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Specialization's absurd limit.

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LETTERS

Usefulness of Brain Blood-Flow Imaging

I have read with interest the article by Eckard, Purdy, and Bonte entitled "Temporary Balloon Occlusion of the Carotid Artery Combined with Brain Blood Flow Imaging as a Test to Predict Tolerance Prior to Permanent Carotid Sacrifice" (1). I agree with their conclusion that balloon occlusion is a useful screening procedure. However, their results do not justify the other part of their conclusion: "The addition of brain blood-flow imaging using Tc-HMPAO can further improve the safety margin of internal carotid artery occlusion procedures."

Of their 11 patients whose carotid arteries were ligated, one had a stroke, predicted by the balloon occlusion, which was not tolerated. The flow study was also abnormal. (In the face of an abnormal balloon occlusion, a flow study is an unnecessary expense.) Two patients tolerated the balloon occlusion but had an abnormal flow study. After carotid ligation, one patient did well, the other developed hemiparesis. The predictive value of the flow study was therefore no better than the toss of a coin. Eight patients tolerated the balloon occlusion and had an essentially normal flow study. Seven did well after carotid ligation, but one developed hemiparesis. The normal flow study in this last patient did not predict the poor result and was therefore of no value.

At our hospital, we have also been doing balloon occlusion followed by Tc-HMPAO flow studies in patients whose carotid arteries may have to be sacrificed. In most cases the artery has been preserved, and the number of patients in whom the artery has been ligated is too small to be meaningful. I have serious doubts about the usefulness of the flow study in these patients. It is unfortunate that the authors advocate the use of an expensive examination on the basis of their inconclusive results.

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References

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Reply

We would like to thank Dr. Cornell for his comments. We presented 11 patients who had undergone carotid artery ligation. Before ligation, each patient was tested for tolerance to permanent occlusion with evaluation by temporary balloon occlusion and by brain blood-flow imaging using Tc-HMPAO.

One patient tolerated balloon occlusion for less than 5 minutes, yet still received a Tc-HMPAO blood-flow study. Dr. Cornell questioned the logic of giving this patient the Tc-HMPAO. The Tc-HMPAO was given in this patient before he developed neurologic symptoms from the occlusion. As we stated in our article, we do not maintain technetium generators in our department, and it takes about 45 minutes to get fresh technetium. Thus, the Tc-HMPAO is requested as the study gets underway so that it will be ready at the time of temporary balloon occlusion. Because the Tc-HMPAO was ready at the start of the occlusion procedure in this patient, it was given early during the occlusion, even before he developed neurologic symptoms. Because the Tc-HMPAO had been given, we went ahead and obtained the images.

Dr. Cornell points out that the predictive value of an abnormal blood-flow study in our series was not 100%, nor was a normal blood-flow study 100% predictive of a good result. However, the likelihood of a stroke after occlusion was greater with an abnormal blood-flow study (50%, as opposed to 12.5% with a normal blood flow study). The patient who had a stroke after occlusion with a normal blood-flow study was complicated. Although her stroke may have been related to internal carotid artery occlusion with reduced flow, it is also possible that her symptoms developed from vasospasm after clipping of a middle cerebral artery aneurysm and was not related to the internal carotid artery occlusion.

There have been several other recent papers in the neuroradiologic literature addressing the same problem (1–5). If we include the data from our paper, along with the data from these other papers, there were 30 patients who had normal blood-flow studies with temporary occlusion who subsequently underwent carotid sacrifice. Two of these patients developed a postocclusion infarct (6.67%). There were a total of 8 patients who had abnormal blood-flow studies and subsequently underwent permanent occlusion without the benefit of extracranial-to-intracranial bypass. Six of these patients subsequently developed signs of ischemia (75%); in 1 patient symptoms resolved with extracranial-to-intracranial bypass, 1 patient recovered completely after several days, 1 patient's infarct may have been related to surgical trauma, and 3 patients had persistent neurologic deficits. The number of patients with an abnormal blood-flow study who have subsequently undergone carotid occlusion is small. In most series, patients with clinical tolerance to carotid occlusion and an abnormal blood-flow study undergo extracranial-to-intracranial bypass.

Though we agree with Dr. Cornell that the expense of the HMPAO study is unfortunate, and we agree that the data we presented are preliminary, insofar as they did not permit statistical evaluation, we continue to believe that blood flow assessment is a useful adjunct in the balloon occlusion test. Further studies are warranted to determine the most appropriate technology.

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5. Añon VV, Aynard A, Gobin YP, et al. Balloon occlusion of the internal carotid artery in 40 cases of giant intracavernous aneurysm: technical aspects, cerebral monitoring, and results. *Neuroradiology* 1992;34:245–251

Editor's note: Dr. Cornell's letter was also referred to Dr. Allan Fox for his comments. They follow.

