Review History for "Micro-macro mechanics of damage and healing in rocks"

Chloé Arson ***2019***

Summary

The paper was sent to two Reviewers: Prof. Eleni Gerolymatou, Chalmers University of Technology (Reviewer 1) and one anonymous reviewer (Reviewer 2). The two reviewers remained anonymous during the entire revision process. After the reviewing process was completed, Reviewer 1 decided to disclose their identity.

In the first round of review, both reviewers and the Editor recommended that minor revisions be made to the manuscript before it could be published.

After minor revisions, Reviewers 1 and 2 recommended accepting the manuscript without any required modifications. The Editor decided to accept the manuscript without further modification.

Review Round 1

Reviewer 1 (Eleni Gerolymatou)

The authors present a review of previous work on the subject of damage and healing in rocks. The work is easy to read and follow and I recommend it for publication subject to some minor modifications.

For brevity, minor comments relating only to spelling or grammatical errors are omitted from the review history.

1. Major Remarks

   - Scope: The scope of the work is not clear. It does not seem to include novel work and the authors themselves make no claim of novelty. On the other hand it does not seem to be a review article, as it includes the mathematical formulations of the different approaches and examples and is strongly focused on the work of the authors' team, as mentioned in the article. The work has a lot of pedagogical merit and would be very useful to doctoral students or researchers relatively new to the field, but that is not claimed as the goal by the authors. It would be very helpful if the scope could be made clear to assist the reader.
   - Structure: The structure is confusing at certain points. For example section 4.3.2 belongs to section 'Link between micro-mechanical and phenomenological damage models', but is not concerned with the link between the two approaches. It is recommended to review the structure and mark the sections corresponding to examples as such.

2. Minor Remarks

   - Abstract: It is mentioned that the use of non-local regularization poses a problem for healing, as this can only happen when damage localizes. Strictly speaking damage localizes in non- local formulations, the width of the discretization simply does not depend on the mesh. The sentence should be reformulated.
   - Introduction: No references are present in the introduction, though differed works are refered to.
   - Line 11: The importance of rock mechanics to civil engineering applications should also be included.
   - Line 100: Substitute 'n the unit vector' with 'n is the unit vector'.
   - Line 143: The rate of damage is mentioned but it has not yet been introduced in the equations of hyperplasticity.
   - Plasticity-damage coupling: As this is used and mentioned often, it would be useful if the authors could include a discussion on the similarities and differences of the two.
   - Equations (43) and (45): It should be mentioned that the first is a sub-case of the second.
   - Line 338: It is mentioned that 'To avoid damage localization, damage softening models are formulated with non-local variables'. This is not true, as the use of non-local variables does not mean that damage does not localize, it just means that the width of the localization band is finite and does not depend on meshing.
   - Line 346: Substitute 'numerical' with 'differential' to match the section titles.
   - Figure 4: No experimental results are provided for comparison, even though it is mentioned that the simulations capture them.
   - Figure 5: Judging by the figure on the bottom right, the localization width is smaller than the element size of the figure above it. This means that the two simulations cannot be directly compared, as non-local formulations need a meshing several times smaller than the characteristic length to work properly. In any case the ratio of characteristic length to element size should be mentioned. For the same comparison it would be of interest to show that the load-deflection curves coincide.
   - Section 3.1: Not all of this is relevant to damage mechanics, especially the parts focusing on granular media, whose fabric may vary, but not as a result of damage. The link to damage should be clarified.
   - Line 475: The dependence on the fabric tensor, which is implicit, is hardly a limitation, at most a constitutive or numerical inconvenience. Moreover the critical state is not truly relevant to damage mechanics. Maybe a clarifying remark could be added as to why this is relevant to the present work.
   - Lines 478 - 490: It would be useful to the reader to refer to the corresponding depictions of the mentioned fabric tensor definitions in figures 6 and 7.
   - Line 505: The used strain rate corresponds to more than 3
   - Line 556: Substitute 'Eq.' with 'Equation' for uniformity.
   - Line 560: The last sentence is somewhat disconnected from the previous text.
   - Equation (96): As far as I could find, the definitions of Yi and C0 have not been introduced. If that is the case, they should be mentioned. If they have been introduced it would be helpful to mention the relevant equation at this point of the text.
   - Figure 13: It would be useful to include the corresponding experimental results.
   - Figure 15: Parts are pixelized and the axes legends of sub-figures c and d are missing. Moreover, it would have been useful to also include the experimental results, at least for sub-figures a and b. missing.
   - Figure 18: The resolution is low. The directions of the principal stresses in the vertical or horizontal axes are not clear.
   - Line 990: Substitute 'inactivate' with 'disactivate'.

