

Dear Reviewer 1, dear editor,

We would like to start by reconfirming our gratitude for the reviewer's interest in our paper. Moreover, here are the comments to the additional technical corrections:

**Lines 75, 88, 98, 138 in the new version: is it really velocity or should it be displacement or strain?**

Thank you for your insightful question regarding the use of the velocity field in the upper bound theorem. I would like to clarify the reasoning behind this choice in limit analysis.

The upper bound theorem is fundamentally a kinematic approach. In this context, the use of a velocity field is essential because the method relies on considering possible collapse mechanisms and calculating the energy dissipation rates in those mechanisms. The key is not the magnitude of displacement, but rather the rate at which work is being done and energy is being dissipated as the system moves toward failure.

Both displacement and strain fields describe the positions/deformation of a given point within the material at a particular instant, but they don't directly inform us about how fast collapse mechanisms are evolving, which is crucial for calculating the rate of energy dissipation and external work. In contrast, velocity fields describe how the material points are moving, which directly correlates to the rate of energy dissipation and external work — the two quantities that must be balanced in the upper bound theorem to determine an estimate of the collapse load.

**Line 133/134 in the new version: I think the sentence „Beyond 100000 elements, the meshing process ...“ can be omitted.**

The sentence was omitted.

**Line 230 new version: I would add an s: follows**

Correction has been made

**My comment to line 3 in the original version: Contrary to your statement you didn't follow my suggestion. A model does not develop over a basal detachment.**

In the original version: “Line 3. I would omit the expression “in a kilometric-scale model” at this position or shift it to the end of the sentence in order to avoid strange associations (a model developed over a basal detachment)”

We apologize for the misunderstanding, originally, we omitted the sentence “in a kilometric-scale model” but we did not actually understand that the reviewer wanted an adjustment to the second part of the sentence. We agree with the reviewer, a model does not develop over a detachment, the wordings have been adjusted.

**My comment to line 20/21 in the original version: The references are modified not in all cases (e.g. Lines 22, 26, 27, 28, 30, 31, 33, etc. in the NEW version)**

Again, we apologize for the error and we performed the needed corrections.

**My comment to line 236 in the original version: what I meant was just to speak of deviatoric stress instead of stress.**

Adjustments have been made.

**Captions Figs. 8 and 9: Still not correct, b and d refer to SD**

Corrections have been made

**My comment to line Line 515 in the original version: I don't see any changes. Comparing with the reference below it seems that it should read Krabbenhøft, K., Lyamin, A., instead of K. Krabbenhøft, A. L.**

Corrections have been made

**I just have a final remark considering the purpose and capability of models in general (I expect no action on this one). I'm starting with three citations of yours: In line 266/267 of the new version you write: „We remind the reader that the values obtained may seem very high but they are merely the results of an optimization process through the use of realistic parameters. Nevertheless, these values remain possible in theory.“ Your comment to my comment to Line 364 in the original version: „In theory they are valid and so they should be found in reality (not considering different aspects that might alter their behavior).“ Your comment to my comment on Fig. 11 in the original version: „It is normal for these values to be higher than the ones observed in reality.“ I think it is valid to draw conclusions from models within the framework the model is defined. And this is what you did and have intended. But caution is needed if conclusions on nature are made based on models. The motivation for a model of course is nature. So I find it important to draw conclusions within the model set up and then discuss in what aspects the results and conclusions may or may not reflect nature and if not what may be reasons for that (wrong assumptions, omission of relevant processes, etc.). I like the following two phrases:**

**“Essentially, all models are wrong, but some models are useful.” (George Box)**

**„The purpose of models is not to fit the data but to sharpen the questions.“ (Samuel Karlin)**

Even though the reviewer did not ask for any corrections to be made, we adjusted the discussion paragraph by adding new a new subsection in which we relate our observations to real field observations.

We hope that these corrections are up to the reviewers expectations!