**Referee: 1   Comments to the Author**

This is a very nice and very interesting paper. It presents a simple, yet very efficient and useful, idea for numerical analysis of material instabilities. The idea and its context are well presented, and comprehensive testing is provided on small strains and finite strains examples.

*We thank the reviewer for the very valuable comments that help greatly improve the quality of this manuscript. Responses to individual comments are as follows. All changes in the revised manuscript have been highlighted in blue.*

A couple minor remarks appeared during the reading:

(1) in section 2.1, it would be interesting that the authors motivate their specific choice for the interpolation of F (eq. 2.10)

*This particular form of interpolation of* ***F*** *is proposed by Sun et al. (2013) as a new adaptively stabilized finite element scheme to simultaneously cure the multiple-locking phenomena related to shear failure, incompressibility imposed by pore fluid, and/or incompressible solid skeleton and produce solutions that satisfy the inf-sup condition. This form of* ***F*** *is implemented in the research code Albany used for all numerical examples. This new reference has been added to the reference list and the following explanation on the choice of F is added after eq. (2.10)*

*“This particular form of interpolation of* ***F*** *is proposed by Sun et al. (2013) as a new adaptively stabilized finite element scheme, which is capable to deliver stable, locking-free numerical solutions.”*

*Sun, W., Ostien, J. T., & Salinger, A. G. (2013). A stabilized assumed deformation gradient finite element formulation for strongly coupled poromechanical simulations at finite strain. International Journal for Numerical and Analytical Methods in Geomechanics, 37(16), 2755-2788.*

(2) in the uniaxial tension test at finite strains, two global minima appear: with the cartesian parametrization, they are separated by a quite small ridge. Can the authors comment on numerical difficulties (if any) associated to that 'weak' separation of two different solutions?

*[QC - My understanding is that the reviewer asks how we pick a single solution to the bifurcation problem is there are multiple global minima. Currently, the program simply returns the first of multiple solutions.]*

*This work adopts a two-stage approach (initial sampling + Newton iterative solve). In the first sampling stage, the objective function is checked at every sampling grid and the minimum value is picked as the initial guess for the following Newton iterative solve. Though the code could detect multiple global minima up to the machine precision, in the case of multiple global minima, it only returns the first of the multiple minima. For the purpose of identifying when bifurcation happens, such implementation is sufficient. This comment has been added after Figure 13.*

*“It should be pointed out that for the Cartesian parametrization, the two global minima are separated by a very small ridge. Though the current algorithm could detect multiple global minima, it only returns the first of the multiple minima. For the purpose of identifying when bifurcation happens, such implementation is sufficient.”*

**Referee: 2   Comments to the Author**

The work considers the problem of the bifurcation of the constitutive behaviour of dissipative materials, identified as a material instability. It mainly focuses on analyzing the incidence, of the parameterization of the normal to the strain discontinuity path, in the performance of one of the possible strategies for the numerical resolution of the material bifurcation problem.

The problem is the classical one in non-linear solids mechanics, consisting of finding the polarization vectors of the “acoustic” tensor, by minimization of its determinant. The considered numerical strategy is based on two stages: a) sampling, in the exploration domain determined by every parametrization in the tridimensional space, in order to (possibly) fall on the convex set around the minimum, and b) and iterative (possibly Newton-like) procedure, in the proposed algorithm, to set its exact value. The performance of different parameterizations (including the one proposed by the authors and termed “Cartesian parametrization”) is then analyzed and compared in terms of the robustness and the associate computational cost.

This is an interesting piece of work, focusing a subject that clearly falls into the scope of IJNME. The paper is well written, clearly structured and related works are appropriately referenced.

*We thank the reviewer for those detailed and very valuable comments that help greatly improve the quality of this manuscript. Responses to individual comments are as follows. All changes in the revised manuscript have been highlighted in blue.*

Before sending a revised version of the work I suggest the authors to consider the following issues:

(1) It seems to me that section 2.1 “Variational constitutive updates” is here superfluous. I do not claim it is wrong, I just claim that the proposed algorithm could be retrieved directly from the chosen constitutive equation (2.13) and, thus, that the derivations in equations 2.1 to 2.12 could be skipped (or sent to an Appendix) in order to not distract the reader’s attention from the main topic of the work.