Reviewer 2 (Anonymous)

The manuscript is generally very well written with an extensive literature review of recent advancements in damage and healing mechanics, along with many examples in both theoretical and numerical aspects from the author's group. Minor revisions are required.

Detailed comments:

1. The author discussed the anisotropy of rocks. However, only isotropic damage criteria are presented (Eqs. 115 and 116). It may be necessary to briefly review anisotropic damage criteria.
2. Many damage models can well capture the rock strength under simple loading conditions and under low confining pressures. It is more challenging to capture the behaviors of rocks under more complex loading conditions and under high confining pressures. Please comment on models from both the literature and the author's group.
3. Many presented examples have been focused on numerical results. Please also include relevant experimental results if available for selected examples.
4. Some equations embedded in figures are illegible, e.g. figure 1 and figure 9. Some data points are covered by the legend in figure 4. The caption of figure 16 is missing. Font sizes are not consistent in different subfigures in figure 14. Please clean some language errors, for example, "both in both" in line 509 should be "both in", "and and" in line 642 should be "and". Please also make the format of references consistent.

Editorial Comments

1. As noted by reviewer 1, the scope of the paper should be made clearer in the abstract and introduction. The introduction states "we present the first attempt to couple Continuum Damage and Healing Mechanics to microstructure evolution". This suggests that the work is novel, but the manuscript mostly contains summaries of the prior work of the author and others. Perhaps "we present a thorough review of recent attempts to couple..." is more appropriate. Please make it clear that this is a review article meant to summarize attempts to couple Continuum Damage and Healing Mechanics if that is truly the intent, or clearly identify the novel contribution of the work. I also suggest the author consider placing the motivating science questions of the introduction before the discussion of the computational models. This may make the motivation of coupling CDM and healing mechanics stronger. This, however, is up to the author.
2. Please make lowercase words that follow equations when the words are a continuation of a sentence. Use periods and commas after equations as appropriate when they are meant to be the end or middle of a sentence, respectively. This will help us greatly when typesetting.
3. Please ensure that figures have changed sufficiently from their original for reproduction without permission of the respective publisher.
4. Consider a bulleted or enumerated list in the Conclusion to summarize findings of sections 2 – 5. Some of the recommendations at the end of the conclusion are not discussed throughout the text where they may be motivated more appropriately. For instance, a note about existing CDM models lacking hyper-plasticity could appear at the end of section
5. Some typos:
   - I suggest replacing "deriving" with "differentiating" where that is meant (e.g. in section 2.2.3 after Eq. (26)).

Review Round 1: Author Response

I thank the reviewing committee for the insightful comments and the detailed review of the paper. Suggestions have helped me think further of how to present the contents and how to explain it better. Below, I am explaining how I addressed each point raised by the reviewers and by the editor. Revisions made in the manuscript appear in blue font.

Reply to Reviewer 1

Reviewer's comments are typed in gray, Author's replies in red.

Major Remarks

- Scope: The scope of the work is not clear. It does not seem to include novel work and the authors themselves make no claim of novelty. On the other hand it does not seem to be a review article, as it includes the mathematical formulations of the different approaches and examples and is strongly focused on the work of the authors' team, as mentioned in the article. The work has a lot of pedagogical merit and would be very useful to doctoral students or researchers relatively new to the field, but that is not claimed as the goal by the authors. It would be very helpful if the scope could be made clear to assist the reader. Point well taken. This paper is a hybrid between a review paper and a summary

  of the research done with my students over the past ten years. Equations help really understand how to get started in formulating a model – better than by just summarizing the strengths and weaknesses of previous models. Reviewer A is right in pointing out that the paper was written for doctoral students or researchers relatively new to the field. This is exactly what I had in mind when I wrote it. I made the scope of the paper clearer in the abstract and in the introduction.

- Structure: The structure is confusing at certain points. For example section 4.3.2 belongs to section 'Link between micro-mechanical and phenomenological damage models', but is not concerned with the link between the two approaches. It is recommended to review the structure and mark the sections corresponding to examples as such.

