Answers to Reviewer #1

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

Viscous regularization of hyperbolic conservation laws is a meaningful approach to stabilize the numerical discretization of such problems. In the present manuscript the authors derive a viscous regularization for a two-phase model following previous work by Guermond and Popov for a single-phase fluid. The basic idea is to design regularization terms such that the entropy residuals (corresponding to the physical entropy and Harten's generalized entropies) are positive. For scalar problems in conservative form it can be proven that this ensures uniqueness of the weak solution. Therefore, positivity of the entropy residuals is considered a useful property also for systems.

The subject can be considered very interesting and relevant. The presentation is clear and the analysis is sound. However, some issues are not properly addressed and the presentation of the numerical results needs to be improved before I can recommend this paper for publication.

Thank you for your very thorough review.

1. The hyperbolic system (1) corresponding to the seven-equation two-phase ow model is not in conservative form because of the gradients $\nabla \alpha_k$ in (1a), (1c) and (1d). Therefore classical results from the theory of conservation laws cannot be directly applied to the present system. Based on the DLM theory [2] the classical definitions of a weak solution and the entropy solution, respectively, can be extended for quasi- conservative problems introducing a path, see [3, 4, 2]. Existence of a solution to a problem in non-conservative form has been investigated in [5]. In [6] existence of a solution to the Riemann problem has been verified. In particular, shock waves are influenced by the path since it enters the generalized Rankine-Hugoniot jump conditions, see [4]. Note that rarefaction waves and contact discontinuities are not affected by the choice of the path. It would be interesting for the reader to know what notion of weak solution and entropy solution is applied here.

The problem under consideration is indeed a nonlinear hyperbolic system in non-conservative form. We have added a paragraph reviewing previous work dealing with weak solution and entropy condition for non-conservative system of equations in the introduction (see the second and third paragraphs of the introduction). We also modified the section 3.1 entitled *Methodology* accordingly.

We thank you for your insightful review. The following references have been added: [1], [2], [3], [4] and [5]. (numbers refer to the list of references given below, in our reply letter).

2. In the abstract, see page 1, line 20, the authors claim that regularization ensures uniqueness of the weak solution. No evidence is given in the manuscript, i.e., neither a reference nor a proof, that justifies this statement. For scalar problems in conservative form it is well-known that uniqueness is ensured if an entropy inequality holds for all entropy-entropy flux pairs. However, the problem at hand is not in conservative form. See also the first remark.

Our wording was incorrect and we have corrected it: we meant to state that the viscous regularization ensures convergence of the numerical solution to a weak (minimum-entropy satisfying) solution and not uniqueness of the weak solution. See also our answer to your first remark.

3. In (3) the authors give a particular choice for the interfacial pressure and velocity. It should be mentioned that other choices are considered in the literature for which the second law of thermodynamics can be verified, see e.g. [7].

The definitions of the interfacial pressure and velocity are not unique, as you rightfully mentioned. We added References 8 and 30 with further examples of the interfacial variables. We now emphasize that the theoretical results devised hold as long as the choice of the interfacial variables preserve the entropy inequality (see paragraph between Equations (3d) and (4a)).

4. On page 5, l. 34-37, it should be mentioned that hyperbolicity also requires a full set of linearly independent (left/right) eigenvectors. For the considered system this only holds true if the non-resonance condition is satisfied, see [1].

We fully agree with you and modified the text accordingly (see text below Eq. (7)).

5. In view of my first remark it would be interesting to know whether the field associated to the eigenvalue λ_{3+dim} in (6) is linearly degenerated. In this case the jump in α corresponds to a contact discontinuity.

We have looked into this. In older publications dealing with the 7-equation model, reasonable estimates for the interfacial velocity (e.g., $u_{int} = \frac{\alpha_g \rho_g u_g + \alpha_l \rho_l u_l}{\alpha_g \rho_g + \alpha_l \rho_l}$) have led to a proof that the field associated with $\lambda_{3+dim} = u_{int}$ is indeed linear degenerate ($\nabla \lambda_{3+dim} \cdot r_{3+dim=0}$); see T. Gallouet and J-M Herard, "Numerical modeling of two-phase flows under the two-fluid two-pressure approach", *Math. Models and Meth. in Appl. Sci.*, Vol.14, No.5 (2004). With the more recent definitions of the interfacial velocity (i.e., $u_{int} = \frac{Z_g u_g + Z_l u_l}{Z_g + Z_l}$, with $Z_k = \rho_k c_k$), our derivations were inconclusive (although we think it is possible that this property still holds).

6. For the numerical computations in Section 5.2 the stiffened gas law (32) has to be modified to account for the heat of formation q_k.

Thank you for pointing out this. It was a typo. We fully agree with you and have corrected it (the mistake also occurred at another location in the text).

7. For both computations presented in Section 5 the authors should provide more information on the discretization, e.g., the type of scheme (FVM/DG, explicit/implicit) and its main ingredients. In particular, the discretization of the non-conservative terms in (1) is of interest. Furthermore, the spatial/temporal dicretization and the CFL number should be mentioned.

