From: **Markus J. Buehler** EviseSupport@elsevier.com
Subject: Invitation to revise manuscript JMBBM_2018_729
Date: July 4, 2018 at 4:03 AM
To: msacks@ices.utexas.edu

BM

Ref: JMBBM_2018_729
Title: A Material Modeling Approach for the Effective Response of Planar Soft Tissues for Efficient Computational Simulations
Journal: Journal of the Mechanical Behavior of Biomedical Materials

Dear Dr. Sacks,

Thank you for submitting your manuscript to Journal of the Mechanical Behavior of Biomedical Materials. We have completed the review of your manuscript. A summary is appended below. While revising the paper please consider the reviewers' comments carefully. We look forward to receiving your detailed response and your revised manuscript.

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I look forward to receiving your revised manuscript as soon as possible.

Kind regards,

Markus J. Buehler
Editor-in-Chief
Journal of the Mechanical Behavior of Biomedical Materials

**Comments from the editors and reviewers:**
**-Reviewer 1**

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Ms. No. JMBBM_2018_729

Title: A material modeling approach for the effective response of planar soft tissues for efficient computational simulations

By: W Zhang, R Zakerzadeh, W Zhang, M.S. Sacks

Review:

The manuscript presents a hyperelastic anisotropic model for soft tissues. The paper is overall well-written and well-structured, although perhaps too long. The scientific content is very good and the model is fully verified. Examples on heart valves are also demonstrative. The model is phenomenological, and arguments for using the approach are sound; I fully subscribe them.

Then, I have no doubt that the manuscript merits publication in

JMBBM. However, my recommendation is major revision as to allow the authors to improve and polish the manuscript.

My comments, mainly opinions, but that the authors can consider, are:

1. It seems to me that the last version was not read carefully by the last author, because even though I am not English native and I may be wrong, I can see some small English issues (and the last author is a well known, “experienced” enough if I am allowed to say, researcher from a US University). Just a few examples: page 3, line 29 “…the efficient of…”, page 4, line 54 “…can taking advantage…”, line 61, “…used to perform the actual…”, and so on.

2.  Section 2.1 has many questionable explanations, meaning that they can be debatable. For example, “there is no specific set of invariants and pseudo invariants that can cover all forms of material behavior of interactions”. I guess that the authors have some stored energy form in mind, because any complete set of invariants is equivalent in terms of representing the deformation state of a material. The choice of invariants is tied to reduced forms of the stored energy. Considering isotropic materials, obviously f(I1)+g(I2)+h(I3) is not the same as l(E1)+m(E2)+n(E3). But the general functional form W(I1,I2,I3) fully represents any possible case of isotropic material, the same way as W’(E1,E2,E3). The selection is important when we reduce possibilities. And we typically reduce possibilities because of limited experimental data.

3. Lets see in a simpler way. Consider the position of a point expressed as cartesian coordinates (x,y). Consider it now expressed by polar coordinates (r,theta). Both (xy) and (r,theta) are complete. A function W(x,y) can be written as W(x(r,theta),y(r,theta)) = W’(r,theta). BUT f(x) + g(y) cannot be written in general as h(r) + g(theta), or f(x)g(y) as h( r)g(theta). The differences are the couplings. Depending of the problem, we could express it just as R(r ) (e.g. a wave in an infinite medium) or as X(x) (e.g. a wave in beam).  Invariants I1,I2,I3 are classical inherited from Rivlin, Mooney and co-workers because in rubbers (chain models) I1, I2 and I3 had slight coupling (I3 = 1, and I2 neglected often). Components (of any strain tensor) in material directions are also a good choice.

4. The single reason I know for the preference on Green-Lagrange strains over any other is that they are immediately obtained from the displacements gradient (without spectral decompositions or so). Then, they are used in Finite Element programs to mount the stiffness matrices (material and geometrical). I see no physical reason to prefer them over others. In fact, all stress and strain measures are fully equivalent from a physical standpoint, and the selection arbitrary (except for convenience), since there is a one-to-one TENSOR mapping among them (not component-by-component), see for example:

Latorre, M., & Montáns, F. J. (2016). Stress and strain mapping tensors and general work-conjugacy in large strain continuum mechanics. *Applied Mathematical Modelling*, *40*(5-6), 3938-3950.

5. Precisely, the argument in Fig. 4 applies to a component or when considering stresses to be a constant tensor times the strains. Note that the deformation gradient defines a unique deformation state. On the other hand, going back to Fig. 4, if a material is stretched to infinite, we assume that the deformation is infinite, and the stress needed to arrive at that deformation, also infinite. But if it is compressed to zero, probably I would expect minus infinite, and the stress also minus infinite given the energy needed in fusion. Then if considering “intuition”, which one is more “intuitive”? Also, readers may understand by logarithmic strains the logarithm of one of the stretch tensors, which is the usual nomenclature.

