Review: Snow avalanche friction based on extended kinetic theory
(by M. Rauter, J.T. Fischer, W. Fellin and A. Kofler)

General remarks

The goal of this paper is to develop new Voellmy-type friction relations based on "extended kinetic theory". Although Voellmy-type friction laws are used extensively in snow avalanche modeling, they have several well-known drawbacks. The primary problem is that Voellmy parameters cannot be measured directly in experiments, and parameter selection is based on calibration using extreme avalanche case studies.

This paper attempts to address two open questions in avalanche dynamics. The first question is: Is there a better theoretical foundation (kinetic theory) to describe the Voellmy model? The second question is: Can sets of friction parameters be found for the reformulated Voellmy model that are based on a rational calibration procedure? At the end of the paper I came to the conclusion that the existing Voellmy model is, at least, clear, and the "extended" version does not serve to clarify or improve model results. This is probably the exact opposite of what the authors had hoped to achieve. The paper simply does not meet the expectations defined in the abstract.

My main problem with this paper is the presentation of the kinetic theory. This presentation is a mere copy of several papers, not extending the cited work, but restricting the model to assumptions like steady-state, or simplifying the physics, e.g. describing avalanches as a system of spherical particles of the same size. The presentation is highly mathematical and not directly linked to the material snow. As soon as the authors are confronted with specific values of their kinetic theory model parameters, they appear to be disinterested, simply stating that "...parameters for snow are not available" (p 6 line 20). Do the authors want the reader to believe that the model is an improvement without having a stronger link to the material snow and avalanches?

Didactically, the description of kinetic theory is poorly constructed, lacking a common thread. I question, whether the authors ever identified for whom (except for themselves) they have written this article. I couldn't really find out who is their audience they want to address. I became frustrated with the presentation because it avoids a discussion of the material snow, providing no physical arguments why the model is especially applicable to snow avalanches. I continually asked the question if the authors are introducing new concepts for snow avalanches or applying existing theories without regard to the special properties of snow.

The second problem I have is that much of the content of this paper has already has been published by Fischer et al (2015) in the J.of Glaciology. I propose that the authors divide the paper up in an review of "the kinetic theories of granular flows" under the perspective of application for snow. The second part could be placed in a separate paper dedicated to the application of SamosAT to the two avalanches different from the ones already treated this way in Fischer et al (2015).

Specific Remarks

1. Equation (5) is an example of a vague definition. Is V_p the volume of a single particle or is it the sum of all the particles in the undefined Volume V?

2. The same holds for equation (6). How is the mean velocity defined?

3. How is Fig 2 related to the intention of this paper – What part of a snow avalanche does it represent? The boundary conditions represent in no-way the situation in an avalanche. In an avalanche, the upper surface is stress free and free to deform; while the lower surface has a shearing and cannot move past the hard boundary. If the authors argue it is an infinitesimal volume, then the volume is too small compared to the size of a particle. Although the authors wish to use a continuum theory to describe a granular gas, the “molecules” in an avalanche are considerably larger than in a gas, especially when compared to the location of the boundary conditions.

4. The equations with the f_i 's may be simplified, as e.g. equation 7 for σ_q = proportional E. What is the physical meaning of the f’s. On what physical parameters do they really depend?

5. Give the number value for the fixed ν 's. The volume V is still undefined and different for the three ν 's. e.g. what is the ν for a closed packed volume?

6. p4 last line What is the reasoning for applying a steady-state model to snow? In my opinion avalanches are far from steady conditions.

7. p6 line 11 (Christen et al. 2010). Equ. 27 is exactly the formula of Christen for their random kinetic energy. Only Christen et al. do not go into simplified "extension", and therefore have the possibility to treat non-steady states, which is certainly more appropriate for snow avalanches. It seems to be questionable to use the rather unrealistic assumptions for steady state (including no change of the volume = height of the avalanche!) for the sake of an "algebraic formulation". In the end, the numerical solution provides solutions outside steady state.

8. I assume that the total σ (equ. 25 and 30) is the pressure at the bottom and with the current assumptions (steady state) = mg, that is the weight of the mass in the volume. Dynamic pressures should also be considered.

9. Other questions: p9 first line: What "other distribution" ? The authors might mean "set of parameters". The individual velocities (u_i of the i-th particle) must have a distribution such, that the first and the second momentum of the distribution does not vanish. Only in this way can one have a granular temperature. Define the mean velocity in a mathematical consistent form. Why do you use the factor 3/2 in your formulation of granular temperature? What is the physical significance of this factor?

10. p 16 line 19 I think "distributive" should be "commutative".

Conclusion: Before this paper can be published, the first part of the paper has to be rewritten, and the second part of the paper shortened considerably, if not dropped completely from the paper. I recommend major revisions or outright rejection.