Review of “A damage criterion based on energy balance for an isotropic cohesive zone model”

André Chrysochoos, Loïc Daridon, Mathieu Renouf, Jean-Jacques Marigo, Laurence Brassart

Open Review
Journal of Theoretical, Computational and Applied Mechanics

Identifiers
Open Review
OAI hal-03482933

Reviewed Article
DOI 10.46298/jtcam.7056

History
Review Mar 23, 2021
Rebuttal May 25, 2021
Review 2 Jul 28, 2021
Rebuttal 2 Sep 15, 2021

Review of "A damage criterion based on energy balance for an isotropic cohesive zone model"

André Chrysochoos[1,2], Loic Daridon[1,2], Mathieu Renouf[1,2], Jean-Jacques Marigo[3,R], and Laurence Brassart[4,E]

[1] LMGC, Université de Montpellier, CNRS, Montpellier, France
[2] MIST, Université de Montpellier, IRSN, CNRS, France
[3] Laboratoire de Mécanique des Solides, École Polytechnique, Route de Saclay, Palaiseau, France
[4] Department of Engineering Science, University of Oxford, Oxford, United Kingdom
[R] Reviewer
[E] Editor

Review of version 1

Permalink: hal-03098095v1

Authors Dear Editor and Reviewers,

First of all, we would like to thank you for the celerity with which our paper proposal was reviewed. We propose hereafter to systematically take up the different remarks, questions, suggestions that you formulated and to specify how this has been considered in the 2nd version of our paper.

Editor

Editor The reports are quite contrasted. Reviewer 2 is extremely positive, while Reviewer 1 and Reviewer 3 are much more critical. Both negative reports points at a (perceived) lack of originality and significance, while all reviewers agree on the scientific quality.

Authors Indeed, despite the positive nature of the comments made by Reviewer 2, it seems that the originality of the approach taken to establish a cohesive zone model has not been captured. We try to point it out in this revised version.

Editor I am willing to consider a revised version of the manuscript, however both issues of novelty and significance must be convincingly addressed (in addition to the specific points raised by the reviewers). This should probably require significant modifications (not necessarily limited) to the Introduction and Conclusion, in order to:

- demonstrate the novelty of the proposed formulation as compared to the state of the art (including previous works by the authors).
- highlight the benefits and significance of the proposed approach, compared for example to classical formulations based on traction-separation laws.

There is a brief mention of the benefit of an energy approach in view of experimental calibration based on temperature measurements in the conclusion, but this point should probably be emphasized and further developed. Other potential benefits (computational? Future extensions?) should also be clearly indicated, as appropriate.

Authors Duly noted. These two items seem to us to be closely related. In the revised version, the confrontation of our approach with the one developed for a class of cohesive zone models using a thermomechanical framework has been strengthened. These complements were placed in

Subsection 2.3.4. We wrote in blue these complements. We come back to the interest of having a consistent energy approach not only with respect to the 2nd principle of Thermodynamics but also with respect to the energy balance form (1st principle). The editor is perfectly right when she assumes that we plan to perform damage/rupture tests on ceramic samples and to estimate during loading the thermal and calorimetric effects induced by the deformation process via an infrared camera and quantitative image processing [Proper orthogonal decomposition preprocessing of infrared images to rapidly assess stress-induced heat source fields, Adil Benaarbia and André Chrysochoos]. The dissipated energy fields experimentally constructed will then be compared to the simulated ones (as those shown in Fig. 7). We have mentioned these experimental aspects at the end of the introduction and we evoked future developments in the prospects of the paper (last section).

Editor I would also ask the authors to consider the following points in their revised version:

Editor 2D models are significant from the theoretical point of view, but they are limited for practical applications. A comment on the extensibility of the model to 3D must be included.

Authors It is likely that there was a misunderstanding at this level. We had mistakenly written in the title of Section 3 “2D cohesive zone model”. It is indeed a 3D model.

Editor The reference list is way too long (74 references for a research paper gives the impression that nothing has been left to say), and I would remove all the ones that are not necessary to support the discussion, so only the very original contributions must be cited (in particular in relation to the novelty of the present paper).

Authors Indeed, we wanted to make a broad overview of what is done to manage damage mechanisms in mechanics of materials. We thought, obviously wrongly, that the positioning of our approach would be more obvious. We have reduced the number of citations in the new version.

Editor Refs [33] and [34] are wrong, the author last name is Alfano, not Giulio.

Authors This has been fixed.

Editor Given that the model has been implemented in an open-source platform, would the authors consider making their code available to the community?

