Review History for "Enriching constitutive models with meso-scale behaviour: a thermodynamics-based formulation and examples"

Giang Dinh Nguyen, Arash Mir, Dr, Ha Hong Bui, A/Prof *2022*

Summary (optional)

This paper was submitted to Open Geomechanics, and Prof.. Tamagnini was the reviewer until his departure from the Open Geomechanics editorial board during Review Round 1. Dr. Andò took over the review process from there.

The review process was, as is standard in Open Geomechanics, performed under double blind conditions. There were two review rounds, after which the Open Geomechanics editorial board decidede to accept the paper. Once this decision was made, both reviewers agreed to reveal themselves publicly, they are Prof. Stanislaw Pietruszczak from McMaster University in Hamilton, Canada and Prof. Chloé Arson from Georgia Tech in Atlanta, USA, for which they are thanked.

Prof. Arson was invited to be part of the Open Geomechancics editoral board during the review process of this article, but obviously remained an impartial reviewer.

Review Round 1

Reviewer 1 (Prof. Pietruszczak)

The paper reads very much like an overview of research conducted by the authors over the last decade. There are over 20 references to their own work, most of them directly related to the current submission. There is no visible contribution to the formulation of the problem, computational aspects and/or implementation in the numerical analysis. Virtually all examples provided (viz. Figs. 4-14) are, in fact, taken directly from other recent publications. The reviewer has also serious reservations regarding the simulation of triaxial tests in the softening regime. The latter involve a point integration using a complex damage plasticity model for the localization zone and a trial-and-error adjustment of the shear band thickness, which is assumed to be comparable to the height of the sample. Such an interpretation raises concerns.

Below are some selected comments on the work.

Re. Introduction

- The literature review provided in the Introduction is inadequate. The conceptual framework behind this approach (i.e. the notion of constitutive law with embedded discontinuity) originates from the work of Pietruszczak and Mroz (Int. J. Num. Meth. Eng., 17, 1981) and Pietruszczak (J. Damage Mech., 8, 1999). This should be clearly stated (at present, none of these papers is even mentioned). Also, there is a large volume of work in relation to FE techniques for capturing strong discontinuities (see Jirasek, Comp. Meth. Appl. Mech. Eng. 188 (2000) and/or Olivier et al., Comp. Meth. Appl. Mech. Eng. 195 (2006) for a comprehensive survey and a comparative study) which should also be acknowledged.

Re. Section 2

- The authors state that '...the thickness of the shear band (h) can be considered significant compared to the size of the specimen (H)'. In fact, the numerical analysis employs the ratios of h/H within the range of 0.2-1. Is this really a localized failure mode? The latter is typical for plane-strain specimens, where a narrow shear band (with thickness proportional to median particle size) begins to develop in the hardening regime, while in triaxial tests the failure mode is quite diffused. Thus, the employed ratios of h/H approaching 1 are not indicative of strain localization.

  Also, the kinematic enrichments for the localized failure, viz. eqs.(1)-(4), have already been introduced in early 1980's (cf. references mentioned in the previous point). The same applies to the statements of volume averaging of stress field (eq.26) and traction continuity (eq.30).

- It is implied that this approach does not require analysis of a BVP. Localization always occurs in the context of an inhomogeneous stress/deformation field, so by definition this is a BVP.

- Further on this point; how is the value of 'h' assigned in the context of analysis of an initial- boundary value problem? Some key details regarding the implementation of this approach in the FE framework should be provided.

Re. Section 3

- The behaviour inside the localization band is described using a damage-plasticity model. The description employs 10 material parameters (cf. Section 3.2) in addition to a rather vague notion of thickness 'h'. How can all these parameters be identified? What kind of experimental tests can be conducted to define properties within the localization zone, if the latter is said to have a significant thickness?

- For viscoplastic simulations the applied strain rates are up to 1/s. At those rates the strength in a triaxial test will significantly increase due to dynamic effects, even if the material is rate independent. How can you decouple in this case the rate sensitivity from the inertia effects?

- The relevance of section 2.2 to plasticity formulation used in 3.1, or visco-plasticity framework in 3.3, is not very clear. The latter can be phrased without resorting to thermodynamic considerations. The comment that '...details can be found in Mir et al. 2018...' is not sufficient here.

Re. Section 4

- The bifurcation criterion implies that the onset of localization is path-dependent. In this case, how can one define a unique surface, like in Fig. 9a, along which the transition occurs?