  Understood. I reorganized Section 4 entitled "MicroPmechanics enrichment" as follows:

  4.1. Micro-plane theory:
  Subsection 4.2 in the original manuscript
  4.2. Micro-mechanical damage models: Same general introduction as in the original version of the paper, followed by the former subsection 4.3.1 on the expression of the free energy for a dilute distribution of penny? shaped cracks
  4.3. Example: The Discrete Equivalent Wing Crack Damage (DEWCD) model:
  Part of subsection 4.1 in the original manuscript 4.4. Example: Combining micro-mechanics with phenomenological damage functions:
  Subsection 4.3.2 in the original manuscript

  In Section 3 entitled "Fabric enrichment" and in Section 5 entitled "Homogenization schemes," I also added the word "example" in the title of all subsections that present an example.

Minor Remarks

- Abstract: It is mentioned that the use of non-local regularization poses a problem for healing, as this can only happen when damage localizes. Strictly speaking damage localizes in non-local formulations, the width of the discretization simply does not depend on the mesh. The sentence should be reformulated.

  Agreed. I reformulated the sentence.

- Introduction: No references are present in the introduction, though differed works are referred to.

  Correct. This is intentional. The whole paper reviews the literature, so I kept the introduction and the conclusion short and straightforward. I did not want to interrupt the reading by long series of references into brackets, and I did not want to favor any author by citing their work in the introduction. The names of the authors mentioned in the introduction are cited later in the body of the text. I clarified the scope of the paper and my writing intent in the abstract and in the introduction.

- Line 11: The importance of rock mechanics to civil engineering applications should also be included.

  Of course, how did I forget this? I included the relevance of rocks to civil engineering.

- Line 100: Substitute 'n the unit vector' with 'n is the unit vector'.

  The sentence is "We note $\boldsymbol{n}$ the unit vector normal to $\boldsymbol{\gamma}$". I do not see any verb missing. I left the sentence as is.

- Line 143: The rate of damage is mentioned but it has not yet been introduced in the equations of hyperplasticity.

  Actually the rate of damage mentioned in line 143 refers to the theory of Continuum Damage Mechanics. I start talking about the hyper-plasticity framework just after, in the next paragraph. I do not see any inconsistency, so I left my text as is.

- Plasticity-damage coupling: As this is used and mentioned often, it would be useful if the authors could include a discussion on the similarities and differences of the two.

  Good point. I added a few sentences to Subsection 2.5 (2.6 in version R1), entitled "Irreversible Deformation," to highlight the main similarities and differences between CDM and the theory of plasticity. I did not compare specific constitutive models though, because I think that this would require significant developments that would go beyond the scope of this paper.

- Equations (43) and (45): It should be mentioned that the first is a sub-case of the second.

  Granted. I mentioned that equation 43 (48 in version R1) was a particular form of equation 45 (50 in version R1) just after equation 45 (50 in version R1).

- Line 338: It is mentioned that 'To avoid damage localization, damage softening models are formulated with non-local variables'. This is not true, as the use of non-local variables does not mean that damage does not localize, it just means that the width of the localization band is finite and does not depend on meshing.

  Agreed, thank you for correcting this statement. I rephrased the sentence.

- Line 346: Substitute 'numerical' with 'differential' to match the section titles.

  Yes, of course. I replaced numerical by differential in this sentence.

- Figure 4: No experimental results are provided for comparison, even though it is mentioned that the simulations capture them.

  Sorry for the lack of clarity. The calibration of the model is explained in the original paper (cited in the manuscript) where the model was presented: W. Jin, C. Arson, 2016. Nonlocal enrichment of a micromechanical damage model with tensile softening: Advantages and limitations. Computers and Geotechnics, 94:196–206. Calibration simulations are not reported in the present manuscript. Simulation results presented in Figures 3 and 4 are not replicates of specific physical experiments, but rather, model predictions for two stress paths commonly studied in experimental rock mechanics. Results show that the calibrated model captures general trends observed experimentally in quasi-brittle materials, such as the dependence of damage development on the confining pressure, compressive hardening and the difference of up to one order of magnitude between the tensile and compressive yield stresses. I clarified the text, and I cited references that report experimental observations.

- Figure 5: Judging by the Figure on the bottom right, the localization width is smaller than the element size of the Figure above it. This means that the two simulations cannot be directly compared, as non-local formulations need a meshing several times smaller than the characteristic length to work properly. In any case the ratio of characteristic length to element size should be mentioned. For the same comparison it would be of interest to show that the load-deflection curves coincide.

