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Discussions

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.

The authors present an avalanche model parameter study comparing the more traditional Voellmy model approach to a model (friction relation) based on a theory for granular shear flows. The paper can be divided more or less in two parts. To obtain the "new" friction relations, they derive a depth-averaged model, which is presented in the first part of the paper. Derivations follows the granular shear flows model by Vescovi (2014). Ibn the First part, they present also some parameter studies using material parameters, which might be relevant for small quartz particles. In the second part of

the paper, the authors provide a cross-comparison of their friction relation with the commonly used Voellmy-model. To this end, the authors study two observed avalanche events for which velocity and field data are available. The paper is reasonably well written and the topic can be interest for readers of NHESS. Therefore, the paper might be worth to be published. Some language checking should be done. However, there are several ambiguities in the presented in the paper, which should be addresses by the authors before publishing. General comments: The other present a friction relation for snow avalanches which they base on what they call extended kinetic theory.

First we would like to thank for the review and for the constructive comments on the manuscript.
The term "extended kinetic theory" has not been introduced by this work. "Classic/standard kinetic theory" is based on the statistical description of binary collisions of granular particles. These are the relevant processes/forces at low volume fractions ($\nu < 0.49$). The "extended kinetic theory" includes extensions (Jenkins and Berzi (2010); Vescovi et al. (2013) in our case) to make the model suitable for high volume fractions and therefore dense flows and the description of the basal friction. We use the expression "extended kinetic theory" because it clearly states, that the model does not only include collisional but also quasi-static forces.

Similar approaches have been present previously, e.g., by Körner who also suggested a height dependency on the Voellmy ξ parameter (Körner, H. J. Modelle zur Berechnung der Bergsturz- und Lawinenbewegung, Interpraevent 1980, 1980, 2, 15-55.). The work would be worthwhile to be cited.

Thank you, we will cite this work.

The author argue to present a "new" friction relationship for snow avalanches, however, their first parameter study in the first part of the paper is probably only relevant for small quartz particles.

This is correct and it is also why the first part of the paper deals with the general features of the derived theory/relation. We use the material parameters described in Vescovi et al. (2013) for sand to show some general/qualitative features of the model. We are aware that results of this analysis can not be transferred directly to snow avalanches and also state this clearly in the manuscript. A parameter study is not feasible at this point, because (i) we expect that the general features of the model wil not change qualitatively for different materials and (ii) an extensive parameter study is simply not possible, since these parameters are not measurable directly. We decided to show these general features with the idealized quartz sand instead of guessing material parameters for snow or ice particles. One can maybe guess physical properties like the particle density or particle diameter, however, guessing dimensionless coefficients like the parameter a for the critical state line seems questionable.The fact that we can not transfer results from sand directly to snow avalanches is considered in the second part of the paper where we test the predicted relation on real scale snow avalanches.

Generally speaking, the approach to the problem of finding a suitable friction relation for snow avalanches is new in a few aspects:

Foundations similar to the ones of the here used kinetic theory (KT) are found in the snow avalanche community (e.g. Bartelt et al. (2006), Buser and Bartelt (2009), Bartelt and Buser (2010)). However, (to our knowledge) there is no publication, applying the constitutive microrheological relations from KT (in particular Jenkins (2006, 2007); Jenkins and Berzi (2010); Vescovi et al. (2013)) to snow avalanches.

Buser and Bartelt (2009) state: *Our goal is to find a reduced description of the flow rheology that accounts for the granular interactions without oversimplifying the problem by lumping the granular effects into a single constitutive parameter such as an 'effective' viscosity or 'turbulent' friction (Salm, 1993). At the same time we avoid a formulation requiring the micro-collisional properties of the granules (coefficient of restitution) or the particle size and shape distributions (Jenkins and Savage, 1983; Hutter and others, 1987; Jenkins and Askari, 1994; Louge, 2003).*"

On the contrary our approach is to investigate the microrheological description (the extended kinetic theory in our case) to derive a simplified macroscopic friction relation (or "reduced description" as called in Buser and Bartelt (2009)). We thought of this process as an opportunity to combine different scientific fields more then the development of a new model.
Moreover, based on the work of Vescovi et al. (2013) it is possible to include the "quasi-static/dry friction" and its interaction with "collisional friction" into the rheological model, which has not been applied to snow avalanches before. So far, dry friction has always been added on top of the collisional friction without any strict physical derivation.

