

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2021-78

Anonymous Referee #1

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-RC1>, 2022

The paper "The Effects of Late Cenozoic Climate Change on the Global Distribution of Frost Cracking" by Sharma et al. presents the results of three frost cracking models applied on global scale. The authors used published temperature reconstruction in an 80 to 80 km resolution for four time slices: Pre-Industrial (~1850 CE, PI), Mid-Holocene (~6 ka, MH), Last Glacial Maximum (~21 ka, LGM) and Pliocene (~3 Ma, PLIO) times. These temperatures were used to calculate days spent in the frost cracking window (Anderson, 1998) and two existing frost cracking models (Andersen et al., 2015; Hales and Roering, 2007). The authors analyzed spatial variation of frost cracking intensity (FCI) at individual time slices and observed large deviations of FCI between warmer (PI) and colder (LGM) climate. The paper addresses an important topic about spatial and temporal variation of frost weathering that shapes the Earth's surface. My major concern is that there is no independent data set that enables the validation of the model results. The authors provide three model results but there is no possibility to review the model results for a location where data exist or for different time slices.

The use of paleodata

The study uses paleo-temperatures, which are air temperatures according to the papers by Mutz & Ehlers (2019) and Mutz et al. (2018) and not land surface temperatures as indicated in this paper. Snow cover and vegetation will result in temperature offsets between air and surface temperatures, which will cause large difference in the frost cracking results and are not addresses in this manuscript at all. The paleo-data is available at 80 to 80 km resolution which is much to coarse to apply these to high-topographic environments as the European Alps, Andes or Tibetan Plateau. The coarse resolution is not integrating topographic effects in is not applicable to mountains. The authors should downscale their data, which is a standard procedure in alpine studies (e.g. Fiddes and

Gruber, 2014). The data is available at daily time steps and could be used directly to calculate frost cracking. However, the authors calculate a mean annual temperature and half amplitude of annual temperature. They used sinusoidal daily temperatures but it remains unclear if these temperatures are from the paleo-temperatures or assumed values. A more direct use of paleo-temperatures would be better suited.

Frost cracking models

The authors used three proxies or models for frost cracking but only focus on model 3 in their paper. The days spent in the frost cracking window is only a poor proxy for frost cracking (Anderson et al., 2013). The model by Hales and Roering (2007) is out-dated and not including any lithological differences. Both models are barely used in the results and discussion section, therefore they could be omitted from the manuscript.

The model by Andersen et al. (2015) is applied using soil thickness to constrain a soil layer with a assumed porosity of 30% which is located above a bedrock layer of 2% porosity. The soil thickness is derived from a global database with 5 km resolution and used for every time slice, however, it is unrealistic that soil thickness is a constant over Cenozoic time scales. The substrate classification into soil and bedrock changes water flow in the subsurface within the frost cracking model. For alpine regions the database provides relative high soil depths, however, rockwalls with 30 % porosity are not existing, which highlights the problem of spatial solution and applicability of this model in this way to alpine conditions using a soil map. In addition, the model by Andersen et al. (2015) uses a fixed frost cracking window between -8 and -3 °C that is not supported by laboratory data (e.g. Murton et al., 2006), field data (Girard et al., 2013) or physical models (Walder and Hallet, 1985). As lithology and rock strength show variations across the Earth, lithology will control weathering, which could be incorporated to include more realistic results.

Glaciation

The authors provide a glacier mask in the supplementary and compare this mask to FCI. The glacier mask is not including any glaciations in the European Alps during LGM or 1850 (Little Ice Age). On which scientific basis is the map derived? Why are the authors comparing the spatial distribution of FCI with their glacier mask? When a glacier is there, then there is no frost cracking as the rock is disconnected to atmospheric processes (Grämiger et al., 2018). By not including a glacial cover, the authors are overestimating the FCI by far.

Scale issues

The authors use a simple bottom-up approach to model frost cracking for different time slices. They have no independent data that they could use to validate their models. Consequently, they have a problem to discuss their own results and put them into a perspective. They compare a 80 x 80 km model for North America and Alaska for PI, MG and PLIO to a frost cracking studies at Jungfrauoch that measured frost weathering using acoustic emissions on one rockwall at 3500 m for 4 days (Amitrano et al., 2012) or one year (Girard et al., 2013). I cannot see how these studies support the author's results on much larger scale at different time steps in the past in completely different environments. Furthermore, the author states that their model results at higher Asia and Alaska during LGM are consistent to periglacial processes observed in Oregon (Marshall et al., 2015; Marshall et al., 2017). I cannot see the context between periglacial conditions and landforms in Oregon and the author's observed FCI in other areas of the Earth. These are just a few examples but the whole discussion shows no argumentation. Model results will be compared to models from Hales and Roering (2007) or Andersen et al. (2015), which are used to derive the same model results.

See also detailed comments in the attached pdf.

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Please also note the supplement to this comment:

<https://esurf.copernicus.org/preprints/esurf-2021-78/esurf-2021-78-RC1-supplement.pdf>