Misinterpretations in the citation

TO THE EDITOR: I read with interest the article by Xu et al. (Xu X, Chen X, Li F, et al: Effectiveness of endoscopic surgery for supratentorial hypertensive intracerebral hemorrhage: a comparison with craniotomy. *J Neurosurg* [epub ahead of print April 7, 2017. DOI: 10.3171/2016.10.JNS161589]). It is with pleasure that I see my previous work cited by Xu and colleagues. Unfortunately, I regret to inform you that aspects of my work are misinterpreted in their paper.

In the paper by Xu et al., the authors state that there were 20 patients in our study and that these patients had cerebellar hemorrhage. The conclusion of our study is stated as “no improvement in hematoma removal.”

I regret to say that this is not correct. Our study focused on 7 patients who underwent endoscopic evacuation of a posterolateral type of thalamic hemorrhage, which had ruptured into the trigone of the lateral ventricle and caused acute hydrocephalus. In our *Conclusion* we stated the following:

Use of a polypropylene endoscopic sheath in combination with an endoscope and an approach from Keen’s point to the collateral trigone of the lateral ventricle can improve the efficiency of thalamic hematoma evacuation and prevent shunt-dependent hydrocephalus.

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References

Response
We are really sorry that in Table 3 of our article, we misrepresented the article by Chen et al. Professor Chen and his colleagues reported a case series study involving 7 patients who underwent endoscopic evacuation of thalamic hemorrhage that had ruptured into the trigone of the lateral ventricle and caused acute hydrocephalus. The authors’ finding in that paper was that endoscopic evacuation improved thalamic hematoma evacuation and prevented shunt-dependent hydrocephalus.

In response to Dr. Chen’s Letter to the Editor, we have made appropriate corrections to Table 3, as outlined in a corresponding erratum notice. We apologize to Prof. Chen and to the Editor and readers of the *Journal of Neurosurgery* for causing so much trouble and inconvenience. We would like to express our appreciation to Prof. Chen for kindly pointing out our error.

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Computed tomography angiography in brain death determination: is there justification for its use?

TO THE EDITOR: We noted the study by Garrett et al. (Garrett MP, William RW, Bohl MA, et al: Computed tomography angiography as a confirmatory test for the diagnosis of brain death. *J Neurosurg* [epub ahead of print March 17, 2017. DOI: 10.3171/2016.10.JNS161042]). CTA has been a research focus in Europe for several years, with both France and Germany incorporating CTA in their BD guidelines, more for convenience (availability) reasons than driven by incontrovertible scientific facts.
We would like to comment on several aspects of the recently published study by Garrett et al., which was completed nearly 5 years ago.

First, although the authors are arguably correct for using conventional angiography as the gold standard ancillary test for this study, different criteria were used for CTA and cerebral angiography for the determination of BD. The simplified 4-point criteria used by Frampas et al. are insufficient, as they do not evaluate for flow in the intracranial internal carotid arteries or in the posterior circulation whatsoever. Conventional angiography evaluates for arrest of flow at the point of entry of all arteries intradurally, both in the anterior and posterior circulations. Why would CTA not be held to the same standard? Evaluation of the posterior circulation is of particular importance in this assessment.

Second, their cerebral angiography criteria for no flow beyond the petrous segment of the carotid artery are debatable because others may claim that cavernous segment filling is allowed, as both are still extradural. If anything, it illustrates the difficulty in defining lack of intracranial flow.

Third, all of the patients in the study underwent clinical determination of BD, but some could not undergo the complete clinical evaluation due to an inability to complete apnea testing or the presence of other confounders. The authors begin with 22 patients in the study, but, in actuality, only 18 patients had a complete “formal diagnostic workup for BD.” Of these 18 patients, only 16 actually met the clinical criteria for BD. Thus, it appears that only 16 patients actually met the criteria for enrollment in the study. However, it further turns out that, of these 16 patients, 9 could not undergo complete clinical testing. Thus, in truth, only 7 patients were actually clinically brain dead and underwent subsequent conventional angiography and CTA. The clinical examination cannot be said to be one of the gold standards for this study based on only 7 patients fulfilling the criteria. (Furthermore, 1 patient who underwent CTA experienced cardiac arrest prior to undergoing conventional angiography, and we do not know the details of that case, which may further reduce the study’s numbers.) Moreover, how do the clinicians interpret their finding that 3 of 9 cerebral angiogram studies with no flow actually did show flow on CTA? Is this not indicative of tremendous variability in technique?

Putting this aside, the authors proceed to put forth findings that they had 100% specificity and 75% sensitivity for BD using CTA, but they do not present confidence intervals or p values. This is because the findings are not statistically significant, and the methods are not sound. The findings would be even more concerning if the total number of appropriate patients included in the study was only 7, as illustrated above.

It is important to keep in mind important differences between conventional angiography and CTA. Conventional angiography for BD utilizes a power injection, allowing for good visualization of any intracranial arterial flow under pressure. Conversely, CTA is a venous injection, not under pressure, and thus the timing of the contrast bolus to the brain, especially under circumstances of elevated intracranial pressure, might be quite delayed. Conventional angiography is dynamic imaging; CTA is relatively static, and, if the contrast bolus is not timed correctly, intracranial filling could be missed. This especially could be the case in patients with depressed cardiac output, which is quite often the case with brain-dead patients. We do note that the authors built in a 60-second delay after contrast injection before scanning, which seems reasonable, but, to our knowledge, this is not a validated or universally accepted standard. More broadly, even the imaging reference standard of catheter angiography is subject to variability, as the extent of skull base and intracranial arterial opacification can depend much on the catheter tip position relative to the carotid bifurcation and vertebral artery origins, presence or absence of carotid or vertebral artery stenotic disease, and contrast material injection rates and volumes.

Finally, the single neuroradiologist was not blinded to the clinical data or the findings of cerebral angiography, introducing a significant bias. A more rigorous study should include blinded evaluations of all imaging studies and multiple reviewers with appropriate clinical expertise.

As the authors note, false positives with CTA have been reported, and, based on the findings from these authors, we should not have an increased sense of security in the value of CTA in BD. The possibility of false positives remains quite real, and, without adequate study, CTA should not be utilized in the evaluation of BD as an ancillary test. We would be very concerned if this paper were to lead to incorporating CTA in BD determination in US practice, despite the authors’ repeated claims that their decisions are based on a clinical examination. In fact, they are not based on clinical examination, since the authors used cerebral angiography or CTA in patients with confounders (“10 positive urine toxicology”). The broader point is that, for neurosurgeons, this paper is not a justifying reason to use CTA, and there is no “compelling argument” to include CTA in future national guideline revisions.

References


Disclosures

The authors report no conflict of interest.