*[QC - Should we move this to Appendix section?]*

(2) It is clear that the proposed parametrization (Cartesian parametrization) is the one best performing out of the four compared. However, it is still requiring the two-stages (space-sampling + iteration) strategy. The sampling stage shows necessary, since even with the authors’ proposed parametrization, the algorithm is not converging in some cases (see last row in Table 4). I assume that this stage is also responsible for a large part of the computational cost in many cases. This would make interesting for the reader, that the work elucidated (either by providing some additional information or by performing some additional numerical simulations) the following aspects:

•Split the run time into every stage (at least for some representative cases)

•Provide some comparison, in terms of robustness and computational cost, with strategies skipping the sampling stage (the one of reference [25] for instance), in order to elucidate if further research on the topic should be addressed on skipping the first stage rather than on improving the efficiency of the second.  In any case, I think that some additional considerations on the single/double stage strategies for the numerical solution of the material bifurcation problem, and their associated benefits/disadvantages, should be made in the work (may be in the “Conclusions” section ?).

*1. We could show the split run time of every stage for different parametrizations, for the anisotropic model example (finite deformation); [QC - this is done]*

*2. Oliver’s work [25] applies to tensors with both major and minor symmetries (in order to have accurate solution). One thing we could try, is to run the small deformation example with Oliver’s algorithm, and see how they compare. [we compared Oliver’s method using the small-deformation example, the Oliver’s method is much much faster: using 1000 random initial guess for the eigenvector, the success rate is 100% and the average run time is only 9.84 microsecond. However, Oliver’s method only applies to tangent with major and minor symmetries. When we apply Oliver’s method to the finite deformation example, it cannot detect the correct bifurcation time or the bifurcation direction]*

*3. As the reviewer suggested, comment on the single/double stage approach, include it in the “Conclusions” section.*

*4. My student and I are investigating an alternative (a stochastic optimization) approach to this bifurcation problem. However, this work is not completed and published yet, so no reference can be referred to.]*

(3) The presented numerical examples do not challenge very much the proposed algorithm. In my opinion the challenge of the material bifurcation problem does not lie on the large-strain character of the kinematics or on the complexity of the considered constitutive problem, but rather on the complexity of the stress/strain field that the material is subjected to. In spite that the proposed approach seems to be 3D, and general for symmetric/unsymmetric models, the the considered tangent operators are symmetric (see eqs. 5.5 and 5.10) and, in addition, “in-plane” solutions are enforced (in all cases the z-component of the normals are zero). In my opinion, the benefits of the proposed approach in terms of robustness would be much more convincing if an additional numerical example with a more general character (unsymmetrical model and full 3D solution) was included.

*[QC - We are checking the proposed algorithms using a 3D triaxial extension test example. However, I am inclined to not including any more numerical examples. ]*

(4) Some (minor) comments/suggestions are:

• Similarly to eq. (3.3), in eq. (3.7) I missed the restriction: “subjected to x^2+y^1=1”

• line 206 the intervals [-pi/2,pi/2] should be [-1,1].

*[QC - I don’t think we need the restriction of x^2 + y^2 = 1. Also, the intervals [-pi/2, pi/2] is correct. maybe the confusion is caused by the use of canonical basis x, y, which on the graph, seem to be on the unit sphere?]*

*For tangent parametrization eq.(3.7), x, y are points on the tangent plane with* ***u****=[x, y, 0]T representing the position vector from north pole (Q) to point P on the tangent plane. Therefore, it is not subjected to x^2+y^1=1*. *The range [-pi/2, pi/2] guarantees that the position vector would cover all points on the tangent plane. The confusion might come from the use of canonical basis x,y to parametrize* ***n****. This has been clarified in the revised manuscript.*

*“Note that though the normal vector $~n$ is a unit vector, the position vector on the tangent plane $~u =[x,y,0]^T$ is not subjected to the constraint $x^2+y^2+z^2 = 1$ and the range $[-\pi / 2, \pi / 2]$ is sufficient for the position vector to cover all points on the plane.”*

• In eq. (5.1) and line 282, superscript “e” and the name “elastic“ to denote strain tensor is misleading and I think it should be reduced to “strain”, since it suggests that the total strain of the model is split into inelastic and inelastic counterparts (which is not the case).

*The superscript ``e’’ on strain tensor has been removed.*