

In defence of the efficacy and safety of braces in osteoporotic vertebral fractures

Barry Ting Sheen Kweh^{1,2}[^], Augusto Gonzalvo^{1,3}, Boyuan Khoo¹, Jin Wee Tee^{2,4,5}

¹Department of Neurosurgery, Austin Hospital, Heidelberg, Victoria, Australia; ²National Trauma Research Institute, Melbourne, Victoria, Australia; ³Department of Surgery, University of Melbourne, Parkville, Victoria, Australia; ⁴Department of Neurosurgery, Alfred Hospital, Melbourne, Victoria, Australia; ⁵Faculty of Medicine, Nursing and Health Sciences, Central Clinical School, Monash University, Melbourne, Victoria, Australia *Correspondence to:* Barry Ting Sheen Kweh, MBBS. Department of Neurosurgery, Austin Hospital, 145 Studley Road, Heidelberg, Victoria 3084, Australia; National Trauma Research Institute, Melbourne, Victoria, Australia. Email: barrykweh88@gmail.com.

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We thank Mikula *et al.* for their well-constructed response to the article proposed by Squires *et al.* which highlights the importance of global cost effectiveness (1). The individual burden on a patient who has sustained an osteoporotic vertebral fracture is immense, as is the overall economic cost to the healthcare system if it is not cautiously and meticulously managed. A semi-rigid or soft brace as proposed by Mikula *et al.* is a reasonable compromise which is biomechanically and functionally effective whilst also being financially viable (1). Our stance remains that a brace is more than viable and should be considered in all cases.

Indeed, Pieroh *et al.* have also recently performed a similar systematic review to Squires *et al.* which constitutes the new German Society for Orthopaedics and Trauma (DGOU) German guidelines on this topic highlighting the importance of considering spinal orthoses as a nonsurgical adjunct in the management of osteoporotic thoracolumbar vertebral fractures (2). It is commendable that the authors have not only performed an updated review of the literature, but have also taken into consideration and critically evaluated previous systematic reviews on the subject including our own review published in *Global Spine Journal* (3). Nonetheless, we would contend that there is a greater role for spinal orthoses than the ambivalent conclusion proposed by Pieroh *et al.* (2).

Firstly, we wholeheartedly agree with the findings that a spinal orthosis results in decreased pain and improved quality of life (4,5). This has been a common finding about cohort studies in the literature, and remains especially true for semi-rigid orthoses (2,6). It is noteworthy that other systematic reviews have also found a similar finding (2). We acknowledge that this may have occurred even without the use of an orthosis, but it is still a significant finding that bracing osteoporotic elderly patients resulted in superior functional outcomes (2,3). Secondly, we reiterate the biomechanical evidence underpinning bracing with several studies confirming there is reduced kyphotic deformity and decreased postural sway which may prevent further falls in the setting of an already pre-existing sustained acute osteoporotic fracture whilst anti-resorptive therapies are still taking effect (3,7). The brace therefore acts to both reduce pain on mobilization and also serves as a reminder to wearers of the importance of being cautious with their movements whilst also making improving balance (3,4).

Finally, we would like to note that Squires *et al.* and Pieroh *et al.* performed excellent reviews of the potential benefits of spinal orthoses but did not collect pooled data on potential adverse effects (2,8). In our own review, we found flexible or semi-rigid orthoses had a lower rate of decubitus ulcers of skin complications than rigid braces (6,9). The

[^] ORCID: 0000-0001-9130-1543.

purported arguments against bracing such as the theory that bracing results in muscle disuse atrophy have also been disproven (10). Thus, there is no definitive evidence in the literature that this simple easily implemented intervention inflicts any significant harm on patients. Indeed, the uncommon but often discussed complications such as skin ulcerations can be detected early by alert clinicians and adjustments implemented to the brace as required.

In light of this, we contend that spinal orthoses should be considered in all patients who sustain an acute osteoporotic vertebral fracture and there is statistically significant evidence to demonstrate improved quality of life and functional outcomes with orthosis use (2,4). Furthermore, in the absence of any evidence to suggest it may cause undue harm to patients, combined with the fact that this low-risk intervention has been confirmed to lead to superior functional outcomes, we would argue orthosis use on a routine basis is reasonable unless there is a specific contraindication (3). Further high-quality studies are still required although we acknowledge the inherent difficulty with conducting these studies given the inability to blind patients to the intervention, as demonstrated in the reviews by Squires et al. and Pieroh et al. (2,8). We congratulate the authors on a seminal manuscript on the subject but would ask that readers consider our stance as well.

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