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Reply:

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Cerebral Aneurysms in a Patient with Osteogenesis Imperfecta and Exon 28 Polymorphism of *COL1A2*

Petruzzellis et al[1] present an interesting case of a patient with osteogenesis imperfecta and a ruptured aneurysm at the fenestrated basilar artery. However, they misidentify the fenestration as a vertebral fenestration and, as such, do not seem to relate the fenestration to the basilar aneurysm. Figures 1*A* and 2*A* beautifully show well-known features of the basilar fenestration just above the vertebral junction[2,3]: the joining of both vertebral arteries, subsequent division of the basilar artery into 2 arms, effective widening of the distance between the lateral walls of both arms compared with the basilar diameter beyond, and rejoining of the fenestrated arms into 1 artery.

The relationship of aneurysms of the proximal basilar trunk and basilar fenestrations is well known.[2,3] A substantial series by Campos et al[2] of 59 aneurysms of the basilar trunk found 35.5% in association with definite fenestrations, all but 1 at the proximal end of the fenestration. It is possible that other fenestrations were there but were not discerned because the aneurysms were superimposed over the basilar fenestrations, with the result of a higher incidence. With easy-to-do maximum intensity projections or multiplanar reformations with high resolution on CTA or MRA,[4] viewed with a high index of suspicion, we can now readily show fenestrations. With sectioning of image datasets, aneurysms will less likely superimpose fenestrations.

In the case report by Petruzzellis et al,[1] the discussion of osteogenesis imperfecta is interesting and educational for that entity. However, by not paying attention to the details of their own images, they missed the real point of this case. The important entity of aneurysm at the basilar fenestration is considered to develop as a result of hemodynamic forces on the "crotch" of the fenestration, leading to aneurysms in patients without osteogenesis imperfecta. In this patient with osteogenesis imperfecta and a fenestration aneurysm, the question raised is whether osteogenesis imperfecta is an innocent coincidental bystander.

The authors claim that the aneurysm seen 4 months after coiling is new, with angiograms showing a difference between the right posterior oblique views in Fig 1 and left posterior oblique, lateral, and Towne views in Fig 2. Again, the lack of attention to detail of these images leads the authors to claim that a new aneurysm developed in 4 months. This conveniently shows the neck of the so-called "new aneurysm" in the same spot as the treated aneurysm, just at the left side of the proximal split of the basilar fenestration. We can compare Fig 1*A* with Fig 2*C* for the closest possible orientation, and this comparison gives strong suggestion of the same aneurysm with a refilled neck after coiling, a common enough finding. It seems, then, that this aneurysm is not a rare, newly developed one but another occurrence of lack of attention to the details of the case.

Many reports describing coiling of aneurysms at the basilar fenestration are in the literature.[5] Perhaps this is the first reported case in a patient with osteogenesis imperfecta, but the discussion in this case avoids this main theme through oversight of important findings and claims others that are dubious. The *American Journal of Neuroradiology* has an educational responsibility to show readers exemplary neuroimaging cases and interpretations, in addition to rigorous scientific reports and interesting musings of authors in discussion.

References

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Reply:

We thank Dr. Fox for his comments concerning our previously published letter to the editor entitled, "Cerebral Aneurysms in a Patient with Osteogenesis Imperfecta and Exon 28 Polymorphism Of *COL1A2*."[1] He strongly informs us of a misunderstanding regarding the de novo aneurysm that developed after 4 months. Dr. Fox points out some potential errors in our diagnosis, claiming that the de novo aneurysm is actually a refilling of the previously treated one. We believe that indeed at 4 months, our images revealed that a new aneurysm had developed in front of the previous one (Fig 2*B*). Further evidence of this was found in the posttreatment un-subtracted images (not published due to space constraints), in which it was possible to appreciate the stent crossing the vertebrobasilar junction and the coils occluding 2 different and clearly separable aneurysms.

Finally, we are grateful to Dr. Fox for bringing to our attention some bibliographic references that may help us achieve a better understanding of the anatomic features of this particular case.

Reference

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Retraction of Redundant Publication

The article "Does the Oropharyngeal Fat Tissue Influence the Oropharyngeal Airway in Snorers? Dynamic CT Study" by Tolga Aksöz, Hüseyin Akan, Mehmet Celebi, and Banu Baglan Sakan, published in the *Korean Journal of Radiology* (2004;5:102–06) is for the most part identical to an article by Hüseyin Akan, Tolga Aksöz, Ümit Belet, and Teoman Şeçsen, entitled "Dynamic Upper Airway Soft-Tissue and Caliber Changes in Healthy Subjects and Snoring Patients" published in the *American Journal of Neuroradiology* (2004;25:1846–50). All members of the Ethical Committee on Publication of the Korean Radiologic Society agree that the 2 papers belong to the category of redundant publication.

