

Interactive comment on “SedFoam-2.0: a 3D two-phase flow numerical model for sediment transport” by Julien Chauchat et al.

Anonymous Referee #2

Received and published: 10 July 2017

The authors present a three-dimensional two-phase flow numerical model for sediment transport. This type of model requires closure schemes, not only for fluid turbulence but also for particle phase stresses. The model introduced in this paper includes a series of closure schemes for each, all within a single numerical framework. This particular aspect is a welcome addition to the two-phase sediment transport modelling community. A number of models have been introduced over the past 20 years, using slightly different assumptions, closures, simplifications, and intercomparison has remained elusive and difficult, partly due to the lack of a common numerical platform. The result is that there does not seem to be a widespread consensus on the “best practice” for these models.

The authors present the mathematical framework of the model in sufficient details.

[Printer-friendly version](#)

[Discussion paper](#)



However, I believe it would be useful to include a brief discussion summarising how the present formulations compare to existing published models. While this can be somewhat inferred from the text and table 1, it would probably benefit from a few summary sentences that include a brief discussion of potential implications of the differences. A particular aspect to consider would be whether all models using kinetic theory per table 1 are using the exact same model and how these relate to the kinetic theory introduced in the paper?

The authors also present the numerical implementation well, but I would have expected some mention of constraints on the time step. For example, is there a dynamic adjustment of the time step, as in some earlier models, or a static time step, in which case it would be good to discuss how to set this in the first place?

The overall model is then tested in four specific cases: either benchmarks or applications. The ability of the model to use different closures is well used in these benchmarks/applications to gain insight on best practice for two-phase sediment transport modelling. I, however, did not fully understand the reason behind the split behind benchmark and application. If the reasoning is that the benchmark tests serve as validation of the model, then only a few components are truly validated.

This paper does have some issues that would need to be addressed before publication. The most important aspect is that rationale(s) and justification(s) for the work undertaken are rather weak throughout the manuscript and should be improved.

1) I find the argumentation presented in the introduction to be misguided and I think the introduction needs significant revision. Yes, sediment transport is important and “a major societal issue for the management of natural systems”. Yes, we require “the development of comprehensive models”. However, the authors fail to fully recognise what modelling approaches are actually being used to inform coastal management now and in the foreseeable future, i.e. probably not two-phase sediment transport modelling. The reason is that it still is unrealistic to scale up two-phase sediment

[Printer-friendly version](#)[Discussion paper](#)

transport model to the spatio-temporal scales of interest to coastal managers under the typically preferred approaches that emphasise probabilistic hazard assessment. It is important to stress here that I am not stating that two-phase sediment transport modelling is not useful and important, just not for the overall rationale postulated in the introduction. If the authors want to relate their effort to "better coastal modelling", I believe that an argument revolving around improving representation(s) of detailed complex physical processes in the models used for coastal management would be far more convincing.

2) The authors use several times "motivated by [previous publication]" as justification to include some specific closures or focus on a specific case study. I find these rather weak justifications and would encourage the authors to think about how physical processes are reproduced instead. For example, a reason to introduce a k-omega model could be because of known deficiencies of the k-epsilon model (e.g. pressure gradients).

3) The rationale for the choice of the four benchmark tests and applications is not evident. I note that only one case out of four actually uses "real" sediment. Why and would there be implications for applicability of the model? I also note that only steady sheet flow is looked into, even though wave-generated sheet flows have been a key application of two-phase flow modelling, and would be a very important application given the shortcomings of the single phase approach as stated in the introduction. Again, why this choice? Finally, while a two-dimensional case is indeed presented and the formulation of the model is implicitly in 3D (as well as the code as presume), I note that no three-dimensional validation or application is presented. On these points, I fully recognise that adding more cases may not be feasible within the scope of the paper, in which case some discussion of the points raised above would be needed.

Specific comments:

Page 5, line 9: What about other forces than drag? I think some of the early theoretical

[Printer-friendly version](#)

[Discussion paper](#)



works on multiphase governing equations mention other forces such as added mass and lift.

Page 6, line 1: should read “fluid stresses consist of . . .”

Page 8, equation 19: I believe it would be good to mention the work by W Kranenburg testing different equations. (Kranenburg et al., 2014, Advances in Water Resources)

Page 22, line 4: why using a Coulomb rheology?

Page 22, line 20: this is because the sediment . . .

Page 23: figure 2: Please discuss the discrepancy in the middle bottom panel

Page 24, line 6: can sheet flows not be turbulent?

Page 26, line 18-21: I think this paragraph would be better as part of the rationale for the work.

Figures 3 and 4: The figures are not clear enough. There are more line types in the plots than in the legend, the different colour are not explained.

Page29, line 3: studies of . . .

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-101>, 2017.

Printer-friendly version

Discussion paper

