Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC2
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Comment on esurf-2021-78

Anonymous Referee #2

Referee comment on "The effects of late Cenozoic climate change on the global distribution of frost cracking" by Hemanti Sharma et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-78-RC2, 2022

This manuscript tackles an interesting and understudied question – where and when frost cracking has influenced bedrock weathering on a global scale during the late Cenozoic. The authors have done an impressive job of implementing and testing different existing models for frost cracking. My main concern is that some of their conclusions seem circular or do not add much to the original publications. I think new knowledge can be gained by critically investigating the predictions of frost cracking intensity arising from different assumptions, and that the authors are well-placed to do so here. In the following I therefore suggest a few suggestions to restructure the manuscript.

Main comments

- 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.
- 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.

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

Figures, Tables

Table 2: Too many digits?

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

Line specific comments

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

l. 26: Consider removing ‘long ()’

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

l. 70: ‘Europe’ is not an orogen

l. 74: ‘and soil’ read strange here

l. 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?

l. 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.

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

l. 186: In the case ‘of’ permafrost

l. 187: Fig. 3, not 2

l. 275: discussion ‘of’ regional variations

l. 484: frost cracking ‘occurs’ at lower latitudes