Anonymous Referee #2

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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.

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 sediment transport models listed in Table 1 adopted earlier version of kinetic theory for dry granular flow (e.g., Jenkins and Savage 1983) that does not consider streaming effect due to particle-particle interaction (e.g. Lun and Savage 1987) and the effect of fluid phase. For sediment transport application, the streaming effect provides a smooth decay of granular temperature in the dilute region and ensures a more stable transition of suspension mechanisms from particle collision to turbulent suspension. Hence, in the present model, we adopt the later version of the kinetic theory suggested by Ding and Gidaspow (1990), which include streaming effects and the effect of fluid phase.

The following paragraph has been added to the introduction (from l.22 p. 3 to l. 9 p. 4):

"In this approach, the particle stress associated with particle-particle collisions are modeled by the fluctuation energy of the particle phase (or granular temperature). Various models were developed to model the granular temperature. \cite{Jenkins1998} first applied kinetic theory for dry granular flows to sheet flow, and the granular temperature transport equation was later extended to consider the fluid-sediment turbulence interactions \cite[][e.g.]{hsu2004b,chauchat2008}. The aforementioned kinetic theory considered dense collisions of particles, while the streaming effect of particle random motions were missing in the dilute concentration regime. A further extension to include the streaming effects were developed by \cite[][e.g.]{Lun1987,Ding1990}, and it was implemented into a more complete two-phase model by \cite{cheng2017}. In contrast of solving the transport of granular temperature, \cite{jha2010} used a mixing length concept for the particle phase, and a simpler algebraic model for the granular temperature was used with success."

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?

Yes the time step can be adjusted dynamically using different Courant numbers based on the averaged fluid phase velocities, the relative velocity and the interfacial velocity. This is now written in the manuscript:

"The time step, Δt, can be adjusted automatically based on two Courant numbers, one related to the local flow velocity and the local grid size (the same as for single phase problems) and one related to the relative velocity which is specific to the coupling of the fluid and sediment phase momentum equations in the two-phase flow model. Our practice is to set these two Courant numbers to 0.3 and 0.1, respectively."

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.

Following the reviewer comment we decided to combine former sections 4 and 5 into a single section 4 called "Model verification and benchmarking".

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 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.
We agree with the reviewer's point and in the revised manuscript, we had revised the introduction to better reflect the state-of-the-art in morphodynamic modeling and clarify the goal of this study (see line 29 p. 2 to line 2 p. 3)

"Addressing these issues requires the development of comprehensive models that account for the variety of complex hydrodynamics and sediment transport processes in a regional-scale setting (e.g., Lesser et al. 2004; Roelvink et al. 2009). Because sediment transport occurs very close to the bed, effective parameterizations of sediment transport are needed. In the past decade, significant progresses have been demonstrated to utilize detailed numerical model to understand sheet flow processes and effective parameterizations for coastal sediment transport have been developed (van der A et al. 2013). To further tackle more complex sediment transport problems, such as bedform evolution, scour, bank erosion and dune erosion, further expansion of these models through a community effort is urgently needed. The development of more comprehensive sediment transport models integrating the complexity of the underlying physical coupling mechanisms is the main goal of the open-source community model presented herein."

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).

We would like to thank the reviewer for this comment, indeed, the justification for the implementation of the k-omega model and for the choice of the different test cases was not precise enough and we have modified the introductory par the following subsections:

k-omega
"It is well known that the original $k-\epsilon$ model has been derived for high Reynolds number flows and is not very accurate to describe transitional flows such as the situation of the flow reversal in a wave boundary layer\citep{guizien2001}. For this situation and for near wall treatment the $k-\omega$ model is more suitable and more stable than the $k-\epsilon$ model \citep{guizien2001}. Another physical situation in which a $k-\omega$ model works better than a $k-\epsilon$ model is in the presence of an adverse pressure gradient such as the downward facing step or at the upstream side of an obstacle \citep{menter1993,wilcox2006}. In order to test the influence of the turbulence model, a two-phase $k-\omega$ model is introduced in the present contribution which is very similar to \cite{jha2009,amoudry2014} ones."