The resulting friction relation is similar to ones presented by others, such as Issler and Gauer (2008) as mentioned in the manuscript or, as the reviewer mentioned, Korner (1980) and even Voellmy (1955) to some extend. However, in our eyes the agreement with prior (sometimes empirical) work using a novel approach, employing theoretical/statistical mechanics appears as an accomplishment. Furthermore in the presented approach it was not a goal to obtain a result similar to Voellmy or the NIS-Model - it appears to be a product of the reduced description of the extended kinetic theory. We describe very clearly how we obtain the friction relation and it is not related to empirical approaches (e.g. Voellmy (1955); Korner (1980)) in any way.

Although some interesting features are presented, the authors lack to discuss the limitations/restrictions of their model in respect to snow particles/clouds, which constitute avalanches. I have doubts that, e.g., Eq (7) combined with Eq (9) (which suggests, e.g. an singularity) gives reasonable results for snow clouds.

Equations (7) and (9) express the critical state line. There is a singularity for the quasi-static normal stress when approaching the closest packing. As a result, the closest packing can only be approached asymptotically, which we find reasonable. On the the other hand this form for the critical state line guarantees that quasi-static stresses vanish at volume fractions below random loose packing. This results in no quasi-static stresses in powder snow clouds, which we find reasonable too.
Moreover, the basal friction model derived in this work is only used for the dense flow part of the avalanche. The powder cloud is not included in our simulations.

Similar reservations hold also for the other parameters in the kinetic model part of the paper. These limitations need to be discuss. On the other hand, in the second part of the paper the user restrict themselves again to more or less "traditional models" with a constant density; abandon the avenue the kinetic theory could give to include varying densities in the flow.

This restriction is necessary, because we use an operational avalanche simulation tool, which includes the assumption of constant density. However, c.f. figure 4 the density is almost constant in the dense flow (quasi-static, mixed regime). Note that no powder cloud was investigated in this work.

It is also not clear to me, if the author include the factor 2/5 originating from the velocity

profile is also include in the balance equations or only in the friction relation.

The shape of the velocity profile is only used to derive the basal friction model. It is not included in the momentum balance equation (equation 2). The shape factor which accounts for the non-linearity of the convective momentum flux would be $5/4$ for the assumed velocity profile (e.g. Baker et al. (2016) and citations therein). This factor is not used (respectively set to 1) and not investigated in this work. We will add some text to clarify this issue.

The comparison in the second part as such is interesting, and the methods seems to be legitimate to compare simulation results with observations. However, here it puts the questions, if parameter sets for avalanches with different return periods (RGF probably 1-2 years whereas the VdlS event was probably in the order of 30 years) should be combined.

Thank you for this comment. In the same manner one could ask: why should the return period of an avalanche be related to the parameters describing physical/frictional processes? If the underlying model properly describes the physical processes then return periods are somewhat represented through documented avalanche release areas/volume (as we assume in our case). Beside release volume other snow conditions such as potentential entrainment enter the simulation through the choice of the entrainment parameter e_b (which may ultimately be connected to snow temperature, compare e.g. Vera Valero et al. (2015)). Thus it might be necessary to adapt model parameters for operational avalanche prediction of avalanches with different return periods, but we see no problem in comparing single events where differences of snow properties/volume are explicitly taken into account.

The second part is an application of our new friction relation, to check if it has some advantages to the well established Voellmy model. We will clarify this in the conclusion.

Some specific comments: page 2 line 26 x is the curvilinear coordinate: Eq (1) and eq (2) are by no means written in curvilinear coordinates: you are using local Cartesian here.