  Also, the onset of bifurcation typically occurs in hardening regime when the stress trajectory approaches the failure envelope. This is this not the case in Fig. 9a, as the stress state shown there is on the 'yield surface' (?). In addition, is the failure mode at high confining pressures (Fig. 10a) indeed associated with localization?

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Re. Section 5.

- The triaxial tests in the softening regime are simulated through a point integration using the damage plasticity model (section 3) assigned to describe the properties in the localization band, the thickness of which is '. . . fine-tuned so that the overall response produced by two-scale model can match that by FEA'. In reviewer's opinion this has no physical basis.

Some selected editorial comments:

- The quality of English needs improvement throughout the text.

  (e.g., see the first two sentences in Introduction: 'Constitutive models are used to described..'; 'As a result, it can be written for a volume element and then used to describe the behaviour of an arbitrary volume element in the analysis of BVP as long as this volume element ...')

- p.2, lines 1-2; '... not formulated in a thermodynamically consistent framework... ' The approach referred to does not violate any laws of thermodynamics, so why is this labeled as being not 'thermodynamically consistent'. Also, why is this approach 'not systematic'? I'd suggest rephrasing this paragraph.

- In Section 2.2 (thermodynamics-based formulation) several variables are not defined.

- In Section 3.2, 'beta' is not defined in Table 1 (should be 'alpha', viz. eqs. (53), (59)). Also, proper reference should be given in the Table caption.

- Eqs. (32)-(35) are a repetition of eqs. (23), (30), (4) and (5).

Reviewer 2 (Prof. Arson)

The authors summarize a decade of work on a thermodynamically consistent continuum-based theory of mesoscale enrichment that can be used to predict localization. The key is to assume proper constitutive relationships for the localization zone (e.g., shear band) and for the zone around it ("matrix"). A closed formulation is achieved by introducing a reasonable expression for the free energy and using Ziegler orthogonality rule. By construction, the modeling framework can capture softening behaviors. The authors present the key derivations necessary for numerical implementation and simulate laboratory tests with the proposed approach as well as with the Finite Element Method.

The paper presents a concise introduction to the so-called "two-scale Representative Volume Element" (RVE). None of the contents is novel, and the authors self-cite abundantly and openly. To my opinion, this is acceptable, because the authors clearly state that the goal of the paper is pedagogic. Since the work was already published elsewhere and widely accepted by the community, there is not much to criticize on technical grounds except for the basic assumptions of the proposed theory, which, for the most part, are also clearly stated in the paper. As a result, I have only a few minor comments, and I recommend "minor revisions".

Technical questions/comments:

1. Most of the key equations needed to learn the theory are given. But for Eq. (2), the reader has to refer to other publications to understand the proof. If possible, I recommend that the authors summarize the keys steps of the demonstration that lead to that equation.

2. In Equation (36), the authors make the implicit assumption that the direction of the normal to the shear band and the thickness of the localized zone are fixed once and for all. The authors acknowledge this point later in the paper, but I think that it would be useful to also explain it when the assumption is used for the first time (before equation 36 for example).

3. Equations 45-50 (implicit return mapping algorithm): I recommend adding a few sentences to explain why the proposed algorithm converges unconditionally.

4. In the present version of the paper, the authors make claims on the computational efficiency of their approach and the reader is referred to other publications to be convinced. To make the paper more pedagogic and to make it stand alone, I request adding data. For example, in page 18 (Analysis of localized failure), the authors state that "The enhancement based on two-scale constitutive approach can reproduce the same trend of behaviour at a fraction of the computational cost compared to FEA". This statement and the plots described by it indicate that the accuracy of the proposed model is similar to that of the FEA for the same assumed shear band parameters, and that the main advantage of the proposed approach is computational cost. However, the authors do not elaborate on this latter part. This section would be stronger of the authors could summarize their observations on computational efficiency (time to convergence with the two-scale RVE vs. the FEA, for example). The same comment applies to the statement "The two-scale model assumes the width of the localisation band to be invariant throughout post-localisation phase of deformation. This assumption approximates the variations of the kinematic field within the localisation band and the surrounding bulk and can help reduce the computational cost significantly... " in page 21.

5. Since one of the key messages is that upon proper constitutive assumptions, the proposed approach can be fast and accurate, it would be good to show-case the impact of having a good constitutive model in the formulation. In at least one of the numerical examples presented, the authors could analyze the sensitivity of their approach to the shear band thickness, shear band direction and/or constitutive parameters, and measure the error made in estimating the softening stress-strain curve of a given experiment. Doing so would increase the impact of the paper and make it more pedagogic.

