GIS-based topographic reconstruction and geomechanical modelling of the Köfels Rock Slide

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The Köfels rock slide is one of the most well-known rapid landslides in the European Alps. It has fascinated researchers since the work of Erismann in 1977. In many ways this work duplicates the work of many previous researchers – including Erismann (mass balance, energy analysis, theory of frictional heating) and Brückl and Parodidis (finite element modelling). However, instead of applying continuum-type finite element models, the authors use a 2-D "discontinuum" approach using a DEM model.

In general the paper is well-written and well-structured and I believe that readers with a special interest in the Köfels slide will find the paper interesting and worth a short read. Although the paper addresses the speculation surrounding the Köfels event, it does not appear to resolve – with certainty – any of the issues concerning the release dynamics. This, I think, is to be expected and should not be judged too harshly. Note for example, the last sentence of the conclusions, "Additional triggering factors, for example impact of dynamic loading, have to be considered in further investigations. This line summarizes a section in the discussion (ll 435-450) basically advancing the idea that, "dynamic loading due to earthquake shaking was able to trigger tow of the largest rock slides in the Alps, located about 130km apart. The authors then go on with this idea, "…earthquake triggering … would have caused numerous (smaller) events…" and that further investigations comprising a variety of methods should be applied to resolve this hypothesis. The authors (thankfully) exclude climate induced triggers (permafrost warming, ll 425-435), as well as "reducing rock mass strength by rock mass fracturing and weakening …" (ll 414-425).

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 ….".

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.* 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.

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.

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.