Authors Of course, the open-source platform used here was is LMGC90 which is developed in our laboratory. LMGC90 is a free and open source software dedicated to multiple physics simulation of discrete material and structures. The digital work proposed here makes well in this policy.

Editor Please correct the following typos:

- p.20, lines 357-358: fix the symbols in the in-line formulas: u_d, K_n
- p.23, line 391: remove “of”
- p.33, caption of fig. 13: check “W constant”

Authors All fixed.

Editor p.25, line 435: “6723 meshes”?

Authors This is the number of T3 elements used to discretize the W_1 domain (see Fig. 6). We changed the formulation.

Reviewer #1 (Anonymous)

Reviewer This paper presents a cohesive zone formulation from thermodynamic perspective. The method is well explained with extensive overview of literature and informative illustrations. However, I do not see sufficient scientific novelty in the work to recommend it for publication in JTCAM. There is no substantial difference in the proposed formulation with respect to other cohesive laws that have been proposed in the past 25 years. The introduction and conclusions of the paper also do not provide a clear argumentation for how the present work addresses a gap in scientific understanding. A single numerical example is presented, which is an interesting case but not out of reach for existing formulations.

Authors What is the scientific novelty? It seems that this is indeed the tricky part of that work.

1. The paper proposes to describe damage progress in elastic cohesive zones with an energy criterion: we consider the evolution of the “maximum storable elastic energy for a given damage stage” to predict the evolution of damage. The use of an energy criterion allows us to easily consider multidimensional damage models such as CZM in this paper or volume bulk damage models future developments).
2. The paper wants also to underline that the data of a traction-separation curve is equivalent to the data of the this “maximum storable elastic energy” which itself, automatically, sets the damage evolution, the damage mechanism being supposed to be the only and exclusively dissipative mechanism. In other words, traction-separation law, free energy and damage evolution are closely linked.

To set ideas, let’s consider the following simple case: let $F(u)$ be a 1D traction-separation law. We suppose that the elastic energy is, as often, written as

$$\psi(u, D) = (1 - D)\frac{K_0 u^2}{2} \tag{1}$$

where D is the isotropic damage variable, u the displacement jump, and K_0 the (constant) elastic stiffness of the virgin cohesive zone. We consider a monotonic loading. The deformation energy rate is given, by definition

$$(W_{\text{def}})^0 = F(u)\dot{u} \tag{2}$$

where $F(u)$ follows the traction-separation curve. The elastic energy rate can be split into two parts

$$(W_{\text{e}})^0 = (1 - D)K_0\dot{u} - K_0\frac{u^2}{2}\dot{D}. \tag{3}$$

If we assume now that the damage is the only irreversible mechanism, then the traction force is the conjugate variable of the displacement jump, where

$$F = \frac{\mathrm{d}\psi}{\mathrm{d}u} = (1 - D)K_0 u \tag{4}$$

and

$$(W_{\text{e}})^0 = (W_{\text{def}})^0 - K_0\frac{u^2}{2}\dot{D}. \tag{5}$$

If now the damage is supposed to be exclusively dissipative (no internal stored energy), then the dissipation is

$$(W_{\text{d}})^0 = K_0\frac{u^2}{2}\dot{D} = (W_{\text{def}})^0 - W_{\text{e}})^0 \tag{6}$$

Following the traction-separation curve, the damage evolution has to verify

$$\dot{D} = 2\frac{[(W_{\text{def}})^0 - W_{\text{e}})^0]}{K_0 u^2}. \tag{7}$$

Noting that for each current point $(u, F(u))$ of the traction separation curve, we have $w_{\text{e}} = 1/2F(u) \times u$, the time differentiation, following the curve, reads

$$(W_{\text{e}})^0 = \frac{1}{2}F\dot{u} + \frac{1}{2}\dot{F}u. \tag{8}$$

Then,

$$\dot{D} = 2\frac{[F\dot{u} - (\frac{1}{2}F\dot{u} + \frac{1}{2}\dot{F}u)]}{K_0 u^2} = \frac{\dot{u} - \dot{F}u}{K_0 u^2} = \frac{AF^m}{(1 - D)^p} = A\left(\frac{K_0 u}{F}\right)^p F^{(m-p)} \tag{9}$$

and

$$W_e(U, U_d) < W_e^d(U_d). \tag{10}$$

Note that the right-hand member of this equation is fully determined by the traction-separation curve. Any form of damage evolution law, incompatible with this previous equation, would lead to an energy balance form incompatible with the initial energy assumptions (i.e. form of the free energy, damage unique and exclusive dissipative mechanism). This trivial but heuristic example has been introduced in Subsection 2.3.4.