6. Minor edits:

   (a) 8: "Therefore, formulations of constitutive models for responses inside and outside the localisation band can following standard procedures..." should be "Therefore, formulations of constitutive models for responses inside and outside the localisation band can follow standard procedures..."

   (b) 13: "it is pointless to have a perfect match at this stage without correct underlying correct failure/deformation mechanism" should be "it is pointless to have a perfect match at this stage without correct underlying failure/deformation mechanism"

   (c) 21: "the two-scale model can match that by FEA using the same core damage-plasticity model enhanced with rate-dependent." should be "the two-scale model can match that by FEA using the same core damage-plasticity model enhanced with rate-dependence".

Author Response

We thank the editors for facilitating the review process and the reviewers for constructive comments. In the electronic copy of the modified paper, the principal changes are highlighted in blue. Given both reviewers have already figured out the authors' identity through references to earlier publications, we have included our names in this response to their comments.

Reviewer #1

The paper reads very much like an overview of research conducted by the authors over the last decade. There are over 20 references to their own work, most of them directly related to the current submission. There is no visible contribution to the formulation of the problem, computational aspects and/or implementation in the numerical analysis. Virtually all examples provided (viz. Figs. 4-14) are, in fact, taken directly from other recent publications. The reviewer has also serious reservations regarding the simulation of triaxial tests in the softening regime. The latter involve a point integration using a complex damage plasticity model for the localization zone and a trial-and-error adjustment of the shear band thickness, which is assumed to be comparable to the height of the sample. Such an interpretation raises concerns.

The reviewer is correct that the paper is like an overview of our work over the last 10 years, given there are results from published papers included for illustration. We have explicitly stated that in the original manuscript. However, it is not correct to say that all examples are taken from our publications, given there are unpublished components from a PhD thesis (Mir 2017) by the second author (Dr Arash Mir). In particular, the thermodynamic approach in Section 2.2 for the case of a finite thickness band is based on the use of constraints and allows straightforward adaptation of procedures established in Houlsby and Puzrin (2000) for localised failure. The analysis of localised failure in Section 5 (Figs. 11-14) illustrates the performance of the model against detailed FEM modelling of a triaxial test. This assessment, focusing on a sandstone under triaxial loading condition and the mechanism of localized failure that a constitutive model should possess to describe correctly both pre- and post-localisation behaviour, has not been used or published before. In addition, results in Fig. 10 on Ural marble are unpublished and taken from this PhD thesis. We have revised the second paragraph of Section 5 to clarify the reviewer's concern.

Regarding the simulation using the model described, the additional parameter (shear band thickness) is a physical one added to an existing model that can provide more flexibility in a more physical sense to the calibration of parameters. The strain profiles across the localisation band in Fig. 2 show the evolution of localised failure with thickness of the band evolving and it is in fact comparable to the size of the specimen at an early stage of localisation. It is noted that H is not the height of the sample, but an effective thickness depending on the band orientation and also the volume element considered (see Fig. 3). The observed evolution of localisation band thickness is also consistent with a more recent analysis on localised failure in soils by Rattez et al, 2022 (`https://doi.org/10.1680/jgeot.20.P.120`). The use of a fixed value of the band thickness in this paper is of course a simplification of the physical phenomena, and this is acknowledged in the original manuscript (1st para, Section 5). In short, the mechanism of localised failure behind observed macro behaviour is the focus and addressed in a simplest possible and physically meaningful way in constitutive modelling in our opinion. Of course, there are still several unsolved issues that the reviewer has noticed, but all of them have been openly discussed in the manuscript and addressed in the conclusion to avoid misleading information.

Below are some selected comments on the work.

Re. Introduction

-The literature review provided in the Introduction is inadequate. The conceptual framework behind this approach (i.e. the notion of constitutive law with embedded discontinuity) originates from the work of Pietruszczak and Mroz (Int. J. Num. Meth. Eng., 17, 1981) and Pietruszczak (J. Damage Mech., 8, 1999). This should be clearly stated (at present, none of these papers is even mentioned). Also, there is a large volume of work in relation to FE techniques for capturing strong discontinuities (see Jirasek, Comp. Meth. Appl. Mech. Eng. 188 (2000) and/or Olivier et al., Comp. Meth. Appl. Mech. Eng. 195 (2006) for a comprehensive survey and a comparative study) which should also be acknowledged.

