Mannitol or saline

To The Editor: I read with interest the article by Mortazavi et al.2 (Mortazavi MM, Romeo AK, Deep A, et al: Hypertonic saline for treating raised intracranial pressure: literature review with meta-analysis. A review. J Neurosurg 116:210–221, January 2012). The manuscript will serve as a benchmark at this point in time for the limited data available regarding hypertonic saline versus mannitol use in lowering elevated intracranial pressure (ICP).

However, it is unfortunate that the recent randomized trial published in the Journal of Neurosurgery by Sakellaridis et al.3 was left out of this meta-analysis, and therefore an important Journal of Neurosurgery publication was unavailable in this benchmark review of the literature. The submission date (December 23, 2010) for the article by Mortazavi et al.2 is 6 weeks prior to the publication date for the article by Sakellaridis et al.3 It would have seemed prudent that: 1) the Section Editor for the Journal of Neurosurgery would have noticed the overlap of important articles, one in press and one submitted for publication, and perhaps the importance of inclusion would have been recognized; or 2) Mortazavi et al.2 would have noticed the Sakellaridis et al.3 publication soon after their submission and alerted the Editor of the Journal of Neurosurgery of the need to add the published randomized trial to their review of the literature, especially since the Sakellaridis et al.2 paper certainly would have had its place in our review. In our review, we carefully and critically judged the available literature and questioned the methodology and the rationale for the methodology applied in each study. In addition to the comments in its own Letter to the Editor, by Dr. Defillo,1 we would offer the following additional comments regarding the Sakellaridis et al. paper.2 A strength of the paper is the comparison of osmotic burden, which appears to be a logical assumption. However, there are several shortcomings: In order to be able to compare 2 treatments, these must be as similar as possible. In the study by Sakellaridis et al., mannitol was infused over 20 minutes while 15% hypertonic saline was given as a bolus without any data regarding what time period the bolus was given. The question remains: Is the antihypertensive effect of mannitol or hypertonic saline immediate and via its own inert characteristics or a secondary effect of serum sodium and/or serum osmolarity change? If the latter is assumed to be the case, then a total ICP change over time would be a more objective marker of the result than periodic checks. In the Sakellaridis et al.2 paper, only periodic checks of ICP were performed. Furthermore, ICP spikes received alternating treatments. Although at first glance this may appear to be a strength of the methodology, it actually may be a weakness as the change in serum sodium and osmolarity appears over a longer time than half an hour. Lastly, in regard to our meta-analysis, only papers that had reported failure rates were included. Among the randomized clinical trials reviewed, only 8 had reported failure rates. The Sakellaridis et al.2 paper did not report any failure rates and therefore did not satisfy the inclusion criteria for our meta-analysis. However, if Sakellaridis et al. would provide the total number of episodes treated with either mannitol or hypertonic saline and the number of episodes that each of them failed to lower the ICP to a normal level, then the data presented could be included in the meta-analysis. However, judging from the fact that Sakellaridis et al. did not find any difference in their study, it is reasonable to...

The omission of the Sakellaridis et al.3 randomized trial results (both mannitol and hypertonic saline will lower elevated ICP, sometimes one will achieve results when the other does not, but neither is superior to the other) from the “meta-analysis and review of the literature” is poignant, in that the randomized trial results reflect more the experience of this neurosurgeon than the recommendations made by the “meta-analysis” of current literature before the randomized trial.

It is important that printed peer-review journals maintain an “up-to-date” status as much as possible. This is especially relevant in the age of speed acquired Internet journals, but also for the sake of intellectual veracity by authors, reviewers, commenters, and editors for the flagship Journal of Neurosurgery.

Disclosure

The author reports no conflict of interest.

References


RESPONSE: We thank Dr. Atkinson for his comments. In regard to our review with meta-analysis, as also mentioned by Dr. Atkinson, the paper by Sakellaridis et al.2 was published after our paper was accepted. The Sakellaridis et al.2 paper certainly would have had its place in our review. In our review, we carefully and critically judged the available literature and questioned the methodology and the rationale for the methodology applied in each study. In addition to the comments in its own Letter to the Editor, by Dr. Defillo,1 we would offer the following additional comments regarding the Sakellaridis et al. paper.2 A strength of the paper is the comparison of osmotic burden, which appears to be a logical assumption. However, there are several shortcomings: In order to be able to compare 2 treatments, these must be as similar as possible. In the study by Sakellaridis et al., mannitol was infused over 20 minutes while 15% hypertonic saline was given as a bolus without any data regarding what time period the bolus was given. The question remains: Is the antihypertensive effect of mannitol or hypertonic saline immediate and via its own inert characteristics or a secondary effect of serum sodium and/or serum osmolarity change? If the latter is assumed to be the case, then a total ICP change over time would be a more objective marker of the result than periodic checks. In the Sakellaridis et al.2 paper, only periodic checks of ICP were performed. Furthermore, ICP spikes received alternating treatments. Although at first glance this may appear to be a strength of the methodology, it actually may be a weakness as the change in serum sodium and osmolarity appears over a longer time than half an hour. Lastly, in regard to our meta-analysis, only papers that had reported failure rates were included. Among the randomized clinical trials reviewed, only 8 had reported failure rates. The Sakellaridis et al.2 paper did not report any failure rates and therefore did not satisfy the inclusion criteria for our meta-analysis. However, if Sakellaridis et al. would provide the total number of episodes treated with either mannitol or hypertonic saline and the number of episodes that each of them failed to lower the ICP to a normal level, then the data presented could be included in the meta-analysis. However, judging from the fact that Sakellaridis et al. did not find any difference in their study, it is reasonable to...
assume that the results of our meta-analysis would not change even if these authors reported their failure rates.