  The localization width in the bottom right figure was actually about twice the size of the element size in the Figure above it. The internal length parameter in these simulations was lc = 0.01 m. The element size was 0.0065 m for the coarse mesh and 0.002 m for the fine mesh, so that the ratio characteristic length to element size was 1.5 for the fine mesh and 5 for the fine mesh. I added this information in the manuscript. The load-deflection curves obtained with the coarse and fine meshes coincide up to a certain load, after which, they deviate from each other. This is because the micro-mechanics damage model cannot exhibit full softening. For information, below are the load-displacement curves obtained for the same three-point bending problem, with the same two mesh sizes, but with a phenomenological damage in which the material stiffness can be totally deteriorated. The match between the two force-displacement curves is excellent. I decided to not comment on the load-displacement curves in the revised manuscript, because a detailed discussion is already enclosed in the original article (cited in the paper) where the model was published (W. Jin, C. Arson, 2016 Nonlocal enrichment of a micromechanical damage model with tensile softening: Advantages and limitations. *Computers and Geotechnics*, 94:196-206).

- Section 3.1: Not all of this is relevant to damage mechanics, especially the parts focusing on granular media, whose fabric may vary, but not as a result of damage. The link to damage should be clarified.

  Agreed. I deleted Equations 81-82 of the original manuscript, which were too focused on the anisotropic critical state theory (ACST). I also added some comments to explain how the contact- based and void-based fabric tensors used in granular mechanics could be used to characterize the evolution of crack patterns in rock mechanics.

- Line 475: The dependence on the fabric tensor, which is implicit, is hardly a limitation, at most a constitutive or numerical inconvenience. Moreover the critical state is not truly relevant to damage mechanics. Maybe a clarifying remark could be added as to why this is relevant to the present work.

  OK. I rephrased the statement on limitations to present it as an inconvenient. I cut off most of the text on the ACST.

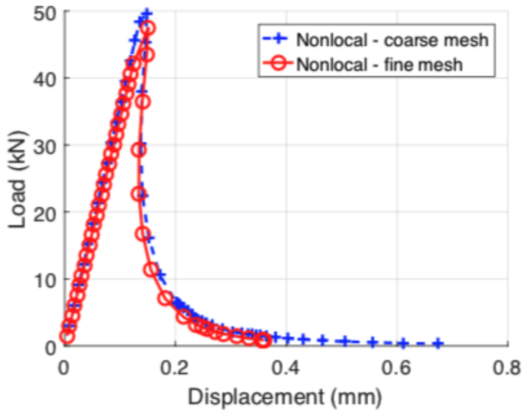

Figure 1: Load-displacement curves obtained in the three-point bending test presented in Subsection 2.6 for the same two mesh sizes as those presented in the manuscript, but with a phenomenological damage model that exhibits full softening.

- Lines 478 - 490: It would be useful to the reader to refer to the corresponding depictions of the mentioned fabric tensor definitions in Figures 6 and 7.

  Understood. I included sketches in Table 1 to illustrate the microstructure descriptors the closest possible to where they are defined. I used the sketches of Figure 6, and added new ones when needed. Since Figure 6 had become redundant, I deleted it.

- Line 505: The used strain rate corresponds to more than 3% per minute.

  OK. I changed "a constant [displacement] rate of 0.034mm/s" by "a constant [displacement] rate of 0.034mm/s, which corresponds to slightly more that 3% per minute".

- Line 560: The last sentence is somewhat disconnected from the previous text.

  Agreed. There was no mention of intra-granular cracks in the text preceding that sentence. So I changed the last sentence of the section for "To capture both the cracks that are observable in microscopy images and those that are not, a micro-mechanical approach is preferred."

- Equation (96): As far as I could find, the definitions of Y_i and C_0 have not been introduced. If that is the case, they should be mentioned. If they have been introduced it would be helpful to mention the relevant equation at this point of the text.

  Sorry for that. I did forget to define these terms. Y_i is the variable that is work-conjugate to the density of cracks oriented with a normal n_i. Y_i is a driving force. C_0 is the damage threshold: if Y_i exceeds C_0, then cracks normal to) n_i propagate. I added the definitions of Y_i and C_0 in the revised version of the manuscript.

- Figure 13: It would be useful to include the corresponding experimental results.