Reply

In reading Dr. Cornell's letter, and reading again the article by Eckard, Purdy, and Bonte, I agree with Dr. Cornell that the conclusion drawn recommending blood-flow imaging is stronger than can be supported from the data in the published paper. The data set is small, and the three strokes that occurred are difficult to relate specifically to the use of the single-photon emission CT study.

One of the eight patients who did well during occlusion studies developed a stroke not predicted by the normal blood-flow study. It is uncertain whether any of the other seven patients suffered an episode of hypotension associated with aspiration, or hypotension due to some other cause, that could in any way be compared with the patient who had a stroke 17 days after the carotid occlusion, where the blood-flow study had shown a small area of abnormality in a watershed zone. Although the development of an infarction in the same region that had shown abnormal blood flow during an occlusion test suggests utility of the blood-flow study, it is uncertain how many patients in the normal blood-flow study category may have had a similar stroke outcome had they undergone the stress of such an unusual physiologic situation.

This interesting short case series stimulates some questions it does not answer. Because all three patients who underwent surgical carotid occlusion had strokes, is balloon occlusion, which presumably would only be done after an uneventful clinical occlusion test evaluation, safer in terms of preventing stroke? Would blood-flow augmentation, such as performing extracranial-to-intracranial bypass before occlusion in the face of abnormality on the flow study, be sufficient to protect patients from the effects of serious hypotensive episodes? The utility of physiologic blood flow studies in the setting of planned carotid occlusions is still not known.

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Oversight by Authors?

I and others have noted with interest the article written by Powell et al in the November issue, "MR Imaging in Acute Multiple Sclerosis: Ring-Like Appearance in Plaques Suggesting the Presence of Paramagnetic Free Radicals" (1). Their proposal as to the nature of the rings in acute multiple sclerosis plaques is very similar, if not identical, to that described in our article in the August 1991 issue of *Radiology*, "Multiple Sclerosis: Histopathologic and MR and/or CT Correlation in 37 Cases at Biopsy and Three Cases at Autopsy" (2). This proposal was also better substantiated in our study, as direct histopathologic correlation was available in our study group.

Similar findings have been previously described in cerebral abscesses, as in the report by Haimes et al in which the macrophage free radical theory was first described, and in *Multiple Sclerosis* by Drayer. Both of these references are quoted in our article.

I think that this was an oversight on the part of Powell et al and the editorial review panel at *AJNR*. A thorough literature review by the authors should have made them aware that we indeed had described this proposal earlier, as it pertains to multiple sclerosis. In addition, as mentioned above, Burton Drayer was the first to describe the T1 hyperintense ring in multiple sclerosis.

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presence of paramagnetic free radicals. *AJNR: Am J Neuroradiol* 1992;13:1544–1546
2. Nesbit GM, Forbes GS, Scheithaver BW, Okazaki H, Rodriguez M. Multiple sclerosis: histopathologic and/or CT correlation in 37 cases at biopsy and three cases at autopsy. *Radiology* 1991;180:467–474

Reply

We regret our omission of reference to the paper published by Nesbit et al referring to MR and CT appearances in multiple sclerosis with histological correlation. Our apparent oversight relates to the timing of our literature search and the inevitable interval that lies between initial submission and final publication of papers. We acknowledge their prior reference to free radicals as the possible cause of the appearances described in their paper, but note that none of their illustrations shows the appearance to which we have drawn attention. While Figure 2 in their series shows a faint curvilinear low signal region on the long echo time image, Figure 4 shows high signal features, and the accompanying legend refers to ring-type lesions showing hyperintensity on both short and long repetition time images. The illustrations suggest an irregular and much thicker ring than appears in our paper. We would have been inclined to attribute this appearance to lipids resulting from myelin breakdown.

We continue to feel that the most salient feature of our material is the relatively thin ring of low signal intensity on long repetition time, long echo time images, showing a remarkably consistent thickness in all three of our cases. The absence of this appearance in the published literature on MR imaging in multiple sclerosis leads us to believe that it must be a very transient appearance, favoring the free radical hypothesis.

To our knowledge there is only one other published illustration showing this appearance (1). The text accompanying this case report refers only to the appearance and disappearance of a contrast enhanced margin on computed tomography and does not speculate on the nature of the low signal ring on the T2-weighted study. While accepting the important contributions of Nesbit et al and Drayer (2), it nevertheless remains our opinion that our material contains original observations relating to the free radical hypothesis, which may ultimately contribute to the fuller understanding of the etiology of multiple sclerosis.

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Editor's note: The statement that Dr. Nesbitt and colleagues made in their article, "however, free radicals in the macrophages may be the most likely cause of this hyperintensity, as has been described in abscesses," is graciously acknowledged by Powell et al in the preceding letter. I note that the Nesbitt et al article appeared in the August 1991 issue of *Radiology*. The article by Powell et al was submitted to *AJNR* in September and received in October 1991. Initial review was completed shortly thereafter, and a final editorial decision was made in December 1991. It is highly unlikely that Powell et al would have been aware of the report by Nesbitt et al at the time they wrote their manuscript, and it is unlikely that reviewers or editors would have detected the Nesbitt article in standard data bases, as it would be unlikely to have been entered in that short an interval.