References


Neoplastic meningitis


Neoplastic meningitis (NM) is a common neurological comorbidity of metastatic cancers.4 Pathological investigations have often found the infiltration of cancer cells within the leptomeninges, and contrast-enhancing areas have been shown on T1-weighted MRI studies. There is an urgent need to treat the NM to improve the patient’s outcome and survival. Lin et al.3 conducted a retrospective study to investigate the combined treatment strategy for patients with simultaneous hydrocephalus and NM that applied a commercially available reservoir—on/off valve—ventriculoperitoneal shunt (RO-VPS) for drainage of CSF and delivery of intrathecal chemotherapeutic agents. They found that the combined RO-VPS system is secure and useful to implant in the head, contributed to symptomatic relief of hydrocephalus, and provided effective delivery of intrathecal chemotherapy in patients with NM and hydrocephalus.

The combination of systemic and intrathecal chemotherapy would be needed in patients with NM after VPS placement or when there is a concern about systemic disseminations. Because in patients with CSF diversion the fluid is directed into the abdominal cavity, the possibility for dissemination of cancer cells into the peritoneal cavity cannot be ruled out. Therefore, the required dosage of chemotherapeutic agents for systemic control of cancer was significantly higher than for intrathecal treatment alone, because of the theoretical difference in pharmacodynamics and pharmacokinetics between intrathecal and systemic environments. On the other hand, NM is not always simultaneously present with hydrocephalus. There is one important point that should be emphasized: that in patients with NM only and without hydrocephalus, the adequate therapeutic choices could be Ommaya reservoir implantation alone5 to allow access for intrathecal or intraventricular chemotherapy without shunting to the peritoneum. Moreover, close follow-up of clinical symptoms and imaging studies in patients with NM is warranted because systemic and intracranial relapse often occurs.2

Disclosure

The authors report no conflict of interest.

References


Response: We appreciate Dr. Hueng and Mr. An’s interest and comments regarding our study. We agree that NM is a severe complication of cancer and requires urgent treatment and close follow-up. We also agree that the standard management for patients with NM and no hydrocephalus is the placement of an Ommaya reservoir, with subsequent intrathecal chemotherapy and systemic treatment. This was the protocol followed in our institution. However, some patients with NM who do not have hydrocephalus initially could develop increased intracranial pressure (ICP) at a later time, as was observed in 2 patients in our study.

Although data on the likelihood of peritoneal dissemination of cancer through CSF shunting procedures are limited, it is probably a very rare event. We did not observe such a development in any of our patients during the follow-up period. Neither did investigators in another series from the Memorial Sloan-Kettering Cancer Center, in which 37 patients with leptomeningeal carcinomatosis received VPSs.1 We agree that there is a risk of developing peritoneal carcinomatosis for patients with NM after VPS insertion, and that these patients must be monitored.
closely. However, we believe that the prolonged survival afforded by placing RO-VPS constructs and subsequent intrathecal chemotherapy confirms that the benefit of treating elevated ICP outweighs the risk in patients with NM who have hydrocephalus.

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Magnetic resonance imaging and clips

To The Editor: I was very pleased to find on the Journal of Neurosurgery list of new articles, “Magnetic resonance imaging and aneurysm clips” by Dr. McFadden (McFadden JT: Magnetic resonance imaging and aneurysm clips. A review. J Neurosurg 117:1–11, July 2012) that poor record keeping should not prevent a patient with his recommendation that “in all cases the operative procedure rather than some special properties of this clip in MRI.

I have nothing but respect and admiration for neurosurgery and neurosurgeons after many years of collaboration and, more recently, as a grateful patient. In a personal way I also owe a debt of gratitude to Dr. McFadden for his contributions to the design of the modern aneurysm clip. I just wondered as I read his paper if Dr. McFadden offered these heartfelt opinions while losing sight of the fact that the Journal of Neurosurgery is not read exclusively by neurosurgeons and that the AANS includes radiologists among its members. Patients without question have benefitted from the close collaboration between neurosurgery and radiology, and this sort of writing does nothing to foster that relationship, one that is under quite enough pressure from the distances effect of the widely used radiology picture and archiving communication systems (PACS).

