LETTER TO THE EDITOR

Author's Response

To the Editor:

re: **Finite Element Modeling of Bulk Muscular Tissue** (*JRRD* 1996;33(3):239–52)

The letter from JC Barbenel and VSP Lee that appeared in Vol. 35 No. 2 of *JRRD* criticizes a number of features of the above-cited article. We thank the writers for their work in making these observations, and the *Journal* for the opportunity to have this discussion.

The central theme of the critique is that the constants extracted in the article for the three-term strain-energy function are inappropriate. The critique proposes a new set of constants (c_{10} = −0.0896 kPa, c_{01} = 0.0863 kPa, c_{11} = −0.158 kPa) in place of those in the article (c_{10} = 2.6 kPa, c_{01} = $c_{10}/4$ (assumed) = 0.64 kPa, c_{11} = 5.7 kPa). These proposed constants are more than an order of magnitude lower than those in the article, and two of the three constants are negative. However, it appears that some computational or typographic error has been made in the critique's set of constants. First, the behavior produced seems physically impossible. For example, uniaxial tension would be caused by compressive surface tractions (that is, the material would be 'stretched' by 'pushing' on it). Using the constants proposed in the critique, a uniaxial tension of stretch ratio 1.2 produces a compressive engineering stress of −0.047 kPa. Second, while the magnitude of the stiffness produced by the critique's constants is obviously less than that of the article's constants, the critique's experimental data plotted in **Figures 1** and **2** are clearly stiffer. Equation 3 of the critique is correct and generates a curve similar to that marked "calculated" in **Figure 1.** Therefore, the problem may be in the least-squares fit of the critique, or a typographical error.

A second main theme is that the behavior produced by the original constants is not realistic, being contrary to experimental results presented in the critique, and contrary to that generally present in the literature. The methods used to produce the data presented in the critique are not described, which makes these data difficult to interpret. All that is stated is that these are compression tests of excised muscle. Post-mortem tissue changes, fluid exudation during testing, and other possible confounding factors come to mind; one would need to know how these were treated in order to evaluate the new findings. Further, it is not obvious to us what literature the critique refers to which shows more abrupt transition (and it is unstated). The article referenced data showing that the Mooney-Rivlin formulation, using the assumption $c_{01} = c_{10}/4$ in the absence of more data, is a reasonable approximation for many elastomers under moderate deformation. The Ogden reference presents a useful discussion on determining elastomeric constants. Though the critique makes much of the difference, Mooney-Rivlin is the same as the three-term strain-energy function with the final constant, c_{11}, set equal to zero. As the article stated, we found that inclusion of one of the constants with the higher-order terms in λ; c_{11}, c_{20}, or c_{30}, was necessary to model the stronger nonlinearity observed in our data.

Thirdly, the choice of axes for **Figure 10** is criticized. **Figure 10** was included because one of the manuscript's reviewers requested a "true" stress/"true" strain curve for the constants. The reviewer's viewpoint was that work in soft tissue mechanics is often presented using various nonlinear conjugate pairs, and that it would be helpful for comparison's sake to see the data in that form. In an alternate viewpoint, the critique states that data are "generally presented in the literature using engineering stress or the load." It is not obvious to us that any particular form dominates in the literature of soft tissue mechanics.

Finally, the article is criticized as being too restrictive, and insisting on $c_{01} = c_{10}/4$. We had no such intent (nor would we agree with one from the limited data we had); in fact, we explicitly did not even restrict choices of strain energy function. The article stated "It should be noted here that it was also possible to fit the data nearly as well using the pair of constants c_{10} and c_{20}, or the pair of c_{10} and c_{30}; there is no strong basis for choosing the pair c_{10} and c_{11}." and that a hypoelastic assumption might be a "reasonable first-approximation."

Again, we thank the *Journal* for the opportunity to reply to the critique.

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