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Interactive comment on "Snow avalanche friction relation based on extended kinetic theory" *by* M. Rauter et al.

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In the following, the comments of the referee are printed in black, our replies are printed in blue.

We thank the referee for his effort in reviewing our article. However, we do not agree at all with the main criticism. First of all the statement, that most of the material was published in Fischer et al. (2015) is simply wrong. In this article we investigated solely the Voellmy model in detail with the help of an optimization method that differs substantially from the one presented in the new work. The referees opinion that the "extended version does not improve model results" cannot be justified by our results.

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Simulations with the new basal friction relation have a greater predictive power than calculations with the Voellmy friction relation. This is a criterion to prefer the new model following Karl Poppers deductive procedure, and readers should have a chance to decide themselves. The argument that snow cannot be modeled by granular models is one which is used extensively against all developments coming from the mechanical community, e.g. even against the early versions of the Savage-Hutter model, which is extensively developed by performing sand chutes experiments. As this model is now widely and successfully used in snow avalanche simulations, there should be a common agreement, that granular flows have great similarities to snow flow to some extend, simply because snow grains and lumped formations of grains can be regarded as granular media. It is clear that thermodynamic issues cannot be tackled with purely granular models. However such considerations are out of the scope of the article. In the following, we provide a detailed response to each issue raised by the referee.

The goal of this paper is to develop new Voellmy-type friction relations based on “extended kinetic theory”.

We have to disagree here: The goal of this work is to investigate the Extended Kinetic Theory (EKT) and investigate possible benefits that can be adopted for snow avalanche simulations. A lot of different approaches to include the EKT into snow avalanche simulations have been tested by the authors and the presented relation appeared to perform best in terms of usability/applicability and accuracy. The fact that we obtain a friction relation (based on EKT with a first principle derivation) that has a similar form to relations presented by others (such as Issler et al. (2005) or Korner (1980) and even Voellmy (1955) to some extend) motivates us to test the new relation with respect to the well known / established Voellmy relation.

Although Voellmy-type friction laws are used extensively in snow avalanche modeling,

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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.

Exactly true - this is why we perform an extensive back calculation in the second part of the paper - however now the turbulent friction term is physically motivated! And this is also why we cannot simply provide fitting parameters for snow (they are not measurable) in the qualitative model test.

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?

We disagree again, since we did not derive the Voellmy model. In our view the two main questions are:
(1) Can we use the theoretical framework of EKT in snow avalanche simulations? Which turned out to be possible with a reduced formulation.
(2) Does the theoretical framework (i.e. the reduced formulation) describe nature?

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.

Beside the first principle derivation (as mentioned above) we have a look at the predictive power of both models: If we calibrate the parameters of both friction relations on the RGF event and preform a prediction of the VdlS event with these parameters, we obtain a normalized global error of 0.27 ($\delta r = 300\,m$, $\delta A = 140\,000\,m^2$, $\delta\,u = 18\,m/s$) for a computation with the Voellmy relation and a smaller normalized global error of 0.15 ($\delta r = 180\,m$, $\delta A = 98\,000\,m^2$, $\delta\,u = 13\,m/s$) employing the proposed EKT based relation. Thus we do not agree with the statement of the referee.

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.

We applied the EKT model and did not further modify it, that is right. We do not further restrict the model assumptions, since steady-state or spherical parameters of the same size come from the original source of the model, Vescovi et al. (2013). This limitations follow simply from the nature of KT. KT is a model from statistical mechanics and the statistics would get incredible difficult when introducing variable particle form and transitional states. The statistical model is already really complex as it is, see Garzó and Dufty (1999).

The presentation is highly mathematical and not directly linked to the material snow.

The presentation of the mathematical formulation of the KT is probably not necessary in such a detail. We suggest to move functions f_1, f_2, f_3, f_4, f_5, G, g_0, F, J, s, L/d into a table or an appendix.

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

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snow are not available" (p 6 line 20).

We speculated a lot about the parameters for snow in the EKT model. However, we where not able to come up with a legitimate method to specify them. We are open and happy for every suggestion in this regard!

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?

The authors are convinced that the model is an improvement since the article provides a clear derivation based on EKT leading to a friction relation that provides simulation results with a higher predictive power, see comments above.

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.

We are sorry to hear that. Please note that we are not native speakers.
Moreover, we think that we captured all important features of this theory, at least for people that had some form of contact with the concepts before. As mentioned before, we think that the concepts of kinetic theory are not completely new in the snow avalanche community (see answer to referee 1).
Moreover, we cited literature which should help in understanding kinetic theory in depth, e.g. the PhD-Thesis of Vescovi (2014) or the two very popular review papers from Campbell (1990) and Goldhirsch (2003).