Test case 1
"The first test case corresponds to a pure sedimentation of non-cohesive particles, this test case allows to validate the implementation of the pressure-velocity coupling algorithm when the flow is induced by the

sediment phase. The other component that is tested here is the permanent contact pressure model (eq. \ref{eq:paff}) for p^{ff} that allows to predict a stable deposited sediment bed."

Test case 2
"The second test case is inspired by \cite{chauchat2010} in which an analytical solution for laminar bed-load driven by a Poiseuille flow has been used to verify a three-dimensional numerical model. The goal of this test case is to verify the numerical implementation of the granular rheology in \sedFoam. The novelty compared with \cite{chauchat2010} is that the solid phase concentration is obtained as a result of the model in \sedFoam however it was imposed constant in our earlier work."

Test case 3
"In this subsection the model results are compared with experimental results from \cite{revil-baudard2015} and \cite{sumer1996} for turbulent sheet flows, the goal of these test cases is to validate the numerical implementation of different turbulence model (mixing length and $k-\omega$) and to calibrate the free parameter B."

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.

As the model is based on physical ground it should work for both lightweight sediments and sand. The point is that there is no experimental data for unidirectional sheet flow providing velocity and concentration profiles for the same experimental condition. Concerning oscillatory sheet flows using sand, it has been addressed by Cheng et al (2017) using the same numerical model (Kinetic theory and k-epsilon only) and we do not think it is needed to repeat these results here. This is now explained in the introduction of the paper. The following sentence has been added to the introduction (l. 12-13 p. 4)

" \cite{cheng2016} have applied sedFoam using the kinetic theory of granular flows and the $k-\epsilon$ turbulence model to reproduce oscillatory sheet flows of fine, medium and coarse sand \cite{odonoghue2004}."

Concerning the 3D nature of the numerical model, it is indeed 3D but, as the reviewer mentioned, the two-phase flow model is quite computationally demanding and we preferred to focus on 2D configurations that already give an overview of the multi-dimensional capabilities of the code. 3D configurations are under development and will be the subject of future papers.

We would like to remind here that the primary goal of this paper is to present, in details, the mathematical and numerical model formulation of model to serve as a documentation for future users. We agree with most of what the reviewer ask but we believe that it goes beyond the scope of the present paper. We did our best to address the major points and we hope that the reviewer will find our answers satisfactory.

Specific comments:
Page 5, line 9: What about other forces than drag? I think some of the early theoretical works on multiphase governing equations mention other forces such as added mass and lift.

Other forces than drag such as added mass or lift forces might also play a role but in most two-phase flow models only drag force is accounted for. According to Jha and Bombardelli (2010) the lift force only accounts for less than 4% of the drag force and it has no significant effect on the results. The added mass force could play a non negligible role in the denser part of the flow and this would deserve further investigation. The following sentence has been added to the manuscript (l. 1-6 p. 6):

"Other forces such as the lift force or the added mass force could play a role in sediment transport, according to \cite{jha2010}, the lift force in dilute suspended sediment transport only represent 4\% of the drag force and the added mass force can be of order of 10\% in the near bed region. The influence of the added mass force would require further investigation that are beyond the scope of the present paper. "

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

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)
Done

Page 22, line 4: why using a Coulomb rheology?
The Coulomb rheology is used because it is the only way to have an analytical solution of the problem that allows to actually validate the numerical implementation of the granular rheology.

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

Page 23: figure 2: Please discuss the discrepancy in the middle bottom panel
There is no discrepancy here, the results obtained using sedFoam with the mu(I) rheology as to be compared with the numerical solution using the mu(I) rheology (in green) and not with the black dotted line that represent the analytical solution using the Coulomb rheology.

Page 24, line 6: can sheet flows not be turbulent?
We have added turbulent before sheet flows to differentiate with the previous laminar bedload case that could also be called sheet flows.

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

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.
The two line colors represent fluid velocity in blue and sediment velocity in red. In order to limit the number of profiles plotted we have replaced the two profiles per case by the volume averaged velocity profile computed as : U = alpha Ua + beta Ub. This is now explained in the manuscript as well.

Page29, line 3: studies of …
Done