Neurosurgical Forum

LETTERS TO THE EDITOR

Golf as a contact sport: possible solutions

TO THE EDITOR: I read with interest the article by Walker et al.³ (Walker CT, Uribe JS, Porter RW: Golf: a contact sport. Repetitive traumatic discopathy may be the driver of early lumbar degeneration in modern-era golfers. *J Neurosurg Spine* [epub ahead of print February 5, 2019. DOI: 10.3171/2018.10.SPINE181113]). I am a physician who has been a golf instructor for 8 years. Ten years ago, I started studying the biomechanics of the golf swing with the help of Mr. Jim Flick, one of the greatest instructors of all time. My goal was to simplify the golf swing and also protect the back. I applaud the authors for their attempt to identify the etiology of back problems. My opinion is that back injuries are quite rare when the spine angle, in 3 dimensions, does not change much during the swing. If the vertebrae remain parallel to each other during the swing, there is not nearly as much pressure as when a portion of the disc space is narrowed during rotation through impact. The authors are correct in identifying that the lower portion of the back on the trailing side is the most common area of injury, and this is where the disc spaces are most narrowed during torsion. When viewing videos of golf swings from face on, there is a correlation between golfers who have back problems and the amount of spine angle change from address position to post-impact position. The greater the spine angle change, the higher the likelihood of back problems.²

One should also be careful of offering possible solutions. I have found that overrotation of the trailing hip (not restriction) then requires losing vertical posture, perhaps explaining the findings of the activation of erector spinae muscles mentioned in the article.¹ I am director of golf science at Jake's Academy in Lone Tree, Colorado. In personally teaching more than 500 young students, I have never witnessed a serious back injury, and complaints of low-back pain are almost unheard of. My older students, many of them septuagenarians, are able to increase their practice times without pain. Most of these students could previously only practice a short time before they experienced back pain. Although there is much work to be done, I do feel that developing golf instruction techniques that teach 3D spine angle maintenance is the key to markedly reducing back injuries. When the spine is tilted too far toward the trailing side from the delivery position through

post-impact, the disc spaces of the trailing lumbar spine are narrowed. This is when the torque is the greatest, and I feel that this narrowing plus the highest level of torque during this portion of the swing is when the majority of the damage is occurring. When the address position is maintained instead of increasing the backward tilt of the spine, I feel that most of these injuries will be prevented. I have found that my students hit the ball farther and straighter when this is accomplished. What's good for the spine is also good for the golf swing!

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Response

We thank Dr. Olshock for his thoughtful response and dedication to golf science from the viewpoint of a health-care provider. A physician who is also a golf instructor brings a very unique and valuable perspective to these topics. Our interest in the golf swing and back injuries arose from experiences in our clinics while counseling patients before and after back surgeries. In Arizona, one of the top retirement states for golfers, the most common questions

after back surgery are, “When can I return to golf?” and “Is there anything I can do to prevent another back injury?” As neurosurgeon scientists and golfers, we attempted to take an evidence-based approach to answer these questions. After an extensive search, sadly, we found little or no scientific literature on which to base our recommendations.

Furthermore, when discussing with our patients what other providers have counseled, we have observed that most reply with answers that are based on opinion and experience rather than on scientific fact. When counseled by nongolfing healthcare providers, enthusiasts are frequently told to quit the game. This response is not only devastating to the avid golfer but also not based on any sound scientific or physiological principle.

We believe that the time has come to look deeper into the etiology of back pain and injury among golfers, develop evidence-based guidelines for return to play, and coordinate better care among healthcare providers, golf instructors, and personal trainers. Currently, the chasm between these disciplines is broad. Golf instructors and trainers work with the goal of increasing swing speeds, which may not necessarily align with the healthcare provider’s intention to reduce spinal strain. One of our trusted advisors, Jim McLean, who is widely regarded as one of the best golf instructors of all time, identified the “X factor” as one metric that is observed in some of the golfers who have hit the ball the farthest in PGA (Professional Golfers’ Association) tournaments.² Mr. McLean was the first to observe that John Daly, when he was young and on the PGA tour, reached a separation of the upper and lower torso of 48°. However, this separation is not achieved by all tour players, and many with smaller separations have seen success.¹ The use of this metric as the only way to achieve distance, especially in an older, less flexible golfer, would be a mistake.

Mr. McLean emphasizes, and we agree, that all instruction must be tailored to the individual. Pushing someone to the maximum separation, whether it be during a golf lesson or yoga class, without proper training and assessment could be harmful and may result in an injury. It is essential to look at these observations in the context that these are measurements in young professional athletes, and although there is something significant to learn, a middle-aged amateur should not attempt to emulate these techniques exactly. The description of these techniques serves to make a point of what professionals achieve. These techniques should be attempted only in the context of a golfer’s age, physical fitness, handicap, swing footprint, and flexibility.

We believe that golf instruction should be taught on the basis of age, prior injuries, prior surgeries, handicap, and goals. The technique described by Dr. Olshock,³ in which the discs stay parallel in 3 dimensions throughout the swing, makes perfect sense to us. Decreasing the crunch during the downswing, increasing hip rotation, standing more upright, and other swing changes have been suggested to improve back pain. An analysis in a spine biomechanics laboratory would be the next logical step.

Finally, we advocate that serious golfers seek treatment and counseling from healthcare providers, whether physi-

cal therapists, sports medicine professionals, or surgeons, who appreciate and understand the biomechanics of the golf swing and the passion the golfers have for the game. These partnerships will maximize the chance of returning the golfer pain-free to the golf course.

