Reviewer's Report on Manuscript egusphere-2024-210 "OpenFOAM-avalanche 2312: Depth-integrated Models Beyond Dense Flow Avalanches" by Matthias Rauter and Julia Kowalski

Reviewed by Dieter Issler

April 23, 2024

Content, novelty and suitability for GMD

This manuscript presents a package of three depth-averaged open-source models for geophysical mass flows (GMFs): fa SavageHutterFoam for dense-flow avalanches (which could also be applied to rock or ice avalanches and with caveats to debris flows), faParkerFukushimaFoam for suspension flows without restriction to the Boussinesq regime, and faTwoLayerAvalancheFoam describing mixed snow avalanches (snow avalanches that develop a substantial suspension layer). The codes use the solvers of the OpenFOAM CFD library for finite-area meshes and are packaged with tightly integrated pre- and post-processing modules, among them a mesh generator. The first two models have been presented previously, at least to some degree, but their merged form, called faTwoLayerAvalancheFoam, is new. The geometrical setting, the governing equations, the closure assumptions and the numerical methods are explained in detail. In addition, the manuscript contains applications of faParkerFukushimaFoam to the idealized test cases used by Parker et al. (1986) to demonstrate the possibility of turbidity currents 'ìgniting" due to entrainment, and of faTwoLayerAvalancheFoam to a simple synthetic terrain for parameter studies and two well-documented powder-snow avalanche events in Austria.

The need to include the suspension layer in snow avalanche simulation has been felt acutely since the early days of avalanche hazard mapping in the 1970s and 1980s, and there have been numerous attempts at creating suitable numerical models for the purpose. Presently, mainly the Austrian two-layer model SAMOS-AT is being used in practice; it couples a depth-averaged (quasi-3D) flow model of the dense core to a depth-resolved (3D) model for the suspension layer. Consequently, computation times are substantial even on modern computers and considerable experience is needed to set up and assess simulations. The suspension layer can attain large heights and has a highly non-uniform velocity profile so that it is not obvious that depth-averaging is justified in this case. However, for practical purposes a reliable, easy-to-use and sufficiently quick code is of very high value; moreover, earlier codes like Eglit's two-layer model from the early 1980s and the quasi-2D code SL-1D (Issler, 1998) have shown in numerous applications that meaningful results can be obtained in this way.

It is thus clear that faTwoLayerAvalancheFoam is of great practical as well as scientific interest. With its focus on model description, numerical techniques and validation, the manuscript is very well suited for *Geoscientific Model Development* and should appeal to many of its readers. As mentioned, the concept of a two-layer depth-averaged model is not new at all, and the governing equations of faTwoLayerAvalancheFoam are very similar to those of Eglit's (1982) model, SL-1D, MoT-PSA (Issler and Vicari, 2024) and—to some degree—RAMMS::EXTENDED (Bartelt et al., 2016). However, the authors' model contains several novel and interesting elements in the formulation of the closure relations, particularly for the suspension rate of small particles. Also, it applies a different numerical technique than all other existing models. In my opinion, the manuscript contains a sufficient amount of novel aspects as to merit publication.

Presentation

The manuscript is clearly structured and well written, striking a good balance between conciseness and detailed explanation of the important points. For most parts, the English is also good; in some passages, improvements are possible, and some suggestions are made in the annotated manuscript.

While the order of the sections is logical and pedagogically well justified, reading the paper feels somewhat tedious because of the length of Sec. 2, which explains the geometrical setting. I think many readers would appreciate it if the model content were presented earlier. One possible approach would be to relegate most of Sec. 2 to an appendix, as most of the material has been presented elsewhere by the first author.

The figures are pertinent and explain the points mentioned in the text well; they cover all issues that require a figure for explanation. A few minor improvements (zooming in in one case, visibility of lettering) are suggested in the annotated manuscript.

The authors have clearly made an effort at referencing copiously, yet this is one of two aspects of the manuscript that I am least happy with. See below under the heading *Major remarks* for my specific remarks.

Major remarks

Curvature effects: The authors formulate the governing equations of their system as what they call surface partial differential equations (SPDEs). Through depth-averaging, the 3D problem of flow over a surface Γ is reduced to a 2D problem on Γ. This procedure is well understood and (fairly) straightforward if Γ is a plane. For general curved Γ, the authors wish to circumvent the involved formalism of intrinsic differential geometry because it requires computation of the metric tensor and Christoffel symbols. Therefore, they work with the 3D velocity or momentum vector throughout, a fact they emphasized in an earlier paper by Rauter et al. and also ought to point out here. They use the projection operators onto the local tangent plane, ∇_s^Γ, and onto the surface-normal direction, ∇_n^Γ to account for the geometry of Γ.