I tend to think most reviewers are like me, in that they get around to reading their journals several months after they arrive. Even then, it is the rare individual who reads every article in its entirety, unless there is a specific interest in the subject covered. The point I am making is that while Dr. Nesbitt is correct in that "he got there first," the suggestion of the role of free radicals was not mentioned in his abstract or key words, and unless Powell et al (and the reviewers) had fortuitously come across his article and read it in its entirety, it is unlikely that they would have known of its existence at the time the Powell et al article was being written and reviewed. It probably reflects the analogy of "two ships passing in the night."

AJNR editors attempt to remove all statements to "priority," as such statements add little to the radiographic lesson and are subsequently proved to be incorrect. I suspect that both groups of authors arrived at a similar speculation based on the suggestion by Haimes et al (1). While Nesbitt et al certainly got theirs into print before Powell et al, I think the "oversight" on the part of Powell et al and the editorial review panel at *AJNR* was unintentional and attributable to the large volume of literature on this subject and the inability, even with the best of literature searches, to cover it all in the short time between the publication of the first and the submission and review of the second.

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Specialization's Absurd Limit

Regarding "The Asymmetric Appearance of Intracranial Vessels on Routine Spin Echoes MR Images: A Pulse Sequence-Dependent Phenomenon" (1), the organ-system approach to radiology certainly holds advantage for patients in that clinicians can become specialized in the details of one anatomic area. This has turned out to be particularly effective in neuroradiology, where the complexities of the anatomy and the intricacies of the disease processes are highly specific.

However, when this specialization reaches a point that every piece of knowledge in the radiology community has to be rediscovered and republished from a neuroradiology perspective, specialization has been taken to an absurd limit. This has occurred in the article by Fujita et al, who analyzed the asymmetric appearance of vertebral arteries with relative angulation in MR images. This effect was fully analyzed for the iliac arteries 5 years ago in an article in *Radiology* in 1987 (2).

An invitation is extended to neuroradiologists and, for that matter, to all radiologic subspecialists to rejoin the radiologic community. There is a wealth of knowledge and expertise within the broader discipline, particularly in the developing new technologies. Sometimes discovering that the wheel has already been invented is more effective than reinventing it oneself.

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2. Mayo J, McVeigh ER, Hoffman N, Poon PY, Henkelman RM. Disappearing iliac vessels: an MR phase cancellation phenomenon. *Radiology* 1987;164:555–557

Reply

Before I noticed the asymmetric appearance of vertebral arteries during routine head MR examinations, I had read the article by Mayo et al in *Radiology* describing the asymmetric appearance of the iliac arteries and the theoretical analysis of the phenomenon. Therefore, it is certain that our article, which describes the identical phenomenon in the underlying physical principle, was inspired by a preliminary knowledge that originated from their article. Although the physical principle is exactly the same and the style may be similar, which frequently occurs in scientific writing, the materials included some relatively new techniques and the context was written with emphasis on neuroradiologic aspects of the phenomenon. In writing the article, I tried to take great care so that the credit for the first discovery of the phenomenon would be ascribed to them. Therefore, I believe that our article does not take credit from them, but rather it reevaluates and reinforces their work from a neuroradiologic perspective.

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Editor's note:

In reply to the letter from Drs. Henkelman and Mayo, *AJNR* certainly does not wish to "reinvent the wheel." In fact, every effort is made not to republish material that has appeared in other journals. However, the issue here appears to be one that resembles the difference between theoretical and applied mathematics. As Dr. Fujita acknowledges, Drs. Henkelman and Mayo described the phenomenon. Dr. Fujita properly acknowledged their report in his bibliography and further confirmed their work by applying it in another situation. I think it is legitimate science to give further credence to a theory by applying it to a similar but different anatomic location, much in the way that an applied mathematician would show that a theorem is valid by showing its practical application.

From a purely informational standpoint, it might be unlikely that neuroradiologists would have read the article by Mayo et al on the basis of the heading under which it appeared. The first authors called it a "disappearing vessel" whereas the later authors referred to an "asymmetric appearance" of vessels.

Erratum

Lee R. Guterman's name was omitted from the byline of the March 1993 issue article, "Intraarterial Papavarine as an Adjunct to Transluminal Angioplasty for Vasospasm Induced by Subarachnoid Hemorrhage" (*AJNR: Am J Neuroradiol* 1993;14:346–347). Dr. Guterman, of the Department of Neurosurgery of the School of Medicine and Biomedical Sciences at the State University of New York at Buffalo, should have been listed as second author, between coauthors Kimberly Livingston and Leo N. Hopkins.