Reviewer An additional comment is that after reading the paper it is not clear to me what is meant with the "criterion" in the title, introduction and conclusions. I would find it more logical to use "formulation" instead of "criterion". Also in Figure 5, which is presented as an "Illustration of the damage energy criterion", I cannot find a criterion. The graph is relevant, it shows the evolution of different energy quantities discussed in the paper, but no "criterion" as something that can be used to make a decision.

Authors We were a little surprised by this remark. The lack of clarity in our presentation is perhaps due to the fact that we wanted to give both an energy form to this criterion and a more classical form, involving the thermodynamic force associated with the damage (as it is classically done in Thermodynamics of Irreversible Processes). For the 1D scenario the energy criterion is given in Eq. (17). It can be stated as follows: if the elastic energy is equal to the "maximum storable elastic energy", and if it remains on this threshold between t and $t + dt$, damage progresses. It is the same approach as the one followed in plasticity, except that here, the threshold surface is not set by hardening properties, it is given by the form of the free energy (taken at its maximum value for a given damage state). To get an equivalent formulation of this criterion, involving the thermodynamic forces associated with the damage, we introduced a differential form of the energy balance (Eq. (22)). For the 3D version of the CZM, the energy criterion has naturally the same form (see for instance Eq. (28)).

Reviewer #2 (Jean-Jacques Marigo)

Reviewer The paper is a comprehensive presentation of the construction of cohesive force models in the framework of generalized standard materials. The paper is really well written, the choices are well argumented with a good knowledge of the literature. Accordingly, I strongly suggest that the paper be published. I have only two remarks which do not require that the paper be changed, but that can be considered by the authors for future works:

1. They choose a particular condition of irreversibility for the cohesive model. There exist other choices in the literature which should be compared to their because it turns out that these choices are crucial for fatigue behavior.
2. It would be interesting to study at which type of fatigue law their model leads. For instance, can one obtain Paris laws of fatigue?

Authors Warm thanks for the encouraging assessment of our paper and for these comments. Regarding fatigue damage, we think that the present model is not satisfactory. In this model, the cohesive zone behaves elastically as long as the thermodynamic states verify $w_e(u, u_d) < w_e^d(u_d)$. In fatigue, all loadings have to be considered as irreversible processes. We are currently working on an extension of this type of model based on energy analysis of the behavior that can be applied to high cycle fatigue but also to situations where the cohesive zone behaves anisotropically from an elastic standpoint.

Reviewer #3 (Anonymous)

Reviewer The manuscript "A damage energy criterion for cohesive zone model" by Chrysochoos et al. presents certain energetic considerations with respect to the formulation of traction-separation laws. The chosen cohesive zone model is applied to simulate crack propagation under mixed-mode loading. The study is profound, but to my opinion with a rather low level of novelty.

Authors That's right, it's the same impression as the one of Reviewer 1. We hope that the arguments developed above will also convince Reviewer 3.

Reviewer Comments in detail:

Reviewer p.2: Also many other sophisticated approaches than 1-d, in particular for ductile damage

Authors The reviewer is right, we only focused on cohesive zone models where damage is the one and only irreversible process.

Reviewer p.2: CZM is not inevitably “2D” but can also be a line (=1D) in plane models

Authors That’s perfectly right, it is exactly the case of the 1D version of the model we present (see 1D scenario).

Reviewer p.3: CZM can also embedded in inelastic bulk material

Authors Effectively elastic CZM at the edge of each finite element can be associated with any inelastic behavior to describe the bulk material in each element.

Reviewer p.4 “cohesive zone methods” do not have inherently a “relative dependency on mesh size”. If only a single layer of CZM is employed at a known path of crack propagation, a clear mesh convergence is observed. The problems arise if and only if cohesive zone elements are placed everywhere. Though, certain approaches have been proposed to overcome the problem even under these conditions.

Authors It is what is done in our numerical test, cohesive interfaces are placed between each mesh of the fine zone to allow different crack paths. Our objective is to consider the natural situation where the crack path is a priori unknown.

Reviewer p.4: It’s not clear here whether the discussion comprises “nonlocal” CZM or nonlocal damage models.

Authors Our model is not nonlocal contrary to those we mention based on the level set methods. We agree, they are more nonlocal damage models than nonlocal cohesive models.

Reviewer “CZM is performed under the small strain”: a CZM does not know any “strains”, but only separations.

Authors The reviewer is absolutely right. We have eliminated this unfortunate sentence in the 2nd version of our paper.

Reviewer Eqs. (1) and (3) can be converted to each other equivalently (and also into u_d). The main point is how the D’s enter the constitutive equation.

