



## Letter to the editor regarding Barlow et al: “Locking plate fixation of proximal humerus fractures in patients older than 60 years continues to be associated with a high complication rate”



### To the Editor:

With great interest, we have read the article by Barlow et al<sup>1</sup> entitled “Locking plate fixation of proximal humerus fractures in patients older than 60 years continues to be associated with a high complication rate.” We acclaim Barlow et al for their clear description of the postoperative radiographic outcomes, especially because varus and valgus of the shoulder were addressed extensively. The varus or valgus position of the humeral head has been proved to be one of the most important surgeon-based factors that influence the complication probability of head-sparing reconstructions (open reduction–internal fixation).<sup>3,5</sup> Furthermore, it is very interesting that the average time to failure postoperatively was 16 months. This finding suggests that a longer-than-usual (1-year) follow-up is advisable, which the authors shortly mentioned in the discussion and which we perceive as an important collateral finding that merits mention in the conclusion as well.

Although the study by Barlow et al<sup>1</sup> is robust in patient numbers, a few methodologic questions were raised that we would like to address. As described in the discussion of the article, a retrospective study design was used, in which information bias and selection bias are issues that should make us cautious of drawing too firm conclusions. A retrospective study such as this is obviously prone to information bias. All data were retrieved from the patients’ status reports, which results in risks of missing data and of inconsistent data because reporting was not protocolized or performed by 1 trained assessor.

Regarding selection bias, Barlow et al<sup>1</sup> presented an interesting finding concerning the use of augmenting open reduction–internal fixation of the proximal humerus with an endosteal implant (fibular allograft). There was no significant difference in the failure rate between patients treated with fibular allograft and those treated without it. The fact that both were included without (the mentioning of) a clear indication of when a fracture was typified as

needing a fibular allograft might result in significant selection bias. In the current literature, the indications for the use of fibular allografts are numerous.<sup>2,4,7</sup> However, some studies have suggested using the fibular allograft only for anatomic neck fractures.<sup>6</sup> These fractures in particular are known to be prone to avascular necrosis, and therefore, the complication rate is expected to be higher than in a group comprising all fracture types treated with fibular allografts. This might have had significant implications on the outcome of the group described by Barlow et al. Perhaps, reporting a short overview of which types of fractures received a fibular allograft and which did not would shed sufficient light on this matter, quite possibly reducing the perceived selection bias. In addition, regarding selection bias, the authors used a “principle-based” approach in the treatment of proximal humeral fractures with open reduction–internal fixation. This approach encompasses tension band suturing of the rotator cuff and the consistent use of calcar screws. A constant approach, such as this principle-based approach, has been proved to be important in treating complex proximal humeral fractures to achieve the most stable construct after anatomic reduction is established. However, Barlow et al did not mention how often patients received additional tension band sutures or calcar screws and whether there were differences in outcomes between patients who received these additional tension band sutures or calcar screws and those who did not. Stratified reporting hereof would greatly improve the force with which inferences can be made from the results.

Furthermore, the clinical outcomes are somewhat unclear. Barlow et al<sup>1</sup> described the use of a visual analog scale score for pain and the Single Assessment Numerical Evaluation score as outcome measures. The visual analog scale score was surprising low in patients with a failed osteosynthesis. This finding raises the question of the degree of arm function in these patients. In our opinion, if the function of the arm was acceptable, there would not have

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been an indication for reoperation. The other clinical outcome score Barlow et al used was the Single Assessment Numerical Evaluation score, which is not validated for upper-extremity injuries. In our opinion, it would have been more appropriate to use a validated patient-reported outcome measure such as the Disabilities of the Arm, Shoulder and Hand questionnaire, the Constant score, and/or the Oxford Shoulder Score.

Nevertheless, we would very much like to thank Barlow et al<sup>1</sup> for their valuable contribution to the ongoing discussion regarding the optimal treatment of proximal humeral fractures. We know how much easier it is to criticize a report as opposed to producing it and acknowledge the hard work that was invested in it. Therefore, we would very much appreciate it if the authors would respond to our letter to further clarify the methods used and their results to further increase the impact of their study.

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*Leanne S. Blaas, MD*  
*Department of Trauma Surgery*  
*Zaandam Medical Center*  
*Zaandam, The Netherlands*  
*Department of Trauma Surgery*  
*Amsterdam University Medical Center Location Boelelaan*  
*Amsterdam, The Netherlands*  
*E-mail: [blaasl@zaansmc.nl](mailto:blaasl@zaansmc.nl)*

*Charlotte M. Lameijer, MD, PhD*  
*Department of Trauma Surgery*  
*Amsterdam UMC Location Boelelaan*  
*Amsterdam, The Netherlands*

*Robert Jan Derksen, MD, PhD, MSc*  
*Department of Trauma Surgery*  
*Zaandam Medical Center*  
*Zaandam, The Netherlands*

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