We thank the reviewer for suggesting these references. However, in the original manuscript, several references to the work by Pietruszczak and co-authors were mentioned, and a brief assessment was given (end of 4th paragraph of the Introduction). We have also provided brief assessments of the strong discontinuity approach (under the name Enhanced Assumed Strain, and XFEM) used for the FE-based enrichment in the literature (2nd paragraph of the Introduction), including the work by Oliver (1996). In the same sense, other kinds of enrichments using higher-order theories and smeared crack/deformation concepts have also been addressed in the original manuscript to provide a landscape for our focus on constitutive modelling and associated enhancements at the constitutive level. It has been specifically addressed in the original manuscript (3rd paragraph) that these FE-based enrichments are not within the scope of this study that focuses on constitutive modelling and enrichments not involving a numerical method for the solution of BVPs. In the revision, all references suggested by the reviewer have been added.

Re. Section 2

- The authors state that '?the thickness of the shear band (h) can be considered significant compared to the size of the specimen (H)'. In fact, the numerical analysis employs the ratios of h/H within the range of 0.2-1. Is this really a localized failure mode? The latter is typical for plane-strain specimens, where a narrow shear band (with thickness proportional to median particle size) begins to develop in the hardening regime, while in triaxial tests the failure mode is quite diffused. Thus, the employed ratios of h/H approaching 1 are not indicative of strain localization.

This statement (first para of Section 2.1, quoted in full here: "As can be seen in Fig. 2, despite very brittle behaviour observed, the thickness of the localisation band can be considered significant compared to the size of the specimen, making the assumption of a finite thickness band reasonable") refers to the experimental observation illustrated in Fig. 2. It indicates, with evidence from the experiment, that the localisation process is not abrupt even in an extreme case (snap-back, very brittle behaviour) and the width of the localisation band (obtained using strain profile) is significant enough compared to the specimen size. The thickness of the (active) band is in fact an evolving quantity as can be seen in Fig. 2 for brittle behaviour, and sand under triaxial condition (see Rattez et al 2022; https://doi.org/10.1680/jgeot.20.P.120). As observed in both sands and rocks, localisation of deformation takes place and this process is gradual towards an ultimate state (a crack in rocks, and a thin shear band in sands).

The numerical analysis of localized failure did not use h/H=1 or approaching 1, and it is clearly indicated in Fig. 13 that h/H=1 is the case of homogeneous behaviour (of a classical continuum constitutive model). Fig. 13 shows the results of a parametric study for the effect of band thickness on macro behaviour. Given the evolving thickness of the localisation band, one might have to either use the band thickness as a variable (see Nguyen Bui, 2020; https://doi.org/10.1016/j.ijsolstr.2019.05.022), or accept a simplification with fitting element to make the constitutive model simpler. The latter option is used in this paper and as can be seen in Fig. 11, the ratio h/H=0.25 used is not close to 1.

For further information, our calculation using experimental data in Rattez et al (2022; https://doi.org/10.1680/jgeot.20.P.120; Fig. 8 on the evolution of shear band thickness in sands) results in the ratio h/H ranging from about 0.05 (ultimate) to 0.4 (early stage of localisation). In such cases (a cylindrical specimen with a diameter of 70mm, and height of 140mm, with an assumed shear band angle of 45°), the effective size H of the specimen is about 100mm.

Also, the kinematic enrichments for the localized failure, viz. eqs.(1)-(4), have already been introduced in early 1980's (cf. references mentioned in the previous point). The same applies to the statements of volume averaging of stress field (eq.26) and traction continuity (eq.30).

We totally agree with the reviewer that all equations mentioned are classical, as also seen in the references next to the equations in the original manuscript (the volume-averaged stress and strain are too trivial to have a reference). However how to use them all in constitutive modelling to deal with localised failure in a more physical and also more correct way is another aspect that we would like to address in this paper, given classical continuum constitutive models are invalid in post-localisation stage of behaviour. Of course, what we have been doing in this paper is still not totally correct, in the sense that all constitutive models are incorrect. However in our opinion it is in a good direction by adding a physically meaningful parameter (thickness of the band), given all what a constitutive modeller can do is to approach as close as possible to the physical and observable phenomena. The formulation and examples in this paper show the importance of having correct mechanisms of localised failure behind the macro responses in constitutive modelling of geomaterials.

- It is implied that this approach does not require analysis of a BVP. Localization always occurs in the context of an inhomogeneous stress/deformation field, so by definition this is a BVP.

