Neurosurgical Forum
LETTERS TO THE EDITOR**The need for research prioritization in cervical myelopathy**

TO THE EDITOR: We read with great interest the article by Chen et al.¹ (Chen YC, Kuo CH, Cheng CM, Wu JC. Recent advances in the management of cervical spondylotic myelopathy: bibliometric analysis and surgical perspectives. *JNSPG 75th Anniversary Invited Review Article. J Neurosurg Spine*. September 2019;31(3):299–309).

The authors have undertaken a comprehensive bibliometric analysis of research activity in cervical spondylotic myelopathy (CSM), identifying significant and international growth (especially taking place in Asian countries more recently) and a focus on surgical series. This echoes our own findings, published this year in the *Global Spine Journal*, in which 79% of papers studied a surgical cohort, with 45% evaluating a surgical technique.²

Chen et al. conclude that more prospective investigations are needed on the timing and choice of surgery. Whilst we agree that many aspects of surgery remain unanswered (such as its optimal timing, its use in multilevel disease, or recurrent cervical myelopathy³), we wonder whether the current focus on surgical technique may represent a research inefficiency: Are there other areas that could offer more substantial scope for outcome improvement?

Chalmers et al. estimated that as much as 85% of the global \$240 billion US annual research investment is wasted due to problems with study design, publication bias, and research duplication.⁴ These were collectively referred to as research inefficiencies.

Arguably our focus on surgical technique may constitute an inefficiency: to date, the synthesised evidence has not demonstrated consistent or substantial benefit for one surgical approach over another in cervical myelopathy,⁵ yet while this continues to be our focus^{1,2} important aspects of the condition, such as natural history, etiology, diagnosis, and postoperative recovery⁶ remain relatively unexplored and largely unknown.^{7,8}

Chalmers et al. have argued that the establishment of consensus on research uncertainties is the solution. Specifically, through the representative engagement of those living and working with a disease, a list of the most important and unanswered research questions relevant to clinical practice can be formed—research focused here

has a stronger opportunity to deliver practice-changing information.⁴

Clinical progress is undoubtedly the objective for clinical researchers in CSM. However, in a common condition that today causes almost universal disability and produces among the poorest quality of life of any chronic disease, this should be as efficient as possible.⁹

Consequently, with the support of the AOSpine Spinal Cord Injury Knowledge Forum, we have launched the AOSpine RECODE-DCM initiative (Research Elements and Common Data Points for Degenerative Cervical Myelopathy [www.recode-dcm.com]).¹⁰ This is an international, multistakeholder consensus process, including spine surgeons but also other healthcare professionals (such as those from primary care, physiotherapy, pain medicine, neurology, and rehabilitation medicine) and people with cervical myelopathy. It aims to establish, the top 10 future research priorities for future cervical myelopathy research, among other objectives.

We hope that this initiative can support the growing and international appetite to improve outcomes in CSM, by aligning the research expertise and endeavoring to explore the top unanswered research questions. The process is ongoing. If you would like to participate and/or collaborate, please get in touch.

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Disclaimer

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Disclosures

All authors are involved in RECODE-DCM, an international, multistakeholder AOSpine initiative to improve research efficiency in cervical myelopathy.

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Response

We thank Dr. Davies and colleagues for their letter to our CSM article. The exponential growth of research on CSM in the past decade, particularly from Asian countries, can be attributed both to the progress of medical science and to the ethnic propensity of the neurological disorder in the region. Despite the emerging enthusiasm for

the research on this disorder, the natural course of CSM and the surgical approach to, and optimal timing of, any intervention for CSM remain elusive.¹

The management of CSM currently trends toward early intervention and to anterior surgical approaches because of better recovery of neurological function and the possibility of preserving spinal mobility.^{2–4} Cervical disc arthroplasty yields excellent clinical outcomes for patients whose CSM was caused by disc herniations and short segments of spondylosis, as well as for patients without ossification of the posterior longitudinal ligament.^{5,6} Future investigations should focus on the effects of spinal motion in CSM.^{7,8}

Collaborative database analyses are advocated because they can take advantage of standardized evaluations and because they can be used to design a prediction model of the long-term outcomes of CSM, since institutional-based studies inevitably have difficulty with long-term follow-up.^{9,10} Therefore, the principal investigators of RECODE-DCM are to be commended for such an initiative. Projects aiming to organize a multicenter clinical database for CSM to enhance surgical procedures and to improve patients' recovery should be applauded. More outcome parameters, study end points, and CSM's long-term clinical effects will thus be anticipated, and the database will capture long-term outcomes and help build a successful risk-prediction model (e.g., evidence-based selection criteria) that can help in the selection of the appropriate candidates for surgery. Furthermore, such database analyses could contribute to building a predictive model for decision support and communication not only for healthcare providers but for patients as well.

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Incorrect analysis of motor evoked potential efficacy for pedicle subtraction osteotomy

TO THE EDITOR: We read with both interest and concern the article published by Lau et al.¹ (Lau D, Dalle Ore CL, Reid P, et al. Utility of neuromonitoring during lumbar pedicle subtraction osteotomy for adult spinal deformity. *J Neurosurg Spine.* September 2019;31(3):397–407). The study reports on the utility of intraoperative neuromonitoring (IONM) in detecting new motor and sensory deficits in patients surgically treated for adult lumbar spine deformity using pedicle subtraction osteotomies (PSOs). We challenge the authors' conclusions and believe that their data are insufficient to support their claim that motor evoked potentials (MEPs) miss the detection of new iatrogenic neurological injuries. In an effort to provide accurate information to the readers, some of whom are surgeons eager to determine whether MEP IONM is safe and beneficial to their patients, it is our duty, as experts in the field of IONM, to raise awareness to the major flaws of this paper.