  Like Figures 3-4, Figures 12-13 present synthetic cases aimed at showing that the model can capture trends and orders of magnitudes reflecting rock behavior. Calibration simulations are presented in the paper (cited in the present manuscript) where the model was originally presented (W. Jin, C. Arson, 2017. Discrete equivalent wing crack based damage model for brittle solids. International Journal of Solids and Structures, 110:279–293). I clarified the text of the manuscript.

- Figure 15: Parts are pixelized and the axes legends of sub-figures c and d are missing. Moreover, it would have been useful to also include the experimental results, at least for sub-figures a and b.

  The lack of legibility of some figures is due to a problem of file compression that occurred when I submitted the manuscript to the journal. Apparently the manuscript file was recompiled before being sent out for review. The version of the manuscript that you received is not the one I submitted. Equations appear correctly in the full sized figures that I have on my computer: I am sending these full sized figures for version R1. I added legends of subfigures c. and d. together with an explanatory sketch: both figures represent projections of the principal micro-stresses on the radial and vertical axes, with compression in the bottom left quadrant, and tension in the top right quadrant. On these figures, I deliberately decided to not include the experimental data in the plots because it was impossible to match the time at which secondary and tertiary creep occurred. As a result, I put the emphasis on the fact that the model could capture the change of creep regime, from secondary to tertiary, with creep rates that matched the ones observed in physical experiments.

- Figure 18: The resolution is low. The directions of the principal stresses in the vertical or horizontal axes are not clear.

  The lack of legibility of some figures is due to a problem of file compression that occurred when I submitted the manuscript to the journal. Apparently the manuscript file was recompiled before being sent out for review. The version of the manuscript that you received is not the one I submitted. Equations appear correctly in the full sized figures that I have on my computer: I am sending these full sized figures for version R1. I added sketches in Figure 15 to explain how to read the micro-stress map. I also edited the text referring to Figure 18 to clarify. Basically, each principal micro-stress is projected along the radial and axial directions. A positive principal stress (tension) is plotted in the upper right quadrant, and a negative principal stress (compression) is plotted in the lower left quadrant.

- Line 990: Substitute 'inactivate' with 'disactivate'.

  Actually, "disactivate" is not English according to the Merriam-Webster Dictionary. One can say "inactivate" or "deactivate". I replaced my first choice, "inactivate", by "deactivate", which seems equivalent.

Reply to Reviewer 2

The manuscript is generally very well written with an extensive literature review of recent advancements in damage and healing mechanics, along with many examples in both theoretical and numerical aspects from the author's group. Minor revisions are required.

- 1The author discussed the anisotropy of rocks. However, only isotropic damage criteria are presented (Eqs. 115 and 116). It may be necessary to briefly review anisotropic damage criteria.

  Granted, in the original version of the paper, I did not present any anisotropic damage criteria. Prompted by Reviewer B, I added a subsection to the manuscript (Subsection 2.3, entitled "Anisotropic damage criteria" in version R1) to explain how to model damage for materials with stress-induced anisotropy and for materials with intrinsic anisotropy. For intrinsically anisotropic materials, I explained why it is necessary to have different damage functions for different directions. I took one of the models established by my group as an example.

- Many damage models can well capture the rock strength under simple loading conditions and under low confining pressures. It is more challenging to capture the behaviors of rocks under more complex loading conditions and under high confining pressures. Please comment on models from both the literature and the author's group.

  Thank you for the suggestion. The first paragraph of the added subsection 2.3. ("Anisotropic damage criteria") explains why it is so challenging to capture complex macroscopic responses that involve stiffness reduction, non-linear stress/strain response and transition from brittle to ductile behavior. I summarized the challenges encountered when simulating confinement-induced strengthening. I cited references from the literature and some of my own work.

- Many presented examples have been focused on numerical results. Please also include relevant experimental results if available for selected examples.

  Most simulation results presented in this paper (e.g., Figures 3-4, Figures 11-12, Figures 17-18 and Figures 19-20 of the revised manuscript) are synthetic cases that show that the proposed model can capture the trends and orders of magnitude reflecting rock behavior. So no experimental results are available. Calibration simulations are presented in the papers where the models were originally published (these papers are cited in the presented manuscript). Simulations presented in Figure 14 are the only ones that were compared to experimental results. However, since it was impossible to match the time of occurrence of tertiary creep, I did not superpose the experimental and numerical results and insisted instead that the model could capture the occurrence of tertiary creep as well as the creep rates.