6. The coupling for Green-Lagrange strains is larger than for logarithmic strains because their components hardly represent what it is usually interpreted by us. For example, in a simple shear test, the shear Green-Lagrange strain goes to infinite, whereas it is well known that physical shearing begins to drop to zero after about gamma=3 (it becomes increasingly uniaxial stretching). Classical logarithmic strains represent correctly this observation.

7. Given the above comments, and taking into account also the length of the manuscript, I would consider the possibility of eliminating most of Section 2.1. There is no need to get into debatable arguments when they have little to do with the essence of the manuscript. I would just

they have little to do with the essence of the manuscript. I would just mention that you prefer G-L strain. It is a fine personal choice as good as any other!

8. Page 16, “1) It’s difficult to compare model parameters…”. One of the reasons for this may be that there are not enough tests to have a physically determined problem for the proposed form of the energy, see for example for the simplest isotropic case:

Latorre, M., De Rosa, E., & Montáns, F. J. (2017). Understanding the need of the compression branch to characterize hyperelastic materials. *International Journal of Non-Linear Mechanics*, *89*, 14-24.

9. Regarding convexity, It is a very good analysis. I asume that the authors refer to convexity respect to the strain measures they used (Drucker stability). Does it also hold respect to the gradient? It may be obvious, but I do not see it. Has this been shown in any publication?

In summary, these are just comments that the authors may consider to enhance the manuscript, which overall, is a major contribution to the field and should be published.

**-Reviewer 2**

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**A Material Modeling Approach for the Effective Response of Planar Soft**

**Tissues for Efficient Computational Simulations**

Paper id: JMBBM_2018_729

by Will Zhang, Rana Zakerzadeh, Wenbo Zhang, Michael S. Sacks

The paper deals with the development of an effective constitutive model capable of reproducing the essential responses of soft tissues.

The considered topic falls within the aim and scope of the journal and is of interest for the journal's audience.

The manuscript in the present form is not suitable for publications and some aspects need to be properly amended as specified below.

- Exponential models are cited in eq (10) and extensively used throughout the paper. However, no proper references are provided on this class of models.

- The paper deals only with phenomenological rather than physics-based approaches to the mechanical response of soft tissues. The manuscript would improve significantly if some considerations about the physics behind the adopted energy function (10) will be added in the amended version of the paper. Despite no micromodels are considered, some physical relations (e.g. through invariants, …) must hold when a phenomenological model is adopted. Please comment on this aspect.

- The paper is not very well organized; too many sub-sections are present, some of them seem to treat a similar topic (e.g. 3.2 Convergence and 4.5 Convergence rate). I suggest to clean the paper by keeping only the fundamental sections and sub-sections in a very schematic way. The paper is also very long and many figures are present. Some sections (e.g. sect. 5) have a title that does not explain the content of the section itself. Please correct correspondingly.

- Every figure containing more than one curve must have a legend (see Figs 12, 13, 14, 15, …)

- Fig. 3 is not clear (horizontal axes have not a clear meaning). Fig. 3b: the horizontal axis should be placed at the bottom of the figure, not overlapped with the data.

- Fig. 4: no color scale is provided

- In sect. 2.5 the application to planar soft tissues is proposed; however, it is not clear as the energy function (10) that is claimed to be used along the paper, is considered in the expression (20).

- Also the numbers and kinds of loading paths necessary for parameters estimation has not been clearly explained.

- Fig. 10. This figure is not readable form a quantitative comparison viewpoint. I suggest to add some cross sections in order to display 2D graphs. Separation lines between sub-figures are unnecessary.

- The authors cite a FE code in which the constitutive model has been implemented; some details about the numerical analyses should also be given when the various cases are illustrated in the Results section.

- The paper contains many language errors and need to be carefully proofread. In this form it's not suitable for publication (e.g.: Abstract: 'An robust…'; page 3, line 21: '…to derived…'; page 4 line 51: '… difficult to not possible to extend…'; page 7 line 123: '…not entirely clearly'; page 12 line 199: '… not well pose'; page 16 line 284: '…of parameters obtaining.'; page 21 line 385: '…we use to test the our approach …'; page 31, line 532 'We also testing other…'; and many others). Every sentence must be verified.

others). Every sentence must be verified.

- As a closing remark, despite the topic treated in the manuscript is worth of investigation, the paper in the current form presents this issue in an unclear way and it's hard to read and understand.

I suggest to shorten the paper and for its complete reorganization, and to consider all the above listed points.

Despite the paper deals with an important topic within the mechanics of tissues, in the present form it is not suitable for publication and a major revisions is required.