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Arachnoid webs: preoperative differential diagnosis and surgical exploration

TO THE EDITOR: We read with great interest the article by Nisson et al.⁶ discussing the rare presence of spinal arachnoid webs (SAWs) and performing a systematic review of the literature, including 43 patients (Nisson PL, Hussain I, Härtl R, et al: Arachnoid web of the spine: a systematic literature review. *J Neurosurg Spine* 31:175–184, August 2019). Almost half of the cases presented with symptoms that had been active for more than 1 year before surgery. Arachnoid web excision was performed in 86% of cases. Following surgery, 91% of the patients reported improvement in neurological symptoms. Several aspects warrant further consideration of this uncommon spinal pathology.

Firstly, there is a need for a better preoperative diagnosis. The anamnesis may reveal red flags in context, including trauma, infections, previous surgery, and subarachnoid hemorrhage. Preoperative MRI can be inconclusive. First neuroimaging attempts have focused on the use of direct visualization of arachnoid membranes using 3D constructive interference in steady state (CISS) sequences.⁵ More recently, cardiac-gated phase-contrast (CINE) MRI in multiple axial planes has been suggested, which usually

correctly localizes the SAW and demonstrates a one-way valve like the flow of CSF because of the web.³

Secondly, the clinical and radiological diagnosis should exclude other pathologies that can mimic an SAW, in particular thoracic intradural spinal cord herniation (ISCH). Both are uncommon conditions and can result in significant neurological morbidity if left untreated.⁷ The main aspect is related to the evaluation of the nature of the dorsal cord indentation, as well as the integrity of the ventral subarachnoid space at the level of spinal cord deformity. The “scalpel sign” is usually referred to as pathognomonic for SAW, consisting of dorsal spinal cord indentation and anterior cord displacement. More recently, the “C-shape dorsal indentation” has been further reported for spinal cord herniation.⁷ In practice, differentiating these two separate entities is crucial from a surgical perspective and not always straightforward preoperatively.⁷ Their management is different. The SAW is treated via laminectomy with intradural lysis of adhesions and resection of arachnoid bands, while the ISCH repair involves reduction of the herniation and restoration of the spinal cord to its normal position.

Thirdly, if surgical exploration is performed for an SAW with no major intraoperative findings, one should also consider ISCH and further explore the presence of such a condition.

Fourthly, the “wait-and-scan” strategy versus proactive treatment is, in our experience, quite clear. Conservative treatment with clinical assessment and serial imaging can be offered in asymptomatic cases. Conversely, in symptomatic patients, treatment is always surgical.

Lastly, some authors consider arachnoidolysis as curative.² However, we believe that there is a small risk of re-adhesion, which should be considered and explained to the patient preoperatively. Approaches with arachnoid dissection and decompression have been reported, including by our group.¹ In this context, laminectomy and intradural exploration have become a standard of care, although minimally invasive techniques were also recently described and propose either expandable tubular retractors⁸ or fibroscopic visualization after classic arachnoid opening.⁴

The systematic review by Nisson et al.⁶ nicely fills a gap in knowledge, toward a better understanding of this uncommon entity, which is the arachnoid web.

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Response

We would like to thank Tuleasca et al. for their critical analysis of our paper and positive feedback. Their group raised a number of excellent points that are worth re-emphasizing in the overall management of patients with SAW. First, radiographic findings remain equivocal in many cases, with intraoperative exploration required to make the diagnosis. As mentioned in their letter, we have adopted phase-contrast CINE MRI as part of the preoperative workup in patients with suspected SAW. This sequence is unique in detecting distinct CSF flow pulsations through regions of the web, which can differentiate it from an arachnoid cyst, and is now frequently used for the evaluation of other CSF flow pathologies including Chiari malformation type 1 and assessment of the patency of endoscopic third ventriculostomies. The authors also astutely note that the shape of the spinal cord may be an important predictor of the true pathology, in that SAW typically demonstrates

the scalpel sign, whereas ventral thoracic spinal cord herniations (VTSCHs) cause the spinal cord to assume a “C” shape.

Intraoperative exploration and lysis of webs is the only definitive cure for these patients. Ultrasonography can be a useful adjunct in exact localization following laminectomies and assessment of final spinal cord morphology and position once watertight dural closure is achieved. Tuleasca et al. importantly note that when webs are not encountered intradurally, exploration for dural defects and VTSCH should be undertaken. We advocate—even when SAW is encountered, lysed, and/or resected—careful exploration of the ventral subarachnoid space prior to closure. This may require the release of adjacent dentate ligaments and close attention to neurophysiological monitoring changes during manipulation. We typically have an allogeneic dural graft on standby in the event VTSCH is found and have recently published techniques for repair.¹

Finally, Tuleasca et al. cite recent articles describing minimally invasive surgical strategies for the management of SAW. While further studies are required to examine the benefits and risks of using tubular retractors through a Wiltse plane approach and/or multiportal endoscopic approaches, these techniques provide exciting avenues for further development in an era in which technology is rapidly advancing. In a similar respect, 3D navigation with

software capabilities of fusing intraoperative CT and preoperative MRI has also been adopted by our group for level confirmation and precise localization of thoracic intradural pathologies and may play a role in the future management of SAW.

Once again, we thank Tuleasca et al. for their critical appraisal of our paper and additional pearls based on their own unique experiences with the management of SAW.

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