- Some equations embedded in figures are illegible, e.g. figure 1 and figure 9. Some data points are covered by the legend in figure 4. The caption of figure 16 is missing. Font sizes are not consistent in different subfigures in figure 14. Please clean some language errors, for example, "both in both" in line 509 should be "both in", "and and" in line 642 should be "and". Please also make the format of references consistent.

  The version of the manuscript you received is not the one I submitted. Apparently the manuscript file was recompiled before being sent out for review. The lack of legibility of some figures is due to a problem of file compression during the submission process. The cropped legends also are the result of a recompilation after submission. I am sending the full sized figures for version R1, and I hope that the manuscript that you will receive will be more legible. I fixed the lack of consistency in the fonts in Figure 14 (Figure 13 in version R1). I fixed the typos highlighted by Reviewer B. I noted that one reference was cited as "[52]". It is a typo: no reference was needed there (relevant references were cited elsewhere in the manuscript).

Reply to Editorial Comments

- As noted by Reviewer A, the scope of the paper should be made clearer in the abstract and introduction. The introduction states "we present the first attempt to couple Continuum Damage and Healing Mechanics to microstructure evolution". This suggests that the work is novel, but the manuscript mostly contains summaries of the prior work of the author and others. Perhaps "we present a thorough review of recent attempts to couple..." is more appropriate. Please make it clear that this is a review article meant to summarize attempts to couple Continuum Damage and Healing Mechanics if that is truly the intent, or clearly identify the novel contribution of the work. I also suggest the author consider placing the motivating science questions of the introduction before the discussion of the computational models. This may make the motivation of coupling CDM and healing mechanics stronger. This, however, is up to the author.

  Understood. I made the scope of the paper clearer in the abstract and in the introduction and I rephrased the sentence "we present the first attempt to couple Continuum Damage and Healing Mechanics to microstructure evolution" to "we explain how to couple Continuum Damage and Healing Mechanics to microstructure evolution".

- Please make lowercase words that follow equations when the words are a continuation of a sentence. Use periods and commas after equations as appropriate when they are meant to be the end or middle of a sentence, respectively. This will help us greatly when typesetting.

  Done, thank you.

- Please ensure that figures have changed sufficiently from their original for reproduction without permission of the respective publisher.

  Since this paper is a review paper that takes a number of examples from my previous research, most figures were published in some form in other journals, as stated in the manuscript. All figures were changed compared to the previous published versions. I do not know how to check "how different" they should be to allow publication without permission from the previous publishers. I tried multiple library websites and did not really find an answer. According to Stanford Library's website, I could reuse figures as they are for "fair use", i.e. for commentary, criticism or parody (https://fairuse.stanford.edu/overview/fairPuse/whatPisPfairPuse/). I have always been told that modifying a figure was sufficient to be able to publish it again without permission of the first publisher. But I never heard about specific criteria on how different the figures should be, and did not find such criteria anywhere. Can you please advise?

- Consider a bulleted or enumerated list in the Conclusion to summarize findings of sections 2 – 5. Some of the recommendations at the end of the conclusion are not discussed throughout the text where they may be motivated more appropriately. For instance, a note about existing CDM models lacking hyper-plasticity could appear at the end of section 2.

  The note about existing CDM models lacking hyper-plasticity actually appears at the end of Subsection 2.2, which is the subsection about hyper-plasticity. I prefer to leave my conclusion as they are, because they are more a summary than actual conclusions with new findings. I used bullet points to propose possible ideas for future research, which are concise statements.

- Typos

  1. I suggest replacing "deriving" with "differentiating" where that is meant (e.g. in section 2.2.3 after Eq. (26)).

     OK, I changed "deriving" for "differentiating" after equation 26 and at all other instances where it was necessary.

Review Round 2

Reviewer 1 (Eleni Gerolymatou)

The work can be published as is. I only have the following small modification to suggest, subject to the discretion of the author(s) and the editor:

The term 'invariant', used on line 229, is generally used to denote quantities related to tensors that remain constant under (any) rotation. In this case the axis of rotation is however fixed, meaning that a subclass of all possible rotations is considered. To avoid confusion it would be nice of the author(s) could emphasize the limitation assumed here, even if the same terminology is used as for the more general case.

Reviewer 2 (Anonymous)

The authors have adequately addressed all issues raised by the reviewer.

Editor decision

At the end of Review Round Number 2, the managing Editor has decided to accept the revised version of the manuscript for publication without any further changes.