LETTERS TO THE EDITOR

Regarding "The impact of model assumptions on results of computational mechanics in abdominal aortic aneurysm"

We read with great interest the recent article by Reeps et al,[1] about the impact of model assumptions on the computational mechanics of aneurysms. The authors mention these differences being more important than the differences between patient-specific morphologies and conclude in favor of the pre-stressing computational model.

Although the article is very enlightening, one could wonder as to the extent it is worthwhile to evolve these simulation instruments in order to have accurate rupture risk predictions, without actually becoming a dog chasing its own tail. The problem of estimating aneurysm wall strength is admittedly very complicated and rupture seems to be a localized process, making the identification of possible rupture sites difficult, not always identical to the location of peak wall stress.[2,3] Additionally, since wall thickness, which greatly affects stress values, varies widely, one could doubt whether all these aforementioned problems could be counteracted by further sophisticated computational refinement regarding only the mechanical load on the wall, as the authors suggest.

Furthermore, the real problem in extracting important clinical conclusions from computational studies does not only rely on whether the computational comparisons are dubious or ambiguous, but that the computational findings lack clinical confirmation. For example, findings such as the reported peak wall stress location at sites seldom experiencing rupture (inflection sites of abdominal aortic aneurysm sac near the neck) are highly questionable and this is where the clinical experienced advice and/or criticism is unreplaceable.[4]

In our opinion, the big issue in having undoubtable risk predictions is not that much the refinement of the computational methods but the planning of large clinical studies based on the predictive value of tools that can be objectively estimated and have a confirmed relationship with biomechanical factors, such as aneurysm geometry (eg, centerline tortuosity or the aneurysm neck length).[5,6]

Efstratios Georgakarakos, MD

St. Bonifatius Hospital
Lingen, Germany

Christos V. Ioannou, MD
Theodoros Kostas, MD

Department of Vascular Surgery
University of Crete Medical School
Heraklion, Greece

Yiannis Papaharilaou, PhD

Institute of Applied and Computational Mathematics
Foundation for Research and Technology-Hellas
Heraklion, Greece

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Reply

In recent years, assessment of aneurysm behavior by means of computational simulation techniques have become a widely accepted field of research and has proven to be a very valuable tool (eg, predicting rupture risk, clinical decision finding, or surgical outcome of aneurysm repair). An elaborative overview of literature can be found in references 5 through 7 in our original article and important and highly respected contributions that demonstrate the importance of computational stress analysis in aneurysm rupture risk assessment are cited in references 8, 9, 12, and 16 in our original article.[1-3] Still, there are uncertainties such as wall thickness and strength distributions that one has to be aware of and that currently are considered on a statistical basis[2,3] and in our contribution.[4] Therein, it is clearly demonstrated on a statistically relevant patient group that computational stress analysis in combination with the strength model[2] is superior over classical parameters such as abdominal aortic aneurysm diameter in clinical decision finding.

However, our contribution on model assumptions in computational mechanics of aneurysms is of a different nature. Therein, no statistical patient group is investigated, but rather four randomly selected cases are utilized to show that the choice of computational model assumptions does greatly influence the quantitative results obtained from computational models to an extent that can exceed that of inter-patient variability. At no point in the contribution was the importance of patient-specific morphological characteristics doubted.

We selected seven exemplary model assumptions that have great impact on results and demonstrated the importance of sophisticated complex computational models. Because a lot of researchers have published and will publish computational results on abdominal aortic aneurysms, it is important to see that comparability of such results very much depends on the model assumptions that have been made to obtain them. It is, therefore, a very valuable discussion and demonstration that such results are significantly influenced by not-straightforward and non-obvious factors (eg, pre-stressing or ortho-pressure), as has been pointed out in our contribution, or by the segmentation technique used, as has been previously assessed in reference 11 in our original article.

Evolvement of such simulation instruments is undoubtedly important and is widely accepted in the medical and bioengineering community. We, therefore, do not understand at all the comments made by the letter writers about "a dog chasing its tail" but, nonetheless, strongly disagree. Also, we have never questioned the importance of large clinical trials or the clinical evaluation of predictive tools. On the contrary, such trials are extremely impor-