The coordinates look like local Cartesians only, which is an artifact of their derivation. This derivation includes the assumption of small curvatures (see e.g. Pudasaini and Hutter, 2007), which is standard for these models and also implemented in the used operational simulation software. We are well aware of other formulations explicitly including large curvatures, like Bouchut et al. (2004). We will add "Note that equations (1) and (2) are only valid for small curvatures".

page 3 line 13 Constitutive relations in the framework of three-dimensional continuum mechanics: Here, you only present your relations in respect to simple shear not for a fully three-dimensional framework, see also line 18.

You are right. We presented the model in terms of simple shear boundary conditions, which is well suited for our implementation, and much less complex than a fully 3D formulation. We would change the section title and the first sentence to:
3 Constitutive model
In this section a rheological model formulated for simple shear conditions is presented.

Eq (6) is this correct for simple shear (coefficient 3/2???).

We found this definition for the granular temperature in different papers, e.g. Vescovi (2014), equation (3.7). The random motion of particles is assumed to be three dimensional (although the mean velocity is one dimensional in simple shear), therefore the definition of the granular temperature in simple shear flows should not be different than in any three dimensional flow. However, the definition of the granular temperature does not affect the developed model.

page 8 line 7 This form of constitutive relation is difficult to implement in an operational simulation tool: Why ????

(1) You need a root-finding method to solve the non-linear system of equations (30) and (31). As you can see from figure 2 the system of equations creates a very complex surface in the σ-$\dot{\gamma}$-ν-space. Therefore the root-finding method will not converge from time to time.
(2) Moreover, the equations do not yield a result for every pair of σ and $\dot{\gamma}$ ($\overline{u}$, respectively).
(3) The flow model used by the operational simulation tool is incompressible. Therefore it is not possible to include the variable density. The friction proposed by the EKT can be approximated quite good with the reduced formulation. Below you will find some figures, showing the friction as proposed by the full EKT in comparison reduced formulation (figure 2). We suggest to include these figures in the manuscript.
We implemented the full EKT model into SamosAT as experiment (without variation of the density in the flow model) and conducted some simulations. We did not see any benefits from these tests but had to deal with all the drawbacks like significantly increasing calculation times, "guessing" the friction in regimes where the EKT model gave no solution or the handling of eight constitutive parameters.

The only major drawback we see, is that the reduced model does not account for the non monotonic behavior at high stress levels and the post-peak behavior. We state this clearly in the paper and we will try to include this in future work.

Figure 8 ν 1 ??????? What do you mean with the text in the figure, something missing ???

Thank you, there seems to be a problem with fonts in the figure. In the original version there is written $\nu \ll 1$ (see figure 1 below). We will figure out a workaround.

Figuure 10 caption The error bars show the high fluctuation of velocity of grains which agrees to the assumptions of the kinetic theory: Why ??????? First of all, errorbars represents the error/uncertainty in the measurements.

The data was presented in Sovilla et al. (2015), where velocity fluctuations but not error/uncertainty is mentioned : "Velocity profiles, velocity fluctuations and strain rates are inferred by cross-correlating optical backscattering signals".
The bars shown are in fact no error bars. To clarify, we will change the text to "... is shown (red filled circle: mean velocity, bars: fluctuations)."

page 13 line 18 This leads to a lower friction for larger flow heights and therefore larger avalanches. This behavior is in line with observations: Which ones????? Per contra, how do you explain, for example, observations by Wagner, P. Kalibrierung des Modells für das Ermitteln der Auslauflänge von kleinen und mittleren Lawinen Institut für Alpine Naturgefahren (IAN), BOKU-Universität für Bodenkultur, Institut für Alpine Naturgefahren (IAN), BOKU-Universität für Bodenkultur, 2016"

Typically recommended friction values depend on the size/height of the avalanche and are typically smaller for large events (Gruber et al., 1999; Salm et al., 1990).
It is often shown that the average slope angle depends on the volume of the avalanche, e.g. it is obvious that the average slope angle is correlated with friction and volume with height (Scheidegger, 1973; Bovis and Mears, 1976; Alean, 1985; Korner, 1980).
Unfortunately we did not find any relation between avalanche mass and runout in Wagner (2016).

page 15 line 1 The affected area near the deposition area. The deposition area cannot be analyzed directly because the dynamic model does not simulate the deposition process explicitly.: I'm not sure if I get what do you mean here. Do the simulations not show the final height etc. at the end?????