Recent studies have shown that multimyotomal transcranial MEPs are highly sensitive for detecting nerve root injury when appropriate techniques are used correctly.^{2–4} False-negative reporting of MEPs is uncommon but it has a history of misclassification, such as mislabeling muscles, masking a monitoring change by jumping myotomes or increasing stimulation, the absence of adequate baselines, or failure to monitor the injured pathway.^{5–7} Lau et al. appear to have misclassified some of their false negatives with the inclusion of 3 of 7 injuries derived from data determined to be unreliable at baseline.¹ When a test is unreliable, it cannot perform the task it is designed for, thus precluding its inclusion as a false negative. Also, previous work from the same institution showed that during L3 PSOs, a

60% decrease from baseline MEPs had an 89% sensitivity when the adductors were monitored and a 100% sensitivity when combined with rectus femoris, vastus medialis, and tibialis anterior muscles for a decline of 1 point or more in motor grade.³ Lau et al. reported MEP monitoring of these muscles in all their cases. However, Fig. 2 indicates that they did not do so, as the adductor muscle is absent in this example of an L3 PSO. This misrepresentation of the muscles monitored raises serious concerns whether the pathway at risk was monitored appropriately and could be responsible for the false negatives that the authors report.

Lau et al. reported that the majority of iatrogenic injuries acquired during adult spine deformity surgery were missed by MEP IONM (30% sensitivity). This contradicts numerous previous studies that have described high sensitivity of MEPs.^{2–4,7} The burden of proof is on these authors to show that these false negatives were valid and monitored appropriately, consistent with previous literature. The authors did not meet this standard. We request that this deficiency be remedied by recalculating performance statistics excluding “unreliable” data and publishing an erratum table that specifies the reliability of the MEP baselines for the 10 injured patients (n = 7 false negative, n = 3 true positive). We implore the *Journal of Neurosurgery: Spine* to require Lau et al. to include in this table relevant details, such as the muscles monitored, baseline, and final amplitudes for each relevant myotome, the site and severity of postoperative motor weakness (or sensory deficit),^{2–4} preexisting weakness,⁸ and the time between the end of monitoring and the first neurological assessment.^{9,10}

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The authors report no conflict of interest.

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Response

We welcome assessment of our study as this promotes thoughtful academic discussion of this topic. However, we disagree with Fournier et al. stating that the data and analysis are not representative and sufficient. Data were provided from the neuromonitoring team. They argue that the false-negative rate (FNR) was misrepresented due to the inclusion of 3 patients with unreliable baseline MEPs. This is contradictory to their publications and cited studies, which include unreliable cases.^{1,2} We make a counter-argument that including such cases represents real-world results and what is clinically encountered. Ideally, all tests work perfectly, but they simply do not. Unreliable MEPs are a pitfall of utilizing IONM in adult spinal deformity (ASD) and should be highlighted. Rather than excluding cases, this is an opportunity to innovate and create methodologies to improve IONM. The current FNR is 70.0% (7/10). If we excluded 3 patients, the FNR is still high at 57.1% (4/7). Other performance measures are not affected.

Most studies of IONM in ASD are heterogeneous (mixed analysis, data, and methodologies), and ASD is not a single entity (i.e., flexible scoliosis cases do not have equivalent neurological risk as revision, rigid, deformities). IONM should be evaluated and protocolized by patient, procedure type, and spinal region (cord vs root). Studies cited by Fournier et al. are limited and cannot be appropriately compared to our study. Lieberman et al. studied 35 patients, 26 undergoing PSO;¹ this study was underpowered and concentrated only on L2–4 PSO while our study included L1–S1 PSO. Their conclusion regarding use of quadriceps muscle monitoring overlooks L1, L5, or S1 nerve root injuries during respective PSO levels. Fournier et al. cited a study of 62,038 IONM cases in which they isolated 109 cases with new deficits and reviewed 22 false-negative cases.² The reported FNR was 0.04% (22/62,038);

however, the FNR was incorrectly calculated. The definition of FNR is total false negatives (i.e., new deficits without IONM changes)/total condition positives (new deficits).³ The correct FNR is 20.2% (22/109); 1 in 5 patients with a new deficit had no IONM changes. Also, this study was heterogeneous (500 centers, various spinal regions and pathology), lacked granularity (inpatient database), and only evaluated 2 ASD cases. Fournier et al. then referenced a heterogeneous study of 113 patients who underwent 3-column osteotomy with a 0% FNR.⁴ Osteotomy was performed in the thoracic (cord) and lumbar spine (nerve root). Spinal cord and nerve root monitoring are very different and cannot be analyzed together.^{5–7} Additionally, the reason for such a low FNR is secondary to unconventional criteria of new weakness (2 strength-grades below the pre-operative examination results).

We understand Fournier et al.’s concerns, but their claims and referenced studies do not strongly support their argument or represent what is clinically observed. We believe that there is significant value to using IONM for lumbar PSO. But in its current state, there seem to be deficiencies that account for low sensitivity and high FNR. The goals and conclusions of our paper were to highlight observed performance measures and stimulate a motion for improvements in IONM.

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