measured in millimetres. Can you authors clarify which parameter is being reported and the unit of measurement?

The discussion is comprehensive but compares differing fracture patterns. Whilst I accept that Hanel et al [2] and Ruch et al [3] both used dorsal distraction plating for AO type A fractures, this seems an extension of the sensible indication of this technique. It is of note that all 3 papers share an author [1–3]. Spanning the wrist joint should not be undertaken lightly. If the fracture pattern permits fixation without spanning, as in a severely comminuted metaphyseal fracture with no intra-articular component, this can usually be safely performed with standard volar or dorsal locking plates. This would usually negate the need for a second surgical procedure and allow earlier range of movement, likely resulting in less wrist stiffness. However I accept this is not strongly supported by published evidence. Personally I do not feel that the AO type A fractures are an indication for this technique and I would be interested in the author's experience of treating these AO type A injuries.

Disclosure

The authors have no conflicts of interest to disclose concerning this study.

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