Byung Ihn Choi
President, The Korean Radiologic Society
Kyung Soo Lee
Editor-in-Chief, Korean Journal of Radiology

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Reply:

Byung Ihn Choi, president of The Korean Radiologic Society, reported that members of the Ethical Committee on Publication of that group believe our 2 papers belong in the category of redundant publication.

We don't agree with them. It is obvious fact that those 2 papers are entirely different. Let me explain the differences between the first paper (published in *KJR*) and second paper (published in *AJNR*).

1) The hypotheses certainly are different. In the first paper, the purpose of the study was to validate the premise that snorers may have a smaller oropharyngeal airway area in relation to increased fat infiltration and an elevated body mass index. Because no statistically significant difference was found between snorers and control subjects in terms of total subcutaneous fat width and total parapharyngeal fat pad thickness, we speculated that the oropharyngeal wall muscles may be the cause of narrowing. Therefore, we planned a new study (second paper, *AJNR*) with the purpose of seeking dynamic changes of diameters of the airway and the soft tissue components surrounding the airway during the respiratory cycle.

2) The study methods are different; the measurements are entirely different. In the first paper, airway areas, total thicknesses of parapharyngeal fat pad, and subcutaneous fat pad were measured from the section that had the *smallest oropharyngeal airway area.* In the second paper, on the 2 sections that had the *narrowest and widest airway areas*, anteroposterior and lateral dimensions of the airway and the thicknesses of left and right parapharyngeal fat pads, left and right pterygoid muscles, and left and right parapharyngeal walls were measured, and mean values were calculated for each section. For each subject, *the difference of values in the widest and narrowest phases of the airway* were calculated and used for statistical analysis.

3) The results are not similar. These are quite different because the parameters analyzed are also different. The first basic study had been done with the cephalometric measurements in 2002. In the early months of 2003, the values obtained in that study were analyzed initially and the paper was prepared. Subsequently, the first manuscript was submitted to the *Auris Nasus Larynx* in June 2003. After their negative decision, that paper was submitted to the *KJR,* with some changes based on reviewer feedback, in November 2003. In that first study, the difference in the smallest oropharyngeal airway area between the snorers and control subjects was significant, while there was no significant difference in the total subcutaneous fat width and total parapharyngeal fat width. (After submitting the paper including those measurements and results to the *Auris Nasus Larynx* in June 2003, we decided to seek the changes in some soft-tissue components and in some diameters in relation to respiration. Therefore, we planned a new study measuring oropharyngeal diameters [not oropharyngeal area] and pharyngeal walls and changes in these values to understand which parameters might be the cause of snoring.) In the second study, results showed that the lateral pharyngeal walls in snorers were thinner than in control subjects at the largest phase, whereas they become larger at the end of the expirium, the narrowest phase of respiration. The changes of thickness of the lateral pharyngeal wall between the beginning and the end of expirium in snorers (4.14 mm) were significantly higher than the changes in control subjects (0.66 mm). In that study, changes in the thickness of the lateral pharyngeal wall were significantly related to airway diameter in snorers.

For the second study, we used the CT images obtained from the patients in the first study. We didn't use new study or control groups for 2 reasons. First, to keep out the radiation effects, and second, we thought that if the first study was the basis for the second study, it would be more valuable and reliable.

In conclusion, although the CT scan data obtained from the same patients were used in both papers, we designed the new study (different hypothesis, different measurement parameters, different results, and entirely different discussion) and submitted it to *AJNR* in November 2003.

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Editor's Comment: On Redundant and Duplicate Articles

The availability of large electronic data bases and our ease in querying them makes recognition of redundant and duplicate publications easier. Both are considered to be a type of self-plagiarism. Once an editor recognizes a publication as redundant or duplicate, he or she may choose to inform PubMed with or without warning the author(s). This data base immediately will post a retraction note and a warning indicating the nature of this action. Obviously, this process may have deleterious effects on the reputation of the author(s). If one attempts to open the article published Dr. Aksöz et al in the *Korean Journal of Radiology*, such a warning appears.[1] The Editor-in-Chief of that journal and members of the Ethical Committee on Publication of its parent organization concluded there are enough similarities between that