State of the art simulation tools usually do not model deposition processes. Thus the final flow depth refers to the flow depth at some point in time where velocities/depths are below a certain threshold - thus final flow depths can not simply be compared to observed deposition depths (stopping or densification are not properly captured by the underlying model). Here (similarly to Fischer (2013)) we analyze simulated peak pressures, which correspond to observed affected areas (which are not necessarily equal to the deposition). We will clarify it in the paper accordingly.

Figure 12 a and b. Without any information of at least the rough profile of the path are the figure little meaningful.

We will add the profile of the path to figure 12 (see figures 3 and 4 below).

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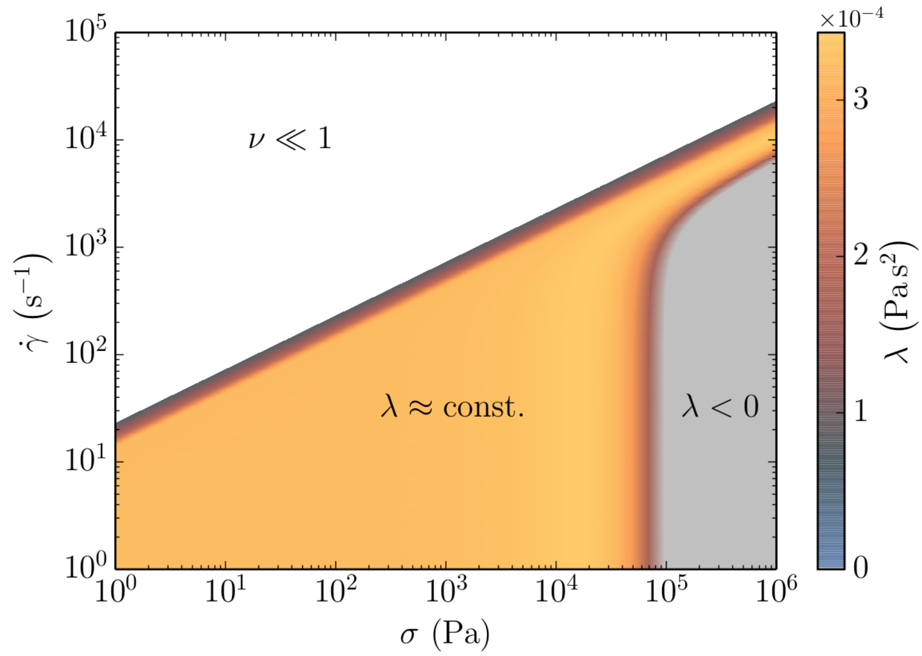