Authors We are well aware of this. In this model, the damage variable impacts the stiffness of the cohesive zone and its evolution is set by the ability of the material to store elastic energy. This ability is fixed by the shape of the tension-separation curve (see responses to reviewer 1).

Reviewer (7) and (8) are no “energies” but the work. It remains to show that this integral is path independent so that the work would correspond to an energy (as potential).

Authors We are also fully aware of the unfortunate habit of using the word energy instead of work. For proof we refer the reviewer to the comments we made after writing the Clausius-Duhem inequality (Eq. (14)). It remains that the literature in general and famous papers in particular, often talk about dissipated energy, stored energy and energy balance... instead of dissipated and stored works and work balance.

Reviewer p.14: It is no “fact” that “$\dot{u}_\mathrm{d} \geqslant 0$ implies $K'(u_\mathrm{d}) \leqslant 0$”. Rather, this is a restriction on the choice of the evolution equation (4) of u_d, of which (19) is just the rate form in form of a Kuhn-Tucker condition.

Authors We probably misspoke at this level. We simply wanted to point out that the very definition of the state variable chosen to describe the damage imposes, by construction, a monotonic evolution, and therefore, because of the Clausius-Duhem inequality (i.e. positivity of the dissipation), the stiffness of the material can only decrease as damage progresses. It seemed to us that this property was reassuring from a physical standpoint, because it was in conformity with the idea that one has of the effect of damage on the elastic stiffness.

Reviewer p.15: In the context of Onsager relation and GSM, some words might be said about rate dependency.

Authors We are not sure we understand what the reviewer wishes here. The main difference between what is proposed by the Onsager linear TIP framework and the GSM framework, comes from the way the correspondence between thermodynamic forces and state variable fluxes is constructed. In the first case, as its name indicates, the correspondence between forces and fluxes is linear and the matrix linking them is assumed to be symmetric definite and positive, which ensures that the 2nd principle (Clausius-Duhem) is not violated. In the GSM framework, a non-linear or even a non-regular correspondence (e.g. plasticity) can exist between thermodynamic forces and fluxes of state variables. To ensure the fundamental inequality, forces are derived from a flux potential (dissipation potential) or reciprocally the fluxes from a force potential (dual dissipation potential), these potentials having the right mathematical properties (convexity, positivity, minimized and null in zero). Both formalisms naturally allow to introduce time effects (rate dependence). In the present work, the damageable elastic material considered is not rate sensitive. This is the reason why this rate dependence was not evoked.

Reviewer "With the chosen definition of the damage variable given in Eq. (4), we derive an evolution equation given in Eq. (18)": If Eq. (19) is meant, then both Eqs. (4) and (19) are exactly identical. Integration of (19): why are cohesive zones necessary at all for the benchmark if an initial crack is present in a brittle material (i.e. without plasticity? I guess that conventional fracture mechanics would give results of similar quality. A benchmark for CZM should incorporate the transition from crack nucleation to crack growth.

Authors A new numerical simulation of a Brazilian fracture test has been performed and added to the article. In this new case, no initial crack has been introduced. This numerical study will be associated with experimental test in a near future. Full-field measurements using IR and visible cameras will allow us to construct dissipation energy fields.

Review of version 2

Permalink: hal-03098095v2

Editor

While Reviewer 3 now gives a positive recommendation, Reviewer 1 still raises the question of novelty and significance, see detailed comments below.

I have to concur with Reviewer 1 on this. In particular, it is a bit disappointing that the authors have not followed my recommendation to explicitly address the important questions of novelty and significance relative to the state of the art in the Introduction. While Section 2.3.4 does provide some helpful clarifications, it still falls short of demonstrating the novelty and significance of the proposed formulation as compared to the extensive body of work in this field, and in particular other thermodynamics-based CZM formulations (only three relevant papers are cited with no details given).

Therefore, I would invite the authors to submit a revised version focusing on novelty and significance in relation to the state of the art. This should involve a complete rewriting of the introduction, to include:

1. a suitable literature review focusing on CZM models that are based on thermodynamics. As the review article by Kuna et al (2015) shows, there have been numerous attempts at formulating CZMs based on the evolution of a damage parameter in the last 20 years, including methods written within the TIP framework. These need to be acknowledged.
2. the identification of gaps in this literature that the present work specifically addresses.
3. a clarification of the extent to which the present formulation differs from these other TIP-based formulations. If the difference is mainly formal and essentially leads to the same model, this needs to be acknowledged. If, on the other hand, the present work brings in significant differences or benefits as compared to other similar TIP-based formulations, it should also be clarified.
4. removing literature review that is not directly relevant to this work, especially the paragraphs on

mesh-size dependency and regularisation methods, which are not the focus of the work.

