

Response to review AR2

The reviewer's comments are mentioned in black and author responses are in blue.

We thank the reviewer for her/his time and efforts in highlighting parts of the manuscript that require changes and clarification.

1. The assumption of soil cover being comparable in the Pliocene is likely most heavily violated in regions that experienced Pleistocene glaciation. I suggest removing these areas with a 'maximum Pleistocene ice extent mask' from the Pliocene results, similarly to what is also done for the LGM. Both masks should be highlighted in another color than the background grey on Fig. 6 to make it more apparent. It is important to also show them on the FCI difference maps for the relevant time-slices (Fig. 7-10), so you don't compare FCI for regions within and outside of ice-sheets for different time slices. The latter would also eliminate the problem with sentences in e.g., line 306-307 and 317-318, where you seem to be unsure about whether FCI-differences result from ice cover during LGM or not.

This is a very valid and important concern. We addressed this as follows: The areas which experienced Pleistocene glaciation have been removed from the Pliocene results of all the three models. The figures (including captions) i.e. Fig. 6-10 and supplement Fig. 1 and 2 have been updated accordingly in the revised manuscript.

The glacier mask is applied and highlighted in FCI results (with a different color) for all the models (i.e., Fig. 6, 7 and Supplement Fig. 1 and 2) and FCI difference maps (i.e. Fig. 8-10), following the reviewer's suggestions. Hence, we no longer compare FCI for regions within and outside ice-sheets in the revised manuscript.

2. Your soil thickness data seem to saturate/max out at ~1 m (fig. 1). Are these minima estimates? It is not clear to me how you handle soils ≥ 1 m in the FCI model, or if you exclude these (extensive) regions. I would also like to see a discussion on how the uncertainties and coarse spatial resolution of the soil data may influence the modelled FCI on a sub-grid scale.

We consider the top soil (usually ≤ 1 m deep) in the FCI models and hence used the dataset from World Harmonized Soil Database (HWSD) v1.2 (Wieder, 2014) for present day. We do not explicitly define regolith and hence the strata below the top-soil is considered as bedrock in the first order estimation. However, future regional (or local) studies may include deeper soil depths preferably at finer spatial resolution, and extensive lithological information.

A discussion on the influence of uncertainties and coarse spatial resolution of the soil data on the modeled FCI on a sub-grid scale has been added in the model limitations (section 5.5) revised manuscript (lines 526-531).

3. I fail to see the relevance of the comparison to permafrost extent, and suggest cutting these sections out of the paper (Sec. 3.3, Sec 5.4.2, and Fig. 11+12).

We agree that the sections and figures relating FCI to permafrost extent does not add much insight into the discussion section and hence sec. 3.3, 5.4.2 and Fig. 11 and 12 are removed from the revised manuscript.

4. The discussion includes a number of 'predictions' that 'confirms' or 'agrees with' the models (e.g., line 287-288, line 429). These statements appear circular since your results are based on the same models, and does not really add anything new compared to reading the original papers. I suggest that you spend more space on comparing the models and testing the effect of the underlying assumptions in the main paper. For example: it is disputed whether the penalty functions that make FCI depend on distance to water give a better representation of the frost cracking process or not. Since you have gone through the trouble of implementing all three models, it would be interesting to use them to evaluate what predictions about global frost cracking the different choices result in. For example, you could test the effect of the penalty function by running models with and without the postulated influence on FCI, but maintaining the influence of porous (wet) soil on the temperature-profiles. Similarly, your section 5.4.1. would be better framed as an evaluation/discussion of the assumptions behind the different models, rather than evaluating your results directly. At present this section does not really add something new that is not in the original papers, which is why everything ends up being in agreement with your results.

We have updated the discussion section 5.3 to not directly compare our results with previous studies but rather compare the first order (global) trend of FCI with existing observations. Furthermore, we followed your advice to give more room in discussion to the importance of the penalty function. Specifically, we added a section in model results (section 4.2) to present the results for model 3 without the penalty function. We observed that the FCI trends were similar to Model 2 output (FCI as a function of temperature gradient if water is available at either boundary). The globally summed up FCI estimates (with inclusion of penalty function) were in agreement with studies on global weathering fluxes from LGM to present, which indicates 20% higher global weathering rates during LGM (than in present day). We concluded that our results speak in favor of the importance of the penalty function for global scale estimates of FCI. Thank you for the suggested change.

5. Section 4 and 5.1-5.3 and Fig. 7-10. These sections are rather long and hard to read. Please try to condense the most important lessons. I suggest referring to Fig. 8-10 as part of your global discussion in replacement of Sec. 5.3. Perhaps you could even consider showing the FCI-difference panels grouped by time-slice (e.g., PI-MH) instead of region, and then showing the regional details as sub-panels (b-d) to each global model (a) in each figure. This would also reduce the number of figures by one.

Thank you for the comment about the length of section 4 and 5.1-5.3. These have been simplified and reduced to condense the most important findings in the revised manuscript. Section 5.3 is

replaced and combined with section 5.2 (global analysis) following the updating of Fig. 8-10 as per above suggestion.

6. Consider calculating a globally summed FCI to highlight what periods frost cracking globally are more important to surface processes.

Thank you for that suggestion. We believe this is a good way to get additional important lessons out of our study. We have added a paragraph in discussion (section 5.1) (lines 365-383) in the revised manuscript to elaborate on above mentioned suggestion.

Figures, Tables

Table 2: Too many digits?

The digits have been rounded off with no decimal values in Table 2.

Fig. 11+12: Stippled black line very hard to see.

The figures have been removed from the main text following the above suggestion (pt. 3 of reviewer's comments).

Line specific comments

I. 20: 'In contrast' – these sentences does not really contrast

Removed 'In contrast' from line 21.

I. 26: Consider removing 'long ()'

Removed long () in line 28.

I. 29-31: Not so clear why vegetation is considered indirect, but other surface processes are considered direct.

This distinction may indeed be somewhat arbitrary after all. We revised the sentence (lines 31-35 in revised manuscript) to simply that that climate affects erosion through changes in vegetation or other said processes.

I. 70: 'Europe' is not an orogen

Line 79: We removed this sentence, since we simply focus our discussion on specific regions (hosting mountainous terrain), but do not conduct a separate analysis.

I. 74: 'and soil' read strange here

Line 83: 'and soil' has been changed to 'and soil dataset'

I. 96-98: I don't understand the reference soil depth information – consider to cut it unless it is relevant enough to explain in more detail?

Line 122: The reference soil depth information has been removed in the revised manuscript.

I. 145: I would not say more complete, but certainly more complex. It is disputed whether the penalty functions (or the ad hoc choice of parameter values in different media) give a better representation of the frost cracking process than a simpler model.

Line 178: 'more complete' has been changed to 'complex'. We have added a section in Results (section 4.2) and Discussion (section 5.1) to elaborate and discuss the sensitivity of Model 3 (Andersen et al., 2015) to penalty functions, in estimation of FCI at global scale.

I. 181-182: This seems redundant. No need to mention it in each section, and twice in this section (also I. 197).

Removed redundant sentences from line 220 and 235.

I. 186: In the case 'of' permafrost

Line 223: corrected from 'if permafrost' to 'of permafrost'.

I. 187: Fig. 3, not 2

Line 225: corrected from Fig. 2 to Fig. 3

I. 275: discussion 'of' regional variations

Line 352: added 'of' between 'discussion' and 'regional variations'

I. 484: frost cracking 'occurs' at lower latitudes

Line 570: added 'occurs' between 'frost cracking' and 'at lower latitudes'