We state in the Introduction that "we aim to tackle localised failure at the constitutive level without resorting to the analysis of BVPs and a numerical method for the solutions of BVPs". This is to make the proposed approach independent of characteristics of any numerical methods, and hence facilitating its implementation in both mesh-based and mesh-free computational codes. This does not imply the analysis of BVPs is not needed. In fact, all our publications cited in this paper involve the analysis of BVPs using the proposed approach. We have revised the 3rd paragraph of the Introduction to clarify this point.

- Further on this point; how is the value of 'h' assigned in the context of analysis of an initial- boundary value problem? Some key details regarding the implementation of this approach in the FE framework should be provided.

The implementation of this kind of model is described in Section 2.3 (Computational aspects). The thickness of the band is a parameter, the determination of which is included in Section 5 (Analysis of localised failure). It has also been stated in Section 5 that this thickness should be smaller that the element (or size of integration point), given the focus of the approach on a large (enough) scale in which the size of a material (or integration) point is larger than the localisation band thickness.

- The behaviour inside the localization band is described using a damage-plasticity model. The description employs 10 material parameters (cf. Section 3.2) in addition to a rather vague notion of thickness 'h'. How can all these parameters be identified? What kind of experimental tests can be conducted to define properties within the localization zone, if the latter is said to have a significant thickness?

We thank the reviewer for this insightful question and totally agree that a constitutive model or approach should always be accompanied with and supported by both experimental tests and analysis of experimental data for identification and calibration of parameters. We have now added a sub-section in Section 5 to address this comment.

- For viscoplastic simulations the applied strain rates are up to 1/s. At those rates the strength in a triaxial test will significantly increase due to dynamic effects, even if the material is rate independent. How can you decouple in this case the rate sensitivity from the inertia effects?

We have not made it clear enough in the original manuscript. Rate-dependency is used as a regularisation method to allow obtaining discretisation-dependent solutions in the analysis of BVPs using the FEM. There are no inertia effects in the finite element analysis (FEA). The first paragraph of Section 3.3 has now been revised to clarify this.

- The relevance of section 2.2 to plasticity formulation used in 3.1, or visco-plasticity framework in 3.3, is not very clear. The latter can be phrased without resorting to thermodynamic considerations. The comment that '?details can be found in Mir et al. 2018?' is not sufficient here.

We thank the reviewer for addressing this and have added more details in Sections 2.3, and 3.1 to explain how to enhance an existing continuum constitutive model using the proposed two-scale approach. The rate-dependent regularisation in Section 3.3 is for FEA of BVPs (e.g. like a direct numerical simulation) used to assess the performance of the approach, and has no direct links with it.

Re. Section 4

- The bifurcation criterion implies that the onset of localization is path-dependent. In this case, how can one define a unique surface, like in Fig. 9a, along which the transition occurs?

Also, the onset of bifurcation typically occurs in hardening regime when the stress trajectory approaches the failure envelope. This is this not the case in Fig. 9a, as the stress state shown there is on the 'yield surface' (?). In addition, is the failure mode at high confining pressures (Fig. 10a) indeed associated with localization?

The surface illustrated in Fig. 9a is the initial yield surface, defining initial elastic region. The behaviour is path-independent inside this initial yield surface, and this allows the determination of the onset of bifurcation on the initial yield surface (i.e. stress states of points on this initial yield surface). We would like to add that the onset of bifurcation also occurs in softening regimes under low (enough) confining pressures and this is a much stronger type of localisation as seen in our experimental observation in Section 2 and several other papers in the literature. The inclination of localisation band decreases with the increase of confining pressure and under high confining pressures, compaction localisation takes place with the occurence of almost horizontal compaction bands. Fig. 10a illustrates the mechanical behaviour under different confining pressures, and not all of them lead to localisation, as the zone on the initial yield surface where onset of bifurcation is detected clearly shows that under sufficiently high confining pressure, localisation does not occur at initial yield (see Fig. 10b).

Re. Section 5.

- The triaxial tests in the softening regime are simulated through a point integration using the damage plasticity model (section 3) assigned to describe the properties in the localization band, the thickness of which is '?fine-tuned so that the overall response produced by two-scale model can match that by FEA'. In reviewer's opinion this has no physical basis.