In their Eq. (7), $\partial\psi/\partial\mathbf{n}^\Gamma = \nabla\psi \cdot \mathbf{n}^\Gamma$, there is some ambiguity concerning the scope of the derivative operator, thus IMO it would be better to write $(\nabla\psi) \cdot \mathbf{n}^\Gamma$; this also applies to other equations throughout the manuscript. For vectors or tensors, the equation is correct both in a global Euclidean frame as well as in curvilinear coordinates if, e.g., a vector is written as $\mathbf{v} = v^i\mathbf{e}_i$ (using Einstein summation convention), but it is not valid for the component functions $v^i(\mathbf{x})$ alone if the basis vectors $\mathbf{e}_i$ vary spatially.

Equation (9) and the sentence embedding it appear to contain a misconception or poor wording: In the neighborhood of a point on a curved manifold, the manifold can be approximated by a paraboloid oriented along the normal vector at that point. This implies that curvature effects in the surface derivative of *scalar* functions are of order $(\Delta \mathbf{x})^2$ and vanish in the limit $\Delta \mathbf{x} \to \mathbf{0}$. In other words, the surface derivative of a scalar function defined in the embedding Euclidean space is equal to the tangential derivative. The same holds for the component functions (v_x, v_y, v_z), relative to the global Euclidean basis, of a vector $\mathbf{v}$ in the embedding Euclidean space, and similarly for tensors.

Non-vanishing effects in depth-averaging over a curved surface arise from the deviation of $\det \mathbf{J}$ from 1, which is neglected in the authors' model. In a depth-averaged model on a curved surface, curvature effects besides $\det \mathbf{J} \neq 1$ arise because the velocity vector must be constrained to the tangent plane at every point. In models directly formulated on the curved surface, the basis vectors $\mathbf{e}_i$ vary spatially, implying that the component functions of a constant vector are not constant. This is captured by the covariant derivative, as described in all classical textbooks on differential geometry.

A second problem of Eq. (9) is that the dimensions do not match: the Gaussian curvature $K = \kappa_1 \kappa_2$, with $\kappa_{1,2}$ the two principal curvatures of Γ at that point, has dimension L^{-2}. The authors cite (Dieter-Kissling et al., 2015) and (Tuković and Jasak, 2012) to support their claim. However, those authors do not use the Gaussian curvature K but the *mean* curvature $H = (\kappa_1 + \kappa_2)/2$, which has the correct dimension L^{-1}.

Yet another problem of Eq. (9) is its failure to account for the tensorial nature of curvature. Take as an example a circular cylinder of radius R, the Gaussian curvature of which is identically 0 because the principal curvature in the axial direction is 0. Equation 9 then predicts that there are no centrifugal pseudo-forces, even if the flow moves normal to the cylinder axis along the circumference. In reality, there is a centripetal acceleration u^2/R. If the Gaussian curvature were replaced by the mean curvature, the centripetal acceleration in the azimuthal direction would no longer vanish but would be too small by a factor $1/2$ while there would be spurious centripetal acceleration in a flow along the axial direction.

I suspect that the authors overlooked some fundamental differences between Tuković and Jasak's (2012) interfacial flow model and GMF models: The thin-film model consists of a transport equation for the adsorbed surfactant mass, with the velocity determined by the bulk flows on either side of the interface; there is no separate momentum balance equation for the interface, only jump conditions for the interface-normal component of the stress tensor in terms of the mean interface curvature and the surface tension. This jump condition must sum the contribution from the surface tension over all directions. This is fundamentally different from the situation in a GMF, where the surface curvature only in the instantaneous flow direction determines the surface-normal stress.

Presumably, replacing the Gaussian curvature by the curvature in the flow direction will require non-negligible changes in the code because the principal curvatures in each cell and their orientation must be computed before the simulation so that the relevant curvature can be computed from them at each time step. Fortunately, this can be done quite efficiently using the first and second fundamental forms of the surface, as in the codes MoT-Voellmy, MoT-muI and MoT-PSA. Nevertheless, fixing this and redoing all simulations with faTwoLayerAvalancheFoam represents a substantial effort and concerns a point that is not in the focus of this manuscript. In my opinion, it would be acceptable if the authors corrected the mathematical formulation in the paper and clearly stated that the simulations were run with an earlier version with an erroneous formulation of the curvature effects. It might be instructive to rerun one of the two real-world examples with the curvature term turned off to obtain an idea of the size of the effect. I suspect, however, that the Gaussian curvature is close to 0 along most of the thalwegs because the bottom of gullies resembles a cylinder.

