Editorial

Intraoperative neurophysiological monitoring and spinal deformity surgery

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Ferguson et al. present a well-written study analyzing a subset of 47 cases (out of an impressive 519 cases over a 3-year period) involving patients in the pediatric age group who experienced changes in intraoperative neurophysiological monitoring, or intraoperative neurorrhraphy (IONM), while undergoing spinal deformity surgery at Shriners Hospital for Children in Philadelphia. The authors, particularly the senior authors Drs. Steven Hwang and Amer Samdani, represent “the few and the proud” pediatric neurosurgeons who not only have a strong interest in pediatric spinal disorders but have also been able to build a significant clinical practice dedicated to this area. At this point, I must disclose that Dr. Hwang completed his pediatric neurosurgery fellowship with us at Texas Children's Hospital, and I am genuinely proud to comment on his paper as one of his previous mentors and as a present-day colleague. I do hope that this does not disqualify me from providing this editorial as I trust that I may provide lightly biased, but fair and balanced remarks.) Based on their considerable experience, the authors have been able to synthesize important and practical recommendations on how to manage changes in IONM (Fig. 3 in their paper), including the contemporary use of the oft-forgotten yet gold standard Stagnara wake-up test.

Children, like adults, are at risk of neurological deterioration during various neurosurgical and spine procedures, and may benefit from IONM. Yet, some neurophysiological techniques commonly used and established in neurosurgical and spine procedures for adults have not reached widespread use in children.

Intraoperative neurorrhaphy dates back to 1935 when Foerster and Altenberger first used intraoperative electroencephalography; in the late 1930s through the 1950s, Drs. Herbert Jasper and Wilder Penfield further developed this technique, using electrocorticography for localization and surgical treatment of epilepsy. Dawson recorded the first somatosensory evoked potentials (SSEPs) in 1947. Understanding and establishing techniques to record other evoked potentials, including those produced by motor activity and by visual and auditory stimulation, followed. In 1978, the first intraoperative use of brainstem auditory evoked potentials was reported. Despite the long history between IONM and neurosurgery and spine surgery, controversy about indications for and the usefulness and techniques of IONM in children still exists.

For IONM, the advent of new electrophysiological stimulation techniques and the development of more refined anesthetic strategies have improved and optimized recording of reliable neurophysiological signals in the surgical setting, especially in young patients. The frequency of publications devoted to intraoperative neurophysiological techniques has increased significantly over the past few years.

Unfortunately, the present study was not designed to answer the key question: Should IONM be considered standard of care in pediatric neurosurgery or spine surgery? Is IONM really a surgical adjunct that makes neurosurgery or spine surgery safer as an “early warning system” by preventing injury (as the authors surmise)? Or does it simply indicate that neural injury has already occurred like “closing the barn door after the horse has bolted” (paraphrasing a favorite saying of one of my teachers, Dr. Roberto Heros)? Moreover, there are likely patient-, surgeon-, and procedure-related factors that contribute to the decision to use IONM. Other factors, such as availability, quality and reliability of neurophysiological data, as well as cost, time constraints, and medicolegal concerns, may also have a great impact on the frequency of IONM use. However, these factors were not analyzed by Ferguson and colleagues and are beyond the scope of their study.

Intraoperative neurorrhaphy is not a perfect test: 100% sensitivity, 100% specificity, 100% positive predictive value, and 100% negative predictive value. It seems disingenuous and confusing to suggest so. The more realistic statistical measures of IONM are the second set of numbers reported by the authors (which are in line with my own limited anecdotal experience): 100% sensitivity, 100% negative predictive value, 92% specificity, and 8.5% positive predictive value.

Nonetheless, I continue to question even the 100% sensitivity and 100% negative predictive value of IONM as reported by the authors. The authors do not outline how many patients demonstrated “perfect” baseline signals: reliable SSEPs and motor evoked potentials (MEPs) in all 4 extremities. If “perfect” signals were not obtained (for example, SSEPs and MEPs in the upper extremities only and no reliable signals in the lower extremities in a child with a history of myelomeningocele—a fairly common and unsettling scenario—undergoing T-2 to the ilium posterior instrumented fusion for progressive neuromuscular scoliosis), there may have been no change in IONM throughout the case. However, the patient may still wake up with a new neurological deficit, further lowering IONM’s sensitivity and negative predictive value. Certainly, the authors must have experienced this limitation of IONM in their considerable series. What were the...
pre-incision IONM details and clinical outcomes of the remainder of the 519 patients who reportedly did not have changes in monitoring throughout surgery.

It might have been useful and interesting to stratify the changes in IONM as a function of patient age and coexistence of neurological disorders such as cerebral palsy or spinal dysraphism in neuromuscular spinal deformities. We had previously shown significant effects of young age and the presence of Down syndrome on MEPs.\(^5,6\) It would not be a far reach to think that the authors might have seen similar effects on IONM due to their large series.

In conclusion, the authors of this study should be congratulated and commended. Perhaps the greatest importance of this study is that it expands the scale of what is typically considered “pediatric neurosurgery.” With the increasing spine and spinal deformity surgery interest and expertise within the new generation of our subspecialty community, this area will undoubtedly continue to grow.

Response

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We would like to thank Dr. Jea for his insightful editorial on our article. We attempted to summarize and consolidate our experience with IONM in the treatment of pediatric spinal deformity, but given the limitations of our retrospective series there are many unanswered questions remaining. As Dr. Jea aptly points out, the use of IONM has become common if not the standard of care in the US; however, its utility is still being elucidated. Currently, it is most useful as an “early warning” system, but limitations in defining a “real” injury that may be reversible as opposed to impending injury, as well as variations in thresholds adopted to define a significant change, limit our understanding of its significance. Although we do not have the specific number of cases in which the baseline IONM was not “perfect,” we certainly had many with asymmetric or nonideal values. For our manuscript, we defined changes in neuromonitoring relative to the baseline values and noted any new neurological deficits postoperatively. We did not encounter any new deficits in any patients who did not have IONM changes. Although our series of patients is relatively large, limitations in IONM may be relatively small and require larger series such as that reported by Thuet et al.,\(^1\) who reported on 3436 patients and noted only a 0.2% false negative rate of any IONM changes. Some of the variations between our studies may be attributed to differences in the modalities measured as well as in the thresholds used to define significant changes. Nonetheless, we hope that our data help physicians to better understand the use of IONM and to improve the safety of pediatric spine surgery.

Reference


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