Answers to Reviewer #2

Ms. Ref. No.: JOMP-D-15-00204
Title: "Viscous Regularization for the Non-equilibrium Seven-Equation Two-Phase Flow Model',
Springer Journal of Scientific Computing

After the recent work by Guermond and Popov where a general class of viscous regularizations of compressible Euler equations is investigated, the present work proposes an extension in the case of the non-equilibrium seven-equation two- phase flow model. To address such an issue, the authors introduce a general viscosity within the adopted model. Then, they derive the entropy evolution law, now perturbed by the additional viscosity. By adopting a relevant definition of the additional viscosity terms, the authors claim that a minimum entropy principle is satisfied. This study is completed by a chapman-Enskog extension to get the associated five-equation model in the limit of infinite relaxation coefficients. The analysis is achieved by considering the incompressible regime governed by low Mach number. The authors claim that the adopted viscosity regularization does not modify the required incompressible regime. Finally, numerical illustrations are displayed in order to attempt to illustrate the relevance of the viscosity regularizations.

My opinion about this work is not good at all since this paper looks like a poor extension of the work by Guermond and Popov.

We disagree with your assessment. This paper deals with a regularization of a two-pressure and two-velocity two-phase flow model of the Baer and Nunziato type. These models are strictly hyperbolic non-conservative system of equations (HNCSE); hyperbolicity being an important property with respect to causality principle. However, this type of system involves many difficulties for the derivation of numerical methods, often rendering the Riemann problem determination complex due to the 7 waves presents (in 1D, more in multi-D). We have provided an alternative numerical method, based on an artificial viscosity principle, and we believe this is a valuable alternative for the solution of such hyperbolic systems. As such, declaring that our work is a poor extension of the work by Guermond and Popov is not correct. Furthermore, in the revised manuscript, we clarify many aspects related to hyperbolic non-conservative system of equations and use an entropy condition following the theory developed by Del Maso-Le Floch-Mariat (DLM). The DLM theory extends the classical definitions of a weak solution and the entropy solution (see paragraphs 2 & 3 of the introduction and the new section 3.1.)

1. The main point of this work concerns the derivation of the minimum entropy principle. Here, the establishment of this property is not clear at all. I think that the proof is incomplete. For instance, Guermond and Popov need (and prove) the positiveness of the density. I think that the positiveness of

partial density is here needed but no proof is given. Moreover, I am convinced that $\alpha_k \in [0,1]$ is also necessary and must be proved. I urge the author to read carefully the paper by Guermond and Popov and reconsider the establishment of their results.

We did read the paper by Guermond and Popov. We agree with you that proof of the positiveness of the partial density was missing and we added it (section 3.4). We also performed the same work for the volume fraction equation. In both cases, the viscous terms are required in order to prove positiveness of the partial density and to show that the volume fraction remains bounded within the interval [0,1].

2. Several times, thew author speak about uniqueness of the numerical solution . I dont understand the meaning of these words. Moreover, this paper does not contains numerical derivations. Page 7, the authors refer to Leveque (pages 27-28 in Numerical Methods for Conservation Laws), but these two pages in the leveques book coincides with the introduction of weak solutions and entropy inequalities. Nothing about uniqueness of the numerical solution.

We meant 'convergence of the numerical solution to the weak entropy solution' and not 'uniqueness of the numerical solution'. We corrected it throughout the paper. We also removed the reference to Leveque's book that is not the most appropriate here as you noticed and replaced it with a more extensive paragraph in section 3.1 that refers to the DLM theory (section 3.1).

3. The numerical schemes, used to get the numerical illustrations, are not specified. However, the derivation of a numerical scheme to approximate the weak solution of the model under consideration is a very difficult task.

A short paragraph explaining our space/time discretization was present but we unfortunately deleted it by mistake in the version that was submitted. We apologize for this. It has been added back (first portion of section 5). All simulations are carried out using continuous finite elements.

4. The presentation of the entropy residual is absolutely not relevant. In section 3.2, I understand the opportunity to omit the underlined terms. However, the equation (15) turns out to be wrong. The authors have to introduce a specific notation to designate the entropy residual.

We disagree. The sign of the entropy equation, which is function of the entropy residual, is used as an entropy condition for non-conservative and conservative system of equations in order to ensure convergence of the numerical solution to a weak entropy solution [15, 19]. We double checked equation (15) and believe it is right. It is used in section 3.4 to show that the entropy residual remains positive with the proper viscous regularization. We added a specific notation to designate the entropy residual, as you requested, to readability.

Reviewer #2: First of all, I thank the authors for their substantial revision which highlight their motivations. I am septic about the real interest of this viscous regularization, but I understand it is suitably designed to recover some important properties. Moreover, as underlined in the introduction, this viscosity is an essential tool to define the shock solutions. I am disappointed since shock solutions are simulated but not studied. I recall that the addition of relevant viscosity is not enough to guaranty the correct capture of the shock solutions. Indeed, the numerical viscosity may seriously perturbe the artificial viscosity. As a consequence, a very particular attention must be paid on the numerical scheme which is not done here. About such a difficulty the authors may read the papers :
Abgrall, Karni, JCP, 2010
Berthon, Coquel, Math. Comput., 2007
In this sense, i don't understand why the simulations are relevant. I urge the authors to give some comments