We had forgotten to give a short paragraph explaining our space/time discretization. Actually, it was present in a previous version of the manuscript and we removed it by mistake! It has been added now (see paragraph at the beginning of Section 5). All simulations are carried out using continuous finite elements.

8. For both test cases it would be interesting to investigate grid convergence because the chosen regularization terms are grid-dependent and vanish with increasing resolution, i.e., stabilization by viscous regularization is successively reduced. In particular, the grid convergence study is recommended for the shock tube problem because the numerical results show a severe smearing for both the contact wave and the shock wave.

We performed a grid convergence for the first test and plotted the numerical solutions of the phasic density (Figure 2a) and the phasic volume fraction (Figure 2b) for comparisons. It is then observed that the contact and shock waves are better resolved as the mesh is refined.

9. Since the authors only give the initial data for velocity, pressure and temperature, the authors should also provide a caloric equation of state to compute the density, i.e., T_k.

Thank you for pointing out this. We have added it.

10. The authors verified positivity of the entropy residual. Therefore it would be interesting to see whether the numerical scheme maintains this property. Therefore I would be interested to see plots of the numerical entropy residual to check whether it remains positive.

Positivity of the entropy residual is indeed ensured for a theoretical point of view. However, when numerically evaluating the entropy residual, negative values can be obtained in the vicinity of the shock. This phenomenon has been previously discussed in [23]. We added a remark explaining such a behavior in Remark-2, at the end of Section 3.5.

11. Besides the two computations presented here I would like to recommend further numerical tests exhibiting stronger variations in the volume fractions and the density, e.g., a shock tube problem for a water-gas configuration with $(\rho, uP, \alpha)_{water} = (1000kg.m^3, 0m/s, 10^5 Pa, 0.99)$ and $(\rho, uP, \alpha)_{gas} = (0.2kg.m^3, 0m/s, 2000Pa, 0.99)$, i.e., the initial states are almost "pure" states of either water or gas, with material parameters as given in [8].

We have run such a case, with nearly pure phases. We used an even smaller value for the volume fraction; instead of 0.01, as you suggested, we used 0.0005. The results are provided using both 200 and 1000 mesh cells.

12. In the conclusion, see page 23, line 41/42 the authors claim that the regularization ensures uniqueness of the numerical solution. Since the authors do not give a uniqueness proof this statement seems to be too strong and should be rephrased. See also my first and second remark.

We corrected this statement that is incorrect and rephrased it.

References

[1] F. Alleges, B. Merlet, Approximate shock curves for non-conservative hyperbolic systems in one space dimension, J. Hyperbolic Differ. Equ. 1 (4) (2004) 769–788.

[2] G. L. Floch, Shock waves for nonlinear hyperbolic systems in nonconservative forms, Tech. rep., Institute for Mathematics and its Applications, Minneapolis, MN (1989).

[3] G. L. Floch, Entropy weak solutions to nonlinear hyperbolic systems in nonconservative form, Comm. in Partial Differential Equations 13 (6) (1988) 669–727.

[4] D. Maso, G. L. Floch, P. Murat, Definition and weak stability of a nonconservative product, Journal de Mathematiques Pures et Appliquees 74 (6) (1995) 483–548.

[5] S. Bianchini, A. Bressan, Vanishing viscosity solutions of nonlinear hyperbolic systems, Annals of Mathematics 161 (1) (2005) 223–342.

The revision of the manuscript has significantly improved its overall quality. The theoretical background on hyperbolic non-conservative problems has been appropriately addressed. In particular, proving non-negativity of the phase density and the phasic volume fraction strengthens the quality of the method, since a rigorous proof is lacking in many publications on methods for two-phase flow. Therefore, it cannot be considered a simple extension of the original work by Guermond and Popov. Moreover, the readability is improved by summarizing the main findings in lemmata. Finally, all my remarks have been appropriately addressed. Therefore I strongly recommend publication the revised version.

In the final version, the authors are recommended to account for the following minor changes:

- p. 2, l. 43 and p. 8, l. 25:
  Replace "Del Maso-Le Floch-Mariat" by "Dal Maso-LeFloch-Murat"

- p. 8, l. 30: replace "Le Flcoh" by "LeFloch"

- Figures 7 and 8: The authors might consider to rescale the pressure and density figures, e.g., $\log(p)$ or $\log(p + P_\infty)$ and $\rho_l/\rho_{l,init}$, $\rho_g/\rho_{g,init}$. Then spurious oscillations will become visible that are suppressed otherwise due to the scaling.

- References [18], [19], [20] and [39]: replace "Floch, G.L." by "LeFloch, P."

- Reference [32]: the list of authors seems to be not correct

- References [39]: replace "Maso, D." by "Dal Maso, G."

- Reference [36]: Replace "Le Mtayer" by "Le Métayer"