It seems as though Dr. McFadden has set up a “paper tiger” in his paper with his very reasonable criticism of the old recommendation to avoid MRI in any patient unless the individual clip was tested for its ferromagnetic properties in the operating room. That unrealistic as well as unnecessary standard of care was not taken seriously by the radiology community and was never implemented at any of the hospitals where I have worked. I had hoped that Dr. McFadden would offer a new approach for screening patients with aneurysm clips in light of the widespread use of MRI-compatible clips over the past decades. He appears to support the traditional approach with his recommendation that “in all cases the operative record should be perused, and unless the implanted clip is identified—eponym, material, and manufacturer (for example, Yaşargil clip, titanium alloy, and Aesculap)—the examination should be refused.” This is the current standard at many MRI centers, but I think that Dr. McFadden, with his extensive knowledge of materials, would agree that poor record keeping should not prevent a patient from undergoing MRI after a recent aneurysm clipping performed at an “all titanium clips, all the time” hospital. As I have suggested in the past, the entire process of MRI would be greatly facilitated if patients were given a card with the details of their clip on the occasion of their discharge from the hospital.

To the credit of Dr. McFadden, Dr. Spetzler, and many others, the issue of MRI compatibility of aneurysm clips has largely been resolved with changes in clip fabrication. The question now, in my view, is under what circumstances could a patient who had surgery in the past decade in the US have an aneurysm clip placed that was not MRI compatible? The only scenario I can imagine, and an improbable one at that, is one in which a neurosurgeon might find an old stainless steel clip among modern ones on a tray and decide it was ideal for some unusual aneurysm. This clip must have been passed over countless times in similar circumstances over 2 decades. If that is indeed the only scenario of concern, then is it not reasonable to offer as a solution the clearing of these
clips from all operating rooms, if there are indeed any left to be found, before some predetermined date in the future? January 13, 2013, has a nice ring to it. Assuming no change in the current manufacturing standards for permanent aneurysm clips, MRI sites all across the country could then reliably assume that any patient subject to clipping after that date at any hospital in the US would be safe to examine with MRI. While of no benefit to patients who underwent clipping in the past, and for whom the standard advice still applies, the efficiency offered by that change when measured in the recovered time otherwise spent calling, record seeking, and discussing with neurosurgeons trying to get them to do as Dr. McFadden suggests when the clip is in doubt (primum non nocere) would be welcomed by patients and physicians alike.

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Disclosure
The author reports no conflict of interest.

Reference

RESPONSE: In response to Dr. Mamourian’s comments, it is cogent to thank him first and above all for not finding factual errors during his perusal of the publication. But his criticisms deserve reply. Dr. Mamourian objects to the language and calls it “spicy reading.” True, the style may be direct, but the language relating to incidents around the one fatal accident is in reality toned down.

Dr. Mamourian’s reference to distracting criticisms is a bit puzzling because the article was written in response to the need for corrections and is driven by nothing so much as criticism. But the criticism spares no one—neurosurgeons, radiologists, and others—guilty of publishing flawed information and addresses no one else, its purpose being to rebuff the continued stream of inaccuracies stimulated nearly 20 years ago by the one recorded disaster.

The article did not offer “heartfelt opinions while losing sight of the fact that the Journal of Neurosurgery is not read exclusively by neurosurgeons and that the AANS includes radiologists among its members.” Quite the opposite, the contents are not heartfelt opinions; an extensive bibliography supports information presented in the paper. Never was I unaware of my audience. Anyone publishing anything current has to be familiar with PubMed and other tools of the Internet as well as online journal publications with a vast audience including neurosurgeons, all medical specialties, metallurgists, physicists, other scientists, a growing population of patients along with interested relatives, and an awakening public. I knew the article would come to the attention of real experts in various related fields, and for this reason and in the name of correct and reliable information, the advice and guidance of world-class metallurgists, materials scientists, and physicists were sought and used.

The critic has tried to sort out patients harboring ferromagnetic metal. However, radiology is doing no more than its duty in properly sorting these patients, and to do otherwise would be negligent. Obviously, none of the endeavors were effectively at work on the day of the fatal incident. And where was “the close collaboration between neurosurgery and radiology”? The regrettable answer is nowhere! It did not exist.

To reiterate the basics: 1) Neurosurgery adapted nonferromagnetic metals to the fabrication of aneurysm clips years before the advent of clinical MR scanners. 2) The only offender is stainless steel, and if it is the implanted metal or if there is doubt, do not expose the patient to the magnetic field of an MR scanner. 3) Do not rely on patent recipes to safeguard the patient, such as a card-carrying patient from an all-titanium hospital and other schemes; operative records, skull radiographs, and communication with neurosurgery personnel provide invaluable and far more reliable information.

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