Fig. 1. Original version of figure 8

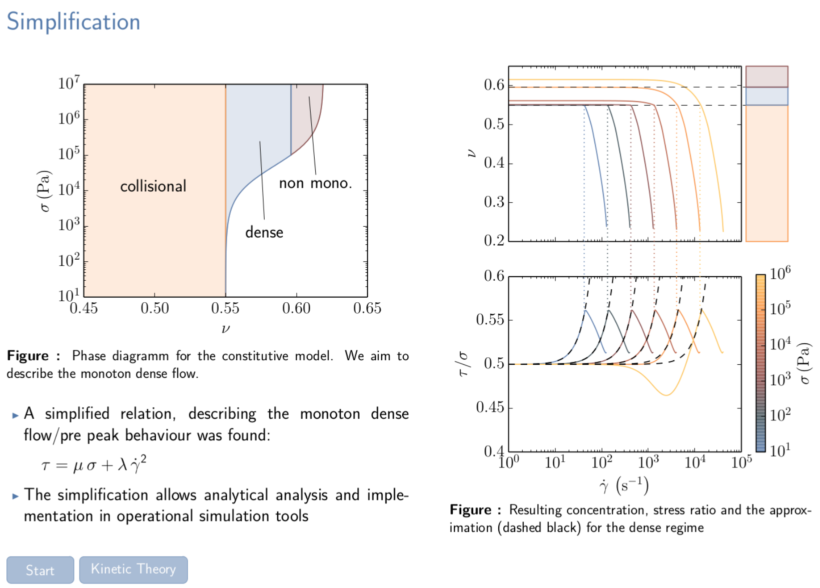

Fig. 2. Full EKT compared to the reduced formulation. We suggest to add an addapted version of these figures to the manuscript.

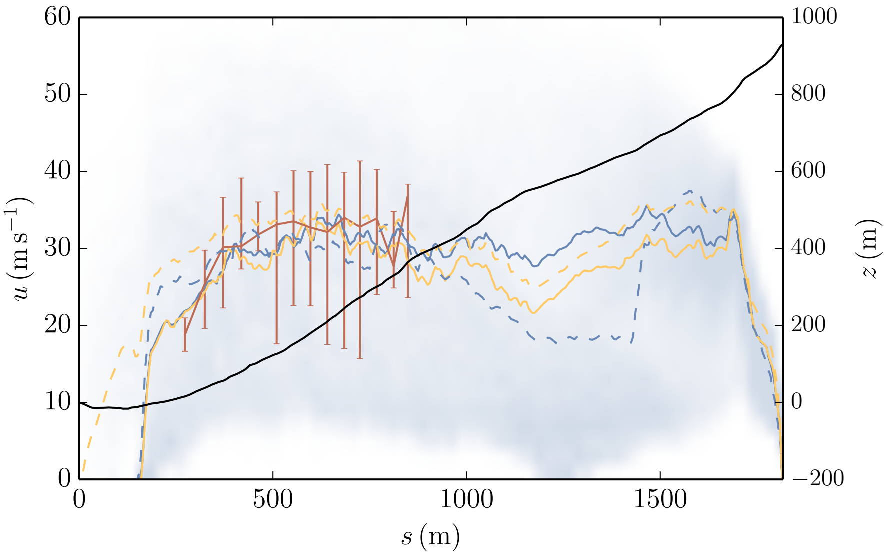

Fig. 3. We will add the path profile to figure 12. Here shown in black for Rgf.

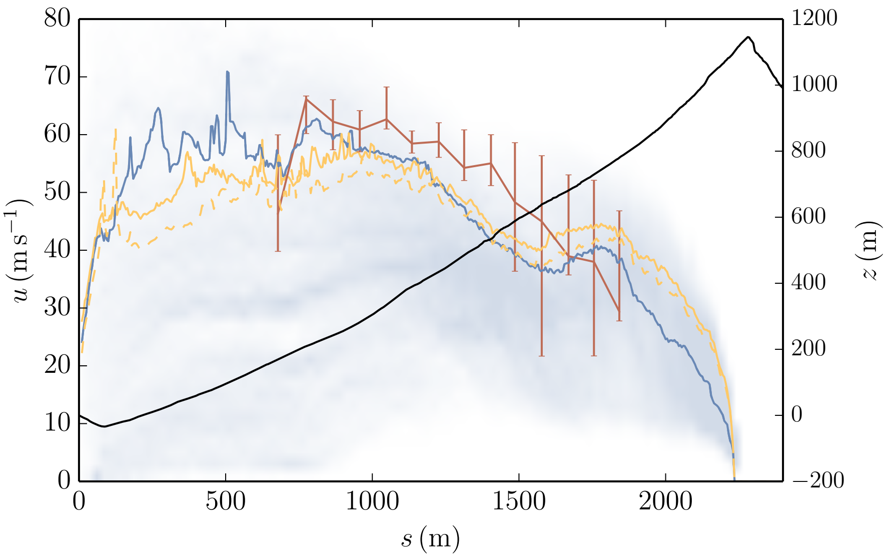

Fig. 4. We will add the path profile to figure 12. Here shown in black for VdlS.