

reviewing the outcomes of plateau fractures, residual articular incongruities appear to be relatively well tolerated, but only when joint stability is restored. The restoration of the coronal plane alignment and the retention and repair of the meniscus if necessary are more critical in determining outcome than articular step-off alone [5]. There is no doubt that the anatomical reduction of the articular surface should be pursued. To achieve this, we have noticed that it is best to follow the main fracture plane through the split component of the fracture. In the majority of the cases, this technical maneuver dramatically facilitates access to the areas of comminution. If the joint depression is of the rim itself - a rim impaction and depression - this is also a split wedge. The horizontal portion of the depressed rim represents the length along the rim of the split wedge, and its point of exit (x) is where the normal cortex is encountered distally. The vertical height of the wedge is given by the extent of the crushed cortex between the height of the rim and the point (x). The three-dimensional extension of the original classification allows accurate spatial localization of this area. This ability will guide the surgeon in planning the final reduction and fixation which will restore joint stability. To enhance the reproducibility of this new three-dimensional classification we tried to keep it as logical and straightforward as possible. The six principle types described by Schatzker continue to be the basis of this new classification and are incredibly relevant in differentiating the low-velocity fracture types (I to III) from the high energy ones (IV to VI). The modifiers A (anterior) and P (posterior) indicate the three-dimensional location of the rim lesion(s) responsible for joint instability. We acknowledge that our new template may not suffice in the rare instance to describe an incredibly complex joint fragmentation, but such an exceptional injury would defy accurate description while using any existing classification. We do not deny that joint comminution may be so extensive as to challenge any attempt at anatomical reconstruction. Failure to restore joint congruity may indeed interfere with joint motion, but more important is the restoration of joint stability because instability inhibits the return of normal joint function from the start. We are grateful to have had this opportunity to clarify important aspects of our manuscript and hope that the additional information will enhance the understanding of our concepts.

Author's conflict of interest disclosure

None of the authors received payments or services, either directly or indirectly (i.e., via his or her institution), from a third party in support of any aspect of this work. None of the authors, or their institution(s), have had any financial relationship, in the thirty-six months prior to submission of this work, with any entity in the biomedical arena that could be perceived to influence or have the potential to influence what is written in this work. Also, no author has had any other relationships, or has engaged in any other activities, that could be perceived to influence or have the potential to influence what is written in this work.

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Mauricio Kfuri^{a,b,*}

^aDepartment of Orthopedics, University of Missouri, Columbia, MO, United States

^bDepartment of Biomechanics, Medicine and Rehabilitation of the Locomotor System, School of Medicine of Ribeirão Preto, University of São Paulo, Ribeirão Preto, Brazil

Joseph Schatzker

Division of Orthopedics, Sunnybrook Health Sciences Center, University of Toronto, Ontario, Canada

* Corresponding author at: 1100 Virginia Ave, Missouri Orthopedic Institute, Columbia, MO, 65212, United States. E-mail address: kfurim@health.missouri.edu (M. Kfuri).

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Letter to the Editor

Enforcing standards for systematic reviews



Re: Comparison of arthroscopic reduction and percutaneous fixation and open reduction and internal fixation for tibial plateau fractures (*Injury* 2018;49:1208–1214).

I note this recent systematic review includes data from 7 RCTs, and details the quality domains of these 7 RCTs such as allocation concealment and blinding, including a Risk of Bias table. Noting the low risk of bias in nearly all domains for all studies (unusual for surgical trials, particularly without the use of placebo) I attempted to look up the included trials. The first paper was a retrospective file review of cases presenting to one institution. The second paper did not appear to be an RCT (no mention in the abstract and unequal distribution of case numbers). The remaining papers are in Chinese only and not accessible to me.

The paper appears to contain fabricated and false data. Without investigating further, it is possible that none of the papers are RCTs, let alone RCTs at low risk of bias. Ioannidis has recently published on “The Mass Production of Redundant, Misleading, and Conflicted Systematic Reviews and Meta-analyses” [1]. To allow such an obviously flawed paper to be published reflects poorly on the safeguards and procedures for peer review at the journal.

