Letter to the Editor

Response to; Letter to the Editor Re: Standardised virtual fracture clinic management of radiographically stable Weber B ankle fractures is safe, cost effective and reproducible (April 2017)

Dear Mr Chesser,

Thank you for your response to our article [1].

The study date period was from September 2013- September 2015 [2], which predates the BOAST 12 guidelines [3] which were published in August 2016. To base a traditional vs virtual fracture clinic (VFC) cost comparison on guidelines which did not exist at the time of collection period was therefore not deemed appropriate. We have clarified in our cost analysis that ‘There was no standardised protocol for the management of these injuries prior to the VFC. There was therefore a lot of variation in how these injuries were managed prior to the introduction of our standardised VFC protocol. We have therefore estimated the cost of a traditional model based on the . . . typical scenario seen prior to the VFC’.

In your altered version of our cost analysis, you have assumed standardised care is being delivered in the ED prior to an appointment. However, If the patient is examined ‘out of cast’ in clinic as you suggest in your response, then you have not included the cost of a plaster cast in the ED in addition to the walking boot applied in a fracture clinic. In addition, with regard to the number of radiographs obtained, you have assumed that the ED physicians do not obtain an Xray in a plaster to confirm safe position in plaster before leaving the ED; given that the BOAST guidelines state that ‘Adequate reduction must be confirmed by review of repeat radiographs and documented before transfer from ED’ it would be reasonable for these to be obtained with the associated costs of doing so. Your response therefore, highlights the difficulty of an accurate cost analysis where no standards of care exist.

Our standardised VFC management protocol for this injury continues to evolve at our institution following this research and the publication of the BOAST guidelines and there is now no routine 6 week radiograph or follow up appointment made for patients with this injury; this adapted protocol therefore costs less, when compared to the cost analysis for the VFC protocol which was in use when our data was collected.

With regard to your comment that ‘The report fails to record patients’ feedback or any outcomes, which we would believe is essential when a system is changed”; our primary outcome was fracture union and as we have stated in our discussion, ‘patient satisfaction is now routinely assessed as part of the VFC process’. This is an improvement both from the VFC protocol used as part of the study period and the period that predates the VFC protocol, both of which had no formalised patient feedback specifically for this injury.

We would agree with you that ‘the telling bit of the article was the lack of standardised protocol for a common injury’; there were indeed no formal standards for care at our institution prior to the introduction of standards of care via our VFC in 2013. Perhaps even more telling than there being no standards for care at our institution prior to 2013, is that there had been no national standards of care for such a common injury prior to the publication of the BOAST guidelines 3 years later that we introduced our own.

We wholeheartedly agree that ‘if more effort was put into this, we would be likely to treat our patients more efficiently at less cost to the health care provider’, and this is what we believe we are able to do at our institution with our VFC model.

Our institutions standardised VFC protocol for this injury was BOAST standard compliant 3 years prior to the introduction of these standards. This study therefore adds to evidence available and further supports the use of these BOAST guideline standards. We feel that this study supports the use of a standardised care protocol using a VFC model and that it is cost effective when compared to unstandardised care prior to the BOAST guidelines.

Best wishes and many thanks again for your comments,

References


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Letter to the Editor

A double-plating approach to distal femur fracture: A clinical study; how apt is the technique? How strong is the evidence?

Dear Editor,

We read one of the articles published in October 2017 edition of your journal by Sternberg et al., titled “A double-plating approach to distal femur fracture: a clinical study” and studied all its aspects in detail [1].

We commend the authors for their application of this novel idea and getting the desired results. However we will like to raise a few important points and seek some clarifications for the same.

Firstly, in the methodology section, the authors mention that they included type A3 and C3 fractures according to the A0 classification [2]. But in the results, it is evident that they have included all the three subtypes of A and C type fractures. So we seek some clarification from the authors regarding the same. A1 and C1 fractures are not comminuted. The aim of the study was to evaluate dual plating in unstable distal femur fractures that were comminuted. So this is another point that is unclear.

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Secondly, in the first nonunion case of the 92 year old female with A3 type of fracture, we can see medial defect and comminution. Also the authors recommend meticulous medial dissection for the buttress plate. All this will compromise the soft tissue vascularity and could hamper union in such cases. Also whether or not bone grafting was done, needs to be mentioned.

Thirdly, we find no mention about the type of plate used for medial side. Was it a pre contoured plate or something else? What do the authors recommend? Also the type of screws, they have used proximal to the fracture site requires a mention; e.g., do they plate A1 fractures, that are non comminuted in a complete locking mode or they use compression screws to compress the fracture site and hold the plate close to the bone. Using only the locking screws, could make the construct too stiff and hamper union [3,4].

The authors also talk about poor bone quality in the patients, but there is no objective assessment of the bone marrow density in any patient. Have they assumed that older patients as a rule have osteoporosis?

The final query on our part is regarding the periprosthetic fractures. What was the approach used by the authors in those cases? Total knee arthroplasty is commonly done by a midline approach, so what was the approach they used. In the surgical technique, they mention usage of dual incision universally in all the included cases. Was it the same for the periprosthetic fractures too? Why did not they use the same midline scar for the approach, that was previously used for the arthroplasty?

We will like to convey that we appreciate the work done in this study, but the lack of control group is a serious limitation that is unacknowledged.

Conflicts of interest

The authors declare that there is no conflict of interest.

References


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Dear editor,

We read with great interest the article entitled “Masquelet technique versus Ilizarov bone transport for reconstruction of lower extremity bone defects following posttraumatic osteomyelitis” by Tong et al. [1] and congratulate the authors for their study on segmental bone defects following posttraumatic osteomyelitis.

However, the study raises some concerns that we would like to address. We think that the title is not appropriate for the text, as “Ilizarov treatment” defines circular external fixation, but only 4 of 19 patients were operated with Ilizarov external fixation in the study and monolateral external fixators were used for 15 patients [2].

Also, we think that application of these techniques on two dissimilar bones which differs about vascularity and soft tissue coverage is a limitation for this study.

Finally, authors did not clarify precisely how the decision was made about Masquelet technique or bone transport technique. We wonder what were the criteria used to allocate a patient to perform Masquelet technique or bone transport.

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References


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Letter to the Editor

Masquelet technique versus Ilizarov bone transport for reconstruction of lower extremity bone defects following posttraumatic osteomyelitis

Dear editor,

We would like to commend the authors Sinnathamby et al. for their study assessing the increased detection of blunt
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