

Confirmation of Surgical Decompression to Relieve Migraine Headaches

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This study by Dr. Poggi and colleagues represents an invaluable addition to the literature with respect to the surgical treatment of migraine headaches, specifically because it achieves its self-defined goal of corroborating the results that have been obtained by Dr. Guyuron,¹⁻³ the pioneer of this field, and Dr. Dirnberger,⁴ who is the only other author besides Dr. Guyuron to report reproducible results with migraine surgery in the peer-reviewed literature. This well-written article describing the experience of Dr. Poggi and colleagues does have several points, many self-admitted, that warrant highlighting and further discussion. Specifically, the retrospective nature of the study design sabotages to a moderate degree the conclusions reached, only insofar as a prospective, randomized study design with “sham surgery” would certainly deliver a more powerful and robust result regardless of outcome. Obviously, such a study design would undoubtedly be less likely to gain institutional review board approval—maybe even impossible, depending on the nature and demeanor of the institution—but the suggestion is still valid, nonetheless. Furthermore, this study could benefit from an increased sample size, as only 18 patients were reported. Counterbalancing this, however, is the fact that these were all consecutive patients operated on by the same surgeon with an average 16-month follow-up. This fact should not be overlooked, as it removes operator bias, which definitely would impact the results given the significant learning curve required in learning new operative techniques in this new field. With greater than 12 months’ follow-up in all patients, Poggi et al. waited to publish their results until durable outcomes could be reported. The results, though preliminary, can continue to be tracked but still represent reasonable reportable results in their current form.

A concern arises with respect to the fact that the septal trigger point was not addressed in this study in any of the 18 patients reported. Is this because this trigger point was not suspected in this

cohort, or was it deliberately ignored by the authors? This is an important point, as the success rate of surgery as defined by the authors (a >50 percent improvement in frequency, intensity, and/or duration) might have been even better if this known trigger point had been included in the investigators’ study design, had been addressed with medication(s) during the diagnostic portion of the patient workup, if suspected, and addressed surgically if the diagnosis was supported by intranasal examination, computed tomographic scan, and/or response to medical intervention [i.e., Flonase (Glaxo-SmithKline, Brentford, United Kingdom) or Sudafed (Pfizer, Tadworth, United Kingdom)/Afrin (Schering-Plough, Kenilworth, N.J.)].

The authors chose to perform decompression of multiple trigger points in a staged fashion. This can lead to multiple general anesthetics, multiple recovery periods, incomplete treatment in those with multiple known trigger points, and increased costs (both direct and indirect). It would be interesting to know whether the senior author has now changed his approach, given that the learning curve has been scaled to some degree, and how this may or may not have impacted the results in the next cohort of patients the authors are investigating.

Another question is raised regarding the benefits of surgery relative to the effect seen by the diagnostic administration of botulinum toxin type A (Botox; Allergan, Inc., Irvine, Calif.) before surgery. The authors report an average decrease of migraine headaches from 12 per month to 1.3 per month in the first 30 days, but then rebounding to 3.7 headaches per month subsequent to the acute postoperative period. The authors admit specific outcomes data were not tracked after administration of Botox before surgery, though it would be interesting to see how much more effective the surgery was quantitatively (using the same scales)

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relative to the beneficial effect of the Botox. Furthermore, the authors comment that only 66.7 percent of patients felt that surgery was more effective than Botox, in general. Are there hypotheses to explain why the surgical decompression was not as effective in those patients who benefited more from Botox? One possible explanation was given in an example provided, specifically, in the very first patient treated by the author in this series, where there was incomplete resection of the lateral portion of the corrugator. A recent study confirms that this portion of the corrugator is, in fact, more frequently underresected using the endoscopic technique.⁵ Furthermore, recent anatomical studies indicate that the corrugator size, in general, is underappreciated.⁶ These two studies combined indicate that active effort and specific care and attention must be taken to perform complete corrugator resection, especially in the setting of migraine headache surgical decompression, to achieve the optimal result. It would be interesting to follow up with the authors given this recent addition to the literature and with increased experience with endoscopic and transpalpebral corrugator resection to see whether the success rates improve. It is also possible that additional trigger points, as yet undescribed, exist that may result in a failure rate that could be improved with greater anatomical understanding. This is currently under investigation by several surgeons in a multicenter endeavor, including The University of Texas Southwestern Medical Center, Georgetown University, and Case Western Reserve University.

Another important point to emphasize is the authors' statement that in a majority of patients, the amount of medications taken by the patient was able to be reduced postoperatively. The question remains, though, whether this represented prophylactic medications, symptomatic medications, or both, and also whether this was overseen by a neurologist. Close collaboration with neurologists is absolutely essential when treating these types of patients, a fact that has been well described by Dr. Guyuron and also underscored by Dr. Poggi with respect to preoperative testing. It should be noted, however, that they are just as invaluable postoperatively, as allowing these patients to adjust their dosages and frequencies of migraine headache medications can be fraught with untoward consequences if performed inappropriately.

As would be expected, as corroborative clinical results such as the experience of Poggi et al. are reported and as gradual acceptance of this new theory and its associated techniques grow, many

new questions arise. Investigations into resting muscle tone, histologic and biochemical differences in trigeminal nerve branches relative to non-migraineurs, anatomical variants, and new trigger points are currently ongoing. Translational research in this new field will certainly yield answers to many of these critical questions that will help increase our understanding of this problem that afflicts millions of people worldwide. Perhaps more importantly, discussion of these findings in a multidisciplinary forum, such as the Annual Surgical Treatment of Migraine Headache Symposium in Cleveland, Ohio, has been and continues to be extremely helpful in advancing and redefining the field of migraine surgery and serving as an incubator for exploring and exchanging new ideas.

The remainder of this study's shortcomings are admitted and noted in the Discussion section of this article, and the authors should be applauded for being transparent, forthright, and honest in their own discussion of their findings. The utility and value of this addition to the literature, however, clearly supersedes these shortcomings, as the article corroborates the findings described by Dr. Guyuron and confirms the applicability, reproducibility, and effectiveness of his techniques and their results. What this study does for this field as a whole cannot be overemphasized, and the authors are commended for reporting their results.

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