Neurosurgical Forum
LETTERS TO THE EDITOR

Infantile hydrocephalus treated by choroid plexus coagulation

TO THE EDITOR: We read with keen interest the article by Okano and Ogiwara² (Okano A, Ogiwara H: Long-term follow-up for patients with infantile hydrocephalus treated by choroid plexus coagulation. *J Neurosurg Pediatr* 22:638–645, December 2018). We commend the authors for analyzing this clinically relevant issue. They concluded that choroid plexus coagulation (CPC) with or without endoscopic third ventriculostomy (ETV) can be a safe and effective treatment in children with infantile hydrocephalus and that long-term control of hydrocephalus and normal intellectual development can be achieved. However, there are a few important points that we would like to bring to the attention of the readers of this journal.

The authors observed that the mean ETV success score (ETVSS)¹ of the patients who had undergone CPC alone was 46 ± 12.9 and that of the patients who had undergone combined CPC and ETV was 51.1 ± 7.0 . There was no statistically significant difference between the ETVSSs of those who had undergone CPC only and those who had undergone CPC plus ETV ($p = 0.71$). Of the 5 children treated with CPC alone, only 1 (20%) required a shunt. Of the 9 children treated with CPC and ETV, 5 (55.55%) required a shunt ($p = 0.12$). Of the 5 children treated with CPC alone, only 1 (20%) required a second surgery. Of the children treated with CPC and ETV, since 1 patient underwent repeat ETV, a total of 6 (66.67%) of the 9 children required a second surgery ($p = 0.09$). Although they did not reach statistical significance as defined by the authors ($p < 0.05$), these observed differences tend to convey the message that despite similar preoperative characteristics, CPC alone may have a better outcome than CPC with ETV. We believe that these observations are attributable to significant heterogeneity among these two groups. A comparison of preoperative status on the basis of ETVSS alone should not be done. The authors should compare the demographics of the two groups, which could clarify the skew toward the CPC-alone group. It is relevant to read the authors' comments on this pertinent issue.

The authors divided the patients who underwent CPC with or without ETV into shunt-dependent and shunt-in-

dependent groups. Six of 14 patients underwent ventriculo-peritoneal (VP) shunt placement. Eight patients remained shunt independent, although 1 patient required repeat ETV. The authors included only 7 patients in the shunt-independent category in their description in Table 1 and Fig. 5. We believe that since the authors have categories of patients as shunt dependent or not, all 8 should be included in the shunt-independent category and reanalyzed accordingly.

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Disclosures

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Response

We appreciate very much the interest in our work. As Drs. Sardana and Kedia pointed out, our findings may give readers the impression that CPC alone may have a better outcome than CPC with ETV. At this point, we do not think this is true, and the number of patients in our study was too small and their backgrounds too heterogeneous to draw any conclusions. However, we would like to emphasize that CPC with or without ETV can achieve long-term control of hydrocephalus and normal intellectual development in successful cases. In the future, a prospective randomized clinical trial in a large number of patients with

homogeneous backgrounds will be required to determine which patients will benefit from CPC alone and which will benefit from CPC with ETV.

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Management of pediatric temporosylvian arachnoid cysts

TO THE EDITOR: With great appreciation, we have read the comparative study by Amelot et al.¹ (Amelot A, Beccaria K, Blauwbomme T, et al: Microsurgical, endoscopic, and shunt management of pediatric temporosylvian arachnoid cysts: a comparative study. *J Neurosurg Pediatr* 23:749–757, June 2019). We admire the authors' vast experience demonstrated in this article and gained over a long period of pediatric neurosurgical tradition of treating pediatric temporosylvian arachnoid cysts (TSACs). Without a doubt, this study is unique and at present there is no comparable investigation published with a similar extensive experience gained at a single center. The demonstrated epidemiological data and surgical results will serve as a role model for future investigations on the same subject.

We do agree that the results from this study support the beneficial potential of microsurgical fenestration of TSACs in terms of event-free survival. We also agree that cystoperitoneal shunts should be avoided because arachnoid cysts represent a loculation of trapped CSF in the intracranial compartment, instead of hydrocephalus with disturbed CSF circulation. Thus, establishing communication of a cyst with the regular intracranial CSF-filled cisterns should first be attempted, rather than extracranial CSF diversion.