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.

We are discussing models for granular materials in general and their application to snow.
The special characteristics of the material snow/ice particles in comparison to perfectly spherical particles of a single size are not included in this work. However, these special characteristics are not in the scope of the kinetic theory and therefore not in the scope of this paper.
Moreover we want to note that obviously every real granular material, including sand and even glass/steel beads differs from the particles assumed in KT in some way.
Also, the basal friction relation is obviously only a part of the whole avalanche model. More snow avalanche phenomena are additionally taken into account,e.g. entrainment.

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.

We are applying existing theories which are rather new in the field of snow avalanche simulations, as far as we know. We already answered the treatment of 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.

See introduction.

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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).

It is not our goal to compete with the very good reviews of rapid granular flows and kinetic theory from Campbell (1990) and Goldhirsch (2003) or in the introduction of Vescovi (2014). The second part of this paper shows results of the derivation shown in the first part, in the sense of a validation. Thus the two parts can not be divided.

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?

The exact same definition can be found in basic geotechnical textbooks, e.g. Kolymbas (2011). V_p is the volume of particles in an arbitrary control volume, V is the total volume of the same control volume. We think this is comprehensible and we think it is still comprehensible without indicating explicitly that the entities are related to a control volume.

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

The mathematically correct definition of averaged entities within the field of fluid dynamics is quite complex and would be confusing at this point in our opinion. We think the explanation given in the paper is sufficient to give the reader an idea of what we are talking.

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.

Figure 2 is a sketch showing the definition/orientation of stresses σ/τ, velocity u and shear rate $\dot{\gamma}$ in a simple shear setup, which seems to be necassary, compare question (8) on σ. If the editor thinks this figure is superfluous we can remove it. We missed to point out that figure is based on Vescovi et al. (2013) - we will mention this in the revised manuscript.

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.

The same holds for every granular flow.

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?

As mentioned before, the kinetic theory is a model from statistical mechanics. The functions f_i, F, J, etc. follow from the evaluation of distribution functions. The dependence on the physical parameters is shown by the equations. An exception is the function f_0 which is not related to kinetic theory but critical state theory. This function is the basis for the critical state line. Physically or geometrically interpretable functions are the radial distribution function g_0, well known in the field of rapid granular flows/kinetic theory. s is the "mean separation distance among particles" (Vescovi et al., 2013), L is the "correlation length, accounting for the decrease in the rate of collisional energy dissipation due to the correlated motion of particles that is likely to occur when the flow is dense" (Vescovi et al., 2013). We explain these functions where we think it is useful, e.g. the critical state line, as this is rather unknown in the avalanche community. We listed all equations for the sake of completeness. As mentioned before we suggest to move functions f_1, f_2, f_3, f_4, f_5, G, g_0, F, J, s, L/d into a table or appendix.

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?

We give number values for $\nu_{rlp}(0.55)$ and $\nu_s(0.619)$. There is no closed packed volume - According to the critical state theory, granular material under motion always reaches a certain stress dependent concentration, the critical concentration, expressed by the critical state line (Roscoe et al., 1958; Schofield and Wroth, 1968). There can be a looser packed volume because of the additional collisional stresses, but no closer packed volume (at least for large deformations) (Vescovi et al., 2013).

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.

The assumption of a steady state is used two times in the derivation of the basal

friction model:
(1) Statistics of the kinetic theory. (not part of this work)
(2) Describing the velocity profile.
We discussed the first point already. We will discuss the velocity profile later.
To get rid of the granular temperature we use the equilibrium assumption, which is not limited to steady simple shear but is also applied to dense flows. It is also an option in classic 3D kinetic theory-multiphase-CFD-applications like MFIX (Syamlal et al., 1993) or OpenFOAM (van Wachem, 2000) and produces good results for dense flows. As mentioned in the manuscript, it is justified by the dominance of generation and dissipation terms over convection and diffusion terms: the mean separation distance among particles s is so small, that collisions between particles will dissipate all of the granular temperature within a time, much smaller then the characteristic time scale of the problem.

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.

See answer above.

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.

The pressure at the bottom is indicated with a subscript b and contains the "dynamic pressures".

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.

Here we refer to a different splitting of quasi-static and collisional stresses to the terms of the reduced formulation, $\mu\,\sigma$ and $\lambda\,\dot{\gamma}^2$. We will change the word "distribution" to "splitting".

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?

We choose this formulation of the granular temperature to be consistent with the cited literature, e.g. Vescovi (2014).

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

Thank you, we will change this.

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.

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