To us this is a calibration process that is always needed in constitutive modelling, and given the assumption of fixed width of localisation band this fine-tuning process is just a way to obtain a match with numerical/experimental results (in which the band thickness is evolving). Of course, this is not ideal as the reviewer has addressed, but in our opinion the whole model with an embedded band and its calibration are much more physically meaningful than using a classical continuum model (based on an assumption of homogeneous deformation) to describe behaviour of a volume element or specimen with a localisation band as widely seen in the literature. This is the point we would like to address in this paper.

Some selected editorial comments:

- The quality of English needs improvement throughout the text.

(e.g., see the first two sentences in Introduction: 'Constitutive models are used to described..'; 'As a result, it can be written for a volume element and then used to describe the behaviour of an arbitrary volume element in the analysis of BVP as long as this volume element ?')

We have revised these sentences and proof read the manuscript to improve its readability.

- p.2, lines 1-2; '? not formulated in a thermodynamically consistent framework?' The approach referred to does not violate any laws of thermodynamics, so why is this labeled as being not 'thermodynamically consistent'. Also, why is this approach 'not systematic'? I'd suggest rephrasing this paragraph.

We have revised this paragraph as suggested.

- In Section 2.2 (thermodynamics-based formulation) several variables are not defined.

We thank the reviewer for addressing this and have revised a few parts of this Section to make sure all variables are defined.

- In Section 3.2, 'beta' is not defined in Table 1 (should be 'alpha', viz. eqs. (53), (59)). Also, proper reference should be given in the Table caption.

Thank you for spotting this typo. It is correct that it should be "alpha". We have revised equation (59) to use the same symbol in both Table 1 and Section 3.2.

- Eqs. (32)-(35) are a repetition of eqs. (23), (30), (4) and (5).

We deliberately want to summarise the relationships needed for computational algorithms and have provided an explanation for that.

Reviwer #2

The authors summarize a decade of work on a thermodynamically consistent continuum-based theory of mesoscale enrichment that can be used to predict localization. The key is to assume proper constitutive relationships for the localization zone (e.g., shear band) and for the zone around it ("matrix"). A closed formulation is achieved by introducing a reasonable expression for the free energy and using Ziegler orthogonality rule. By construction, the modeling framework can capture softening behaviors. The authors present the key derivations necessary for numerical implementation and simulate laboratory tests with the proposed approach as well as with the Finite Element Method.

The paper presents a concise introduction to the so-called "two-scale Representative Volume Element" (RVE). None of the contents is novel, and the authors self-cite abundantly and openly. To my opinion, this is acceptable, because the authors clearly state that the goal of the paper is pedagogic. Since the work was already published elsewhere and widely accepted by the community, there is not much to criticize on technical grounds except for the basic assumptions of the proposed theory, which, for the most part, are also clearly stated in the paper. As a result, I have only a few minor comments, and I recommend "minor revisions".

We thank the reviewer for the constructive comments. However, we would like to clarify that the thermodynamic formulation (Section 2.2) and results (Fig. 10 on Ural marble, and all results in Section 5) are unpublished results from a PhD thesis (Mir 2017) by the second author (Dr Arash Mir). Please see our answer to the first comment by Reviewer 1 for more details.

Technical questions/comments:

Most of the key equations needed to learn the theory are given. But for Eq. (2), the reader has to refer to other publications to understand the proof. If possible, I recommend that the authors summarize the keys steps of the demonstration that lead to that equation.

We are not sure about this question, as Eq. (2) does not need any references. We have revised the paragraph above it to make the description clearer.

In Equation (36), the authors make the implicit assumption that the direction of the normal to the shear band and the thickness of the localized zone are fixed once and for all. The authors acknowledge this point later in the paper, but I think that it would be useful to also explain it when the assumption is used for the first time (before equation 36 for example).

We thank the reviewer for this suggestion and have added a comment below Eq. (36) to address this.

Equations 45-50 (implicit return mapping algorithm): I recommend adding a few sentences to explain why the proposed algorithm converges unconditionally.

We have revised the paragraph below Eq. (50) to address this. Convergence can be guaranteed if the strain increment is sufficiently small.

