Answers to Reviewer #1

In blue are reviewer's comments, in black are our answers, in red are Jean's comments, and in green are Marco's comments.

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.

Marco and I think we have a pretty good handle on this. It will include citing the proper references and the relevant theorems. We must remember to thank the reviewer for pointing this out to us. This was invaluable and improved the quality of the manuscript. We believe that the reviewer is actually giving us the answer to his question. In a very polite way, he is guiding us towards the answer. References [3] and the paper by S. Bianchini et al. entitled "Vanishing viscosity

Solutions of Nonlinear Hyperbolic Systems" contain the theorems related to weak and entropy solutions when stabilizing non-conservative hyperbolic system of equations with a vanishing viscosity numerical method.

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: we meant that the viscous regularization

Here too we will need to cite the proper theorems from the list of references giving to us. Also we should be more careful about the wording and write 'entropy solution' instead of 'uniqueness of the weak solution'.

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 mentioned in your above comment. We added references with further examples of the interfacial variables. It is now emphasized in the paper that the theoretical results devised hold as long as the choice of the interfacial variables preserve the entropy inequality.

Easy to do. We will add another choice as an example and cite [7]. We should also emphasize that the results of the paper are independent of the choice of the definition for the interfacial variables as long as the entropy inequality is preserved.

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 (lines LINES).

Another great remark. We will add this

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 should verify this. We need to ask Ray for the eigenvectors of the SEM if he has them and them check that we have a contact discontinuity

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.

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 and we removed it by mistake. It has been added now.

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 and the phasic volume fraction for comparisons. It is then observed that the contact wave is better resolved as expected.

Marco will run finer grid simulations for both problems and plot all of the numerical solutions on a same figure in order to eyeball the increasing resolution effects.

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. It was a typo. We fully agree with you.

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. When numerically evaluating, however, the entropy residual, negative values of the entropy residual are observed in the vicinity of the shock. This phenomenon has been previously discussed in REF. We added a paragraph explaining such behavior in lines LINES.

Marco will re-run some cases and output that value. Make sure you do not output its absolute value

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].

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Marco will run such a case. The reference given by the reviewer does not seem to be correct. But I think this test is very common.

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.

This will be corrected once we answer the reviewer's remarks #1+2.

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