Nevertheless, we are cautious with the conclusion drawn that endoscopic treatment appears to be less effective than the microsurgical technique based on the presented data. This statement is neither truly proven nor supported by this retrospective single-center study. Although the paper's value is without doubt, still some potential limitations should have been mentioned as well. These limitations include the following points. First, there is a numeric imbalance of patients treated with different surgical techniques. The fact that endoscopically treated patients represent only one-third of the patients treated microsurgically might indicate some bias in surgical decision-making. Furthermore, there are significant differences of relevant characteristics of the endoscopic and microsurgical patient groups, namely patient age and cyst volume, which are known factors with potential influence on outcome. The patients treated by endoscopic means are half the age of the patients treated by microsurgery (8.45 ± 4.7 years vs 4.28 ± 4.7 years; $p <$

0.0001), indicating that the endoscopic group might include a higher portion of more complex cases of infancy. In addition, the TSAC size is significantly larger (Gallassi type III 45.7% vs 19.1%, $p < 0.0001$) in the endoscopic group compared to the microsurgical group. In our opinion, it would be worthwhile to reanalyze the results after adapting for differences in age and cyst size.

A further debatable subject is that ruptured TSACs are recommended to be treated using a subdural shunt. In our experience, the combination of endoscopic lavage of the cystic and subdural space combined with a fenestration towards the basal cisterns with or without an external drain may minimize shunt dependency.³ Some recently published studies demonstrated favorable results for the endoscopic technique. This was unfortunately not discussed or cited in this paper and could have been added for a more balanced view on a persistently debated topic.²

Despite questioning the conclusions on endoscopy presented in this study, the authors are to be congratulated for this large patient cohort and their experience and success in treating these challenging conditions.

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Response

We would like to thank Prof. Thomale and his collaborators for their interest and observations, and for providing an excellent critical letter in response to our article.

Prof. Thomale and colleagues prefer to apply caution towards the conclusion that we drew about the endoscopic treatment appearing to be less effective than the microsurgical technique for TSACs. They point out some biases in our studies that might neither truly prove nor support our conclusions. It is true that some limitations do exist, but we can discuss them.

As we precisely stated in the methods section of our article, the purpose of our publication was to analyze a retrospective cohort of TSACs treated by 3 different methods, based mainly on surgeon personal preference (7 senior surgeons) and patient presentation at admission. We also pointed this out in our discussion: "Similar to the literature, there was definitely a bias in our study due to preferences of certain techniques according to surgeons, i.e., some of them performed a majority of microsurgical techniques whereas others preferred endoscopy." As previously demonstrated and as stated in the conclusion, "the success rate of these procedures may depend on the surgeon's preference and skills; these results deserve validation by a randomized prospective study."

Even though our study was not constructed as a case-control study, the endoscopic group represented a 1:2 ratio to the microsurgery group, denoting the appropriate and minimal relationship in attempting to determine differences in inferiority or equality between 2 groups of consecutive patients. Moreover, as the frequency of onset of event-free survival was 40% at 3 years for endoscopically treated patients (which makes it a common and significant event), there is no statistical indication to homogenize the two groups in sample size. Conversely, Thomale et al. were right concerning the lack of power and the difference being nonstatistically significant between the endoscopy and microsurgery groups; it would be beneficial to increase patient sample sizes in the endoscopy group in order to see significance.

In a second instance, Thomale et al. raise the fact that there are significant differences of relevant characteristics in the endoscopic and microsurgical patient groups, namely patient age and cyst volume. By all means, we agree with the authors and the literature concerning cyst volume, and we determined that Galassi type was significantly associated with the occurrence of event complications: 10/99 (10.1%) Galassi I type, 21/77 (27.3%) Galassi II type, and 20/64 (31.2%) Galassi III type ($p = 0.002$). However, in our series, although we observed a tendency for younger children to develop more complications (5.6 ± 4.9 years vs 7.3 ± 4.7 years), we did not find this to be a significant difference ($p = 0.354$).

We agree with Thomale et al. who evoked the need of an additional investigation for ruptured arachnoid cysts (ACs) when subdural shunt treatment is recommended. We are in step with their practice because in our experience, all ruptured ACs were treated by a subdural shunt. Thomale et al. noted that in their experience, the combination of en-

doscopic lavage of the cystic and subdural space combined with a fenestration towards the basal cisterns, with or without an external drain, may minimize shunt dependency.² As we discussed previously and in our series, the majority of ruptured ACs in children and more rarely in adults were associated with acute bleeding or hygromas. We have a very limited experience of such lavage in our department. Although there is no doubt that a lavage-aspiration could improve outcome, we should consider the necessity of the lavage being total, iterative, and complete, because blood and porphyrin residues are one of the main causes (by direct sealing or arachnoidal inflammatory mechanisms) of closing stoma permeability.¹

Thomale et al. regretted not finding in our paper a few recently published results in the literature demonstrating favourable outcome for the endoscopic technique. Notwithstanding this finding, controversy still exists regarding the optimal option (neuroendoscopic fenestrations, microsurgical fenestrations with or without marsupialisation). In the literature and in the various works published recently, alternatively, the superiority of one or the other technique is found according to the different detractors and their practice.³ What is sure is that at this time at our institution, perhaps through the biases they emphasized, endoscopy has not shown superiority over microsurgery.

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