In the present version of the paper, the authors make claims on the computational efficiency of their approach and the reader is referred to other publications to be convinced. To make the paper more pedagogic and to make it stand alone, I request adding data. For example, in page 18 (Analysis of localized failure), the authors state that "The enhancement based on two-scale constitutive approach can reproduce the same trend of behaviour at a fraction of the computational cost compared to FEA". This statement and the plots described by it indicate that the accuracy of the proposed model is similar to that of the FEA for the same assumed shear band parameters, and that the main advantage of the proposed approach is computational cost. However, the authors do not elaborate on this latter part. This section would be stronger of the authors could summarize their observations on computational efficiency (time to convergence with the two-scale RVE vs. the FEA, for example). The same comment applies to the statement "The two-scale model assumes the width of the localisation band to be invariant throughout post-localisation phase of deformation. This assumption approximates the variations of the kinematic field within the localisation band and the surrounding bulk and can help reduce the computational cost significantly?" in page 21.

We agree with the reviewer that adding quantitative assessment for computational efficiency will make the paper better. However, in our opinion this is only essential and reasonable for two approaches that have a common (or the same) computational basis (e.g. 2 constitutive models implemented in the same FE code). This is not the case for the assessment in Section 5, given it is obvious, at least to us, that the time to complete the calculations at the material point level (using our FORTRAN code) is within a minute, while FEA time (using ABAQUS and UMAT routine) is at the order of a few hours. In this sense the difference is clearly at least 2 orders of magnitudes (1 minute vs. minimum 2-5 hours, depending the FE mesh and model, and also number of CPU cores used). We have included the above information in the revised manuscript.

Since one of the key messages is that upon proper constitutive assumptions, the proposed approach can be fast and accurate, it would be good to show-case the impact of having a good constitutive model in the formulation. In at least one of the numerical examples presented, the authors could analyze the sensitivity of their approach to the shear band thickness, shear band direction and/or constitutive parameters, and measure the error made in estimating the softening stress-strain curve of a given experiment. Doing so would increase the impact of the paper and make it more pedagogic.

The effect of thickness of localisation band on macro response, while keeping all other parameters of the model fixed, is illustrated in Fig. 13 of the original manuscript. This is just for illustration purpose as it is not correct in our opinion given the behaviour of the band and its thickness in the model should be coupled to reproduce the experimentally obtained dissipation (as area under load-displacement curve). The readers can refer to Le et al 2022 (https://doi.org/10.1680/jgeot.20.P.105) for the analysis of experimental data showing such coupling effects. We have added a sub-section on calibration of model parameters to address this comment, but do not want to include the suggested parametric study to avoid misleading information.

Minor edits:
8: "Therefore, formulations of constitutive models for responses inside and outside the localisation band can following standard procedures?" should be "Therefore, formulations of constitutive models for responses inside and outside the localisation band can follow standard procedures?"
13: "it is pointless to have a perfect match at this stage without correct underlying correct failure/deformation mechanism" should be "it is pointless to have a perfect match at this stage without correct underlying failure/deformation mechanism"
21: "the two-scale model can match that by FEA using the same core damage-plasticity model enhanced with rate-dependent." should be "the two-scale model can match that by FEA using the same core damage-plasticity model enhanced with rate-dependence".

The reviewer's attention to details and suggestions are appreciated. We have fixed all typos listed and proof read the manuscript too.

Review Round 2

Reviewer 1 (Prof. Pietruszczak)

The authors' responded to all points raised in the original review and revised the manuscript. This is acknowledged and appreciated. Although several issues have been addressed, some concerns raised in the original assessment still remain.

It is recognized that many technical journals accept 'review articles' that provide a comprehensive summary of research on a certain topic. However, the current paper does not belong to this category; it is a restatement of authors' own work already published, and there is no visible novelty/contribution. The computational procedure of adjusting the thickness 'h', together with somewhat arbitrary choice of numerous independent parameters describing the properties inside the 'shear band', does not have a clear physical basis and is open to questions. Other issues, e.g. the use of point integration in simulation of 'triaxial' tests in the presence of localization, are also debatable. Will the results be the same if a small imperfection is introduced and the problem is analyzed as a BVP?

Thus, in spite of authors' visible effort to clarify some points, there are still doubts regarding the acceptance of this submission.

Reviewer 2 (Prof. Arson)

I have read the revised paper and the response to the review comments. To my opinion, the authors have sufficiently addressed all the comments. The few rebuttals that the authors wrote are reasonable and the vast majority of the comments were fully addressed via edits in the paper. I think that the additions made improve the paper and I am comfortable accepting it as is.

Small typo: after equation 7 of the revised version: "where Cij^1 and Cij^1 are two constraints to be used in the energy and dissipation potentials, respectively" should be "where Cij^1 and Cij^2 are two constraints to be used in the energy and dissipation potentials, respectively"

Author Response

We thank the editors for facilitating the review process and the reviewers for constructive comments. In the electronic copy of the modified paper, the principal changes are highlighted in blue. Given both reviewers have already figured out the authors' identity through references to earlier publications, we have included our names in this response to their comments.