Referencing: There are different opinions on whether one should give references for generally known facts. The authors chose to do so, but the selection of references appears haphazard in many cases. If there are dozens of standard textbooks explaining a specific point, choosing a lesser known and relatively recent one appears partial and is probably of little help to most readers. In such cases, my preference would be to say "as shown in all standard textbooks on fluid mechanics" or similar. However, the authors should let themselves be guided by the journals's and the editor's stance on this issue.

Selecting referenes becomes even more of a problem where the authors choose to reference journal papers that do deal with the aspect in question but do not represent a significant advance in this topic. In such cases, I think the seminal paper(s) ought to be cited. This situation arises in many places throughout the manuscript. Perhaps the most serious omission concerns the early Russian work on depth-averaged avalanche models from the 1D constant-density dense-flow models in the mid-1960s (Eglit, Grigorian, Yakimov and others) to Eglit's two-layer model of 1982, later developed further and tested extensively by Nazarov. That work preceded similar work in the West by roughly two decades, and it was accessible through several conference proceedings and translations yet largely ignored. The model described here does not differ from the Eglit–Yakimov model in its general set-up. Its innovations are mainly the 2D formulation (which was impractical in the early 1980s), a different numerical technique and different entrainment and suspension functions. This ought to be communicated clearly.

While I strongly dislike advertising my own work, two papers I have been involved in, from 1998 and 2024 respectively, are of some relevance here: (Issler, *Ann. Glaciol.* **26**, 1998) is a two-layer model that predates all two-layer models cited by the authors. It also is the basis for the code SL-1D, which was widely sold by SLF as part of the package AVAL-1D and still is in practical use in Switzerland today (despite its shortcomings). The very recently published paper (Vicari and Issler, MoT-PSA: a two-layer depth-averaged model for simulation of powder snow avalanches on three-dimensional terrain. *Ann. Glaciol.*, 2024, DOI 10.1017/aog.2024.10) is an extension of Eglit's model to 2D. It uses a simpler numerical technique than Eglit's or the authors', makes different choices for some of the closure relations and is from the ground up designed for use by practitioners with limited computational resources and little time to set up a complex software system. Otherwise, it is rather similar to the model described in this manuscript. I do not request the authors to cite these two papers, but they should at least be aware of their existence.

Minor remarks

Please see the attached manuscript for (a large number of) annotations on misprints, suggested changes of wording, minor remarks concerning the figures, etc. There are two points I wish to specially mention here:

- According to the README in the code repository, faSavageHutterFoam and faTwoLayerAvalancheFoam offer a variety of entrainment formulas. Such choice is very welcome because there is no good, generally accepted solution to the entrainment problem yet. However, IMO the authors have not chosen the models with the strongest physical foundation. In the manuscript, they explicitly present the model used by Fischer et al. (2015) in a calibration study. Assuming that the parameter e_b is a constant, that model predicts the entrainment rate to increase linearly with the flow velocity if the bed shear stress is kept constant. It has been shown by several authors (Norem and Schieldrop, 1991; Fraccarollo and Capart, 2002; Issler and Pastor, 2011; Iverson, 2012; Issler, 2014; Iverson and Ouyang, 2015) that the entrainment rate must scale with the *inverse* of the velocity under such conditions because the eroded material must be accelerated to the flow velocity. Interestingly, this mechanism is incorporated in one of the two models proposed by Medina et al. (2007), but the authors chose to implement the other, which neglects the dynamics of the entrainment process. Equation (21) in the manuscript could be made more physical if one replaced e_b by $e_b + \bar{u}^2/2$.

- The authors are very computer-savvy and therefore consider OpenFOAM-avalanche a simple, user-friendly system. Based on my experience both as a consultant and a researcher interacting with colleagues who do consulting work, I cannot quite agree with this assessment. While most practitioners are reasonably proficient with (their preferred) GIS, installing OpenFOAM and learning how to use it represents a higher hurdle than most practitioners are willing to surmount. I therefore suggest to tone down the respective statements.

Recommendation to the editor

While the second of my major criticisms can easily be remedied by rewriting parts of the introduction and adjusting the referencing in the rest of the paper, the first is more serious and more difficult to address. However, it is not critical for the main message of the paper and could be handled by pointing out that the simulations were run with an earlier code version. In view of this, I must leave it to the editor whether to ask the authors for a minor revision (but extensive in the small details) of their manuscript—preferably with another, quick round of review—or to request a major revision that fully addresses the curvature problem. Irrespective of this decision, I look forward to the eventual publication of this interesting and important manuscript.

Esentepe, 2024-04-23

Dieter Issler