Comments on Reviewer 3 - Anonymous (Referee RC3)

Thus, the paper in many ways centers around the “avalanche truism”: Why did the slide release? Because the friction angle was low. Why was the friction angle low? Because the slide released. The real question is why, what mechanism led to this low angle? And can it be proven, with reasonable speculation, which is basically the science of rock slide geology. This is where the analysis of the modelling methods becomes important. Note the important sentence on line 424: “Based on the results of numerical modelling study it is inconceivable that slope failure occurred under pure static conditions…

Comment: Our intention was not to simulate the progressive failure process characterised by fracture growth and coalescence (for additional information please see comments to Reviewer 2), but to determine the shear strength properties of a fully persistent basal rupture surface when the progressive failure process is completed. But, we fully agree that the question for the mechanism is most important and progressive failure is the main initial failure mechanism. As this is a study investigating an event that occurred around 9,500 years ago, the possible trigger(s) is (are) unknown and thus can only be reconstructed by indirect methods (e.g. lake-sediment based paleoseismology, climatic records). We choose the way to combine (i) estimations of critical values of rock mechanical parameters, and (ii) computer simulations to investigate which parameter values would have been necessary to allow failure under given groundwater scenarios, and (iii) to match the two parts. This is exactly what we did, and for example we found that extreme groundwater conditions may not have triggered the event. In the revised manuscript, we will try to more clearly explain this important aspect of our work.

So, how did the authors model the release dynamics? Firstly, they used DEM methods “to model a thin and discrete basal sliding zone which is able to accumulate large shear displacements” (ll 221). Thus, the failure zone was introduced into the model and “…the main deformation within the system takes place through movement along discontinuities” (ll 241). The selected angle of friction varied between “20deg and 27deg” for scenario A and “25deg and 32deg” for scenario B (ll 260, Section 3.33). It is therefore hardly surprising that the authors obtain a friction angle of 24deg for A and friction angle of 28deg for B (see results in abstract lline 17-20). To me this means that the results the authors obtain arise directly from the selected parameters.

Comment: This is not the case, because we lowered the basal shear strength properties step-wise to reach slope failure conditions (i.e. transition from very small displacements with equilibrium to large displacements where equilibrium can no longer be achieved). Thus the obtained critical friction angles are the results from back-calculations with and without water pressures.

In fact, I question whether a DEM or finite element model can supply other results, especially when the shearing is concentrated in weak shear zones. The elastic shear modulus of this zone was defined to be 22GPa (ll 262). This value is evidently a static value, valid for all time. Maximum modelled deformations in the shear zone are about 0.25m (l 339). The model, thus does not investigate the possibility of shear softening, it is excluded from the modelling a-priori. Therefore, the conclusion the hypothesis that fragmentation or shear softening did not lead the triggering is questionable.

The rock was considered to be discrete blocks with “contacts or interfaces”. However, a “continuum mesh of finite difference zones defines the deformability of the rock mass” (ll230-235). Thus, the model description (for me) is somewhat confusing – was it a continuum model or a discontinuum model? It should be pointed out that modelling a shear interface is possible with standard finite element continuum codes. In my opinion the modelling of the deformability of the surrounding rock is important because it defines how any stress concentrations in the shear layer are carried (or “bridged”) by the surrounding rock. Here, I think additional figures are required showing exactly how the interface and rock-mass are modelled. What are the continuum rock parameters and what are the block interface parameters. What are the deformations in the surrounding rock? Do the discrete blocks rotate, like a layer of ball-bearings? Do they slide? Are all parameters "linear elastic"? According to lines 239+, "Blocks are considered as linear elastic …", there is again no softening, or fragmentation in the surrounding rock. The figures in the paper showing stress distributions are misleading, because the stress concentrations are highly local – perhaps a "zoom-in" to the shear zone where stress concentrations exist, coupled with the bridging stress distributions in the rock massive, would help characterize the failure mechanism.

Comment: Here, we have the impression that our description was unclear and partly misleading. For our revised manuscript we plan to improve the description of the modelling study considerably, including both input parameters and outcomes. We plan to restructure the modelling chapter and will address all the questions and comments mentioned above. Among other aspects, this include for example a clear distinction between constitutive laws and parameters of blocks and structures (i.e. interfaces between blocks).

My final impression of the paper is that it is well-written, certainly of interest to the rock avalanche community. However, the paper applies circular arguments in the modelling – obtaining results that are directly defined by the input parameters and modelling assumptions. This gives the paper a highly speculative character, in which an external hypothesis (earthquake triggering) is advanced to hide the limitations of the modelling effort. From the text, I cannot see the advantages of the discrete element model over a standard finite element approach – especially because both involve adding a "weak" shear zone to the model. How this zone changes (softens, fragments, etc) over time is not considered by the authors, although I suspect the applied DEM model would allow more realistic material behavior.

Comment: Please see also comments to reviewer 2. The primary goal of this study was the back-calculation of the shear strength properties of a fully-persistent basal shear zone. The advantage of applying the distinct element model opposed to classical continuum approaches is, that the "real" geometries of the rock slide can be implemented. Furthermore, we know from other case studies that the active parts of basal rupture or shear zones are very narrow compared to the thickness of entire rock slide. And such localised displacements can be modelled quite well with the distinct element method, especially if larger shear offsets are occurring.

In addition, we think that the continuum approach including some type of strain softening consideration (i.e. progressive failure) is already quite well covered by the work of Brueckl and Parotidis (2001, 2005). They performed 2D FEM calculations (continuum) to study the development of the "creeping rock mass", which represents the initial phase of the Köfels rock slide. In this model a transition of the originally compact rock mass to ''soft'' rock, controlled by a Mohr–Coulomb and no tension yield criterion was assumed. These FEM models focus on the initial failure process by considering progressive failure mechanisms, at least in a simplified way. But pre-existing discontinuities and therefore the anisotropic nature of the rock mass or the failure geometry was not considered.

Thus, the application of the distinct element method in this study is considered to be a useful addition and continuation to explore selected questions and hypotheses. Knowing, of course, that the cause of the Köfels rock slide, and in particular the trigger factors, are still not fully understood.

I would recommend publication if the authors could take the standard linear elastic approach (presented in the is paper) and supplement it with a more complex rock-mechanical modelling, that would either substantiate, or refute, a shear softening hypothesis. Then, and only then, would it be possible to introduce the "dynamic triggering" hypothesis.

Comment: Given that FEM calculations have already been done by Brueckl & Parotidis (2001, 2005), we prefer to remain in the manuscript with the distinct element method. Nevertheless, considerable improvements are planned, and if useful and necessary also new distinct element models. A more comprehensible formulation of the research questions, as well as a comprehensive revision of the discussion, will be sought.