Reviewer #1

It is recognized that many technical journals accept 'review articles' that provide a comprehensive summary of research on a certain topic. However, the current paper does not belong to this category; it is a restatement of authors' own work already published, and there is no visible novelty/contribution.

We have provided detailed responses to this comment in the previous revision. Some key points are repeated here. The new contributions (not published before) include:

- The thermodynamic approach (Section 2.2) for the case of a finite thickness band based on the use of constraints that allows straightforward adaptation of procedures established in Houlsby and Puzrin (2000) for localised failure.
- The assessment of the performance of the proposed model against detailed FEM modelling of a triaxial test (Section 5). In short, it is the performance of an integration point against a type of direct numerical simulation. This is to illustrate how classical constitutive models should be enriched to describe correctly both pre- and post-localisation behaviour, and how such a model performs against detailed FEM modelling of triaxial tests. As addressed in the introduction, such a model is essential for large scale simulation, when the element size (or particle size) should be much larger than the thickness of the localisation band contained in it.

The computational procedure of adjusting the thickness 'h', together with somewhat arbitrary choice of numerous independent parameters describing the properties inside the 'shear band', does not have a clear physical basis and is open to questions.

We have provided detailed explanations to this in the previous revision, including experimental evidence on the evolving thickness of the localisation band, and challenges associated with it in constitutive modelling. In brief, it was specifically addressed in Section 5.1 that the proposed approach in this paper, using a fixed value of localisation band thickness, is a simplification of the observed physical phemomena (evolving thickness of the band). Nevertheless it is a step further from classical constutive models that does not possess any details on the localisation band. In this sense, the proposed approach allows us to carry out both analysis of localised failure at constitutive (material/integration point) level and interpretation of experimental data on localised failure in triaxial tests in a more physically meaningful way. It also reflects our opinion that constitutive models "should be simple but not simpler".

Other issues, e.g. the use of point integration in simulation of 'triaxial' tests in the presence of localization, are also debatable. Will the results be the same if a small imperfection is introduced and the problem is analyzed as a BVP? Thus, in spite of authors' visible effort to clarify some points, there are still doubts regarding the acceptance of this submission..

Our integration point is represented by the proposed model that possesses essential details on the localisation band and the volume containing it. These details include relative size between the localisation band thickness the size of this volume, orientation of the band, and different responses inside and outside the band. Therefore it is able to describe both pre- and post-localisation behaviour in a way more physically meaningful than classical continuum models. In the analysis of BVP in Section 5, a weak element was introduced at about the centre of the specimen (Mir, 2017; we have added that information in Section 5.2 in this revision), and performance of the proposed approach (integration point level) is assessed against this analysis of BVP, as seen in the original manuscript. If the reviewer in fact asked about the use of the proposed approach in the analysis of BVPs, our earlier publications listed at the end of the first paragraph of Section 5 provide an answer to this question: the results from analysis of different BVPs with inhomogeneous stress fields, using both mesh-based and mesh-free methods, have been assessed against a range of experimental data.

Fig. 12 in the original manuscript is used to illustrate the mechanisms behind the "eye pleasing" macro responses that any constitutive models can produce. This point is clearly stated in our conclusion in the original manuscript that fitting cannot be avoidable in constitutive modelling, but its use should be after having correct mechanisms of localised failure in the structure of constitutive models. In this sense, the proposed approach provides a computationally efficient, but physically reasonable, way to tackle localised failure at material point level. As clearly stated in the manuscript and conclusion, it is not the best way (yet) to incorporate mechanisms behind localised failure in constitutive models, given there are several outstanding issues, all openly discussed, to be addressed in future work.

Reviewer #2

I have read the revised paper and the response to the review comments. To my opinion, the authors have sufficiently addressed all the comments. The few rebuttals that the authors wrote are reasonable and the vast majority of the comments were fully addressed via edits in the paper. I think that the additions made improve the paper and I am comfortable accepting it as is.

Small typo: after equation 7 of the revised version: "where Cij^1 and Cij^1 are two constraints to be used in the energy and dissipation potentials, respectively" should be "where Cij^1 and Cij^2 are two constraints to be used in the energy and dissipation potentials, respectively".

We thank the reviewer for constructive comments and for spotting this typo. It was corrected in this revision.