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Ian A. Harris

Ingham Institute for Applied Medical Research, South Western Sydney
Clinical School, UNSW Sydney, 1 Campbell St Liverpool, NSW, 2170,
Australia E-mail address: ianharris@unsw.edu.au (I. Harris).

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Letter to the Editor

Delayed CT head in elderly blunt head trauma patients taking antithrombotics



It was with great interest that we read the article by Scantling and colleagues [1]. The question of the interest of delayed computed tomography (CT) of the head (D-CTH) in elderly patients under antithrombotic medications with intracranial haemorrhage visualized at the first CT of the head is a very relevant question from an epidemiological point of view, with the ever-increasing number of this kind of patients who fall, and from a socioeconomic level for determining the health care costs for the patient or the society, as is well-explained by Scantling et al. in their discussion. The French recommendations [2] concerning the monitoring of patients under antithrombotics following a mild head injury strongly encourage a second D-CTH scan to be performed between 12 and 24 h after trauma. So, we wonder if the precocity of D-CTH (before 12 h) in Scantling et al.'s work [1] could not decrease the number of intracranial haemorrhage progressions. Indeed, Versmee et al. [3] showed a cohort of 419 patients with a delayed haemorrhage rate of 2% seen only on a CT scan of the head done at 24 h. In addition, the results of Scantling et al. of that the progression of intracranial haemorrhage does not differ according to the type of antithrombotic seems to be a mistake to us if we compare anticoagulants (e.g., warfarin, dabigatran, rivaroxaban) to antiplatelet therapy (e.g., aspirin, clopidogrel). We built a table (see Table 1) with Scantling et al.'s data and performed a chi-squared test, which elucidated a progression of haemorrhage that is statistically greater in patients treated with anticoagulants than in those treated with antiplatelet therapy ($p=0.047$). This result is not surprising and is along the same lines of a recent work [4]. Chiu et al. showed that mortality due to a fall in anticoagulated patients was three times higher as compared with that in nonanticoagulated patients probably due to fall recurrences (5%). The pillar of the management of haemorrhage under anticoagulant therapy remains reversal of these. As a result, we know that antagonist treatment administration and its early introduction are among the most valuable factors in stopping the progression of initial intracranial haemorrhage [5]. We believe that the antagonist treatment described in the original article [1] is not precise enough and does not allow the reader to know if, for example, patients

Table 1

Elderly patients using antithrombotic medications divided by type of antithrombotic used and whether or not the delayed CT of the head was worsened or stable/improved.

Antithrombotic	Warfarin/dabigatran/ rivaroxaban N = 32	Aspirin/ clopidogrel N = 134	<i>p</i>
Worsened intracranial hemorrhage	N = 16 (50%)	N = 42 (31,3 %)	0,047
Stable/improved intracranial hemorrhage	N = 16 (50%)	N = 92 (68,7 %)	

under warfarin benefited from vitamin K including at which doses nor about the delay to introduce it. In the same way, we have no idea about the control of antagonist treatment efficacy and a possible second administration. We would like to thank the authors for their quality work. Nevertheless, it seems to us that it is premature to rule out D-CTH from the diagnostic strategy based on the arguments presented in Scantling et al.'s study, especially for patients being treated with oral anticoagulants.

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Nicolas Cazes*

Emergency medical service, Bataillon de marins-pompiers de
Marseille, Marseille, France

Aurélien Renard

Emergency department, Hôpital d'Instruction des Armées Sainte
Anne, Toulon, France

* Corresponding author at: Bataillon de marins-pompiers de
Marseille, Groupement Santé, 9 Boulevard de Strasbourg, 13233
MARSEILLE cedex 20, France.

E-mail address: md.ncazes@gmail.com (N. Cazes).

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