Of course, other modifications throughout the text are welcome if they make the points of novelty and significance stronger. But I feel that making these points clear in the introduction is necessary.

Reviewer #1 (Anonymous)

Reviewer The paper has been enriched with an additional numerical example, which is a significant improvement because having only one numerical example was a weakness of the original manuscript. However, the main point of criticism about novelty and significance of the work has not been answered sufficiently. In the answer by the authors, a list of 2 points is given. I observe that this list just describes once more what has been done in the paper, not how it is a significant addition to the already the mature field of cohesive zone modeling. I understand that the paper provides a new perspective on cohesive formulations, but I do not see what that new perspective adds to the understanding of the physics of fracture, or to the capability to model fracture realistically. In the revised paper, major changes are not in the introduction and conclusions, but rather in the technical description of section 2.

Authors We propose in this new revision of our paper a complete modification of the introduction by focusing on thermodynamic approaches to cohesive zone models. Then we eliminate from the introduction all other generalities about cohesive zones that are not directly related to thermodynamic notions. For this purpose a synthesis of a dozen papers dealing with thermodynamics of CZM will be proposed which will allow us to better position our approach in relation to what is generally done. In our new bibliographic review, we think to evoke, among others, the works of Constanzo et al. (1995), Chandrakanth et al. (1995), Bouvard et al. (2008), Roe et al. (2002), Evengelista et al. (2013), Alfano et al (2013-2017), etc. The objective of the synthesis will be to precisely show how what we propose in this paper with respect to the introduction of damage kinetics, differs from what has been done so far. If you would like us to include certain papers in this synthesis, please let us know.

Reviewer #2 (Jean-Jacques Marigo)

Reviewer I was highly favorable to the publication of the paper in its first version. Since the authors have given satisfactory answers to my questions, my opinion is reinforced and I strongly recommend its publication.

Reviewer #3 (Anonymous)

Reviewer The authors have revised their manuscript carefully and added a very illustrative and convincing benchmark case. I recommend to publish the manuscript.

Review of version 3

Permalink: hal-03098095v3

Reviewer #1 (Anonymous)

Reviewer I have read the paper in detail one more time. I think the story behind the work is more clearly outlined now. The paper provides an original perspective on cohesive modeling, which is potentially a stepping stone towards a new approach to model identification. The paper can be accepted for publication in JTCAM provided that the following points are addressed to further improve clarity of the manuscript.

- line 56, "the energy properties", it is not obvious what is meant with this, can the authors try to find a more meaningful expression?
- line 60 "to perform local energy balances", I suggest to rephrase this as "to evaluate the local energy balance"
- line 129. Here the maximum displacement u_d is introduced as "damage variable". The same term is used elsewhere. I find this somewhat confusing, because the same term has also been used for the stiffness loss D_k, in Equation (1). For u_d I would find the term "state variable" more logical. It is a variable that defines the state and the state corresponds to a certain amount of damage. Note

that in the conclusions, u_d is referred to as “damage parameter”

- line 182. “A first gambling of the thermomechanical approach is to assume”, I suggest to rephrase this as “The thermomechanical approach starts with the assumption of”
- line 213. “In what follows in as much as our approach”. Phrase unclear.
- in Section 2.3.4 a superscript 0 is used $(w_{def})^0$ without being introduced. It appears that it is used to indicate a rate. In other places a dot is used (e.g. Eq. (19)), which is more common. I suggest to use a dot for all rate quantities.

Authors We have taken into account all the remarks proposed by the reviewer.

Editor’s assessment (Laurence Brassart)

The paper presents a formulation of cohesive zone model within the framework of irreversible thermodynamics. In the proposed approach, the damage evolution law is obtained from the evolution of maximum recoverable energy, rather than a-priori prescribed by a traction-separation law. One practical interest of the formulation is that it enables parameter identification based on temperature measurements. From a theoretical viewpoint, the formalism lends itself to further generalizations, including accounting for thermomechanical coupling, energy storage associated with damage (e.g. due to microstructural change), or anisotropic damage.

Three reviewers assessed the manuscript. While all reviewers agreed on the scientific quality of the work, Reviewers 1 and 3 pointed at a perceived lack of novelty of the work with respect to the state of the art. This concern was satisfyingly addressed in the revised introduction. An additional numerical benchmark (Brazilian fracture test) was also added. The paper was finally recommended for publication by all reviewers after three rounds of revisions.