Climate of the Past Discussions

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Comment on cp-2021-60

Anonymous Referee #1

Referee comment on "A new perspective on permafrost boundaries in France during the Last Glacial Maximum" by Kim H. Stadelmaier et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-60-RC1, 2021

General comments:

The general idea of the paper, i.e. testing climate model outputs for the LGM with the spatial distribution of permafrost features is very good. Not being a climate modeller, I cannot criticize very much that aspect of the paper. However, I suggest that the paper could be significantly improved by providing better information from the literature and the co-author's knowledge about the current climate-driven mechanisms that drive active processes, particularly frost cracking, ice wedge activity, growth and decay and cracking in non permafrost regions. A more thorough and clear presentation of the current driving factors and resulting features would help the reader understand the results of the research in the modelling context. This would also benefit the discussion section.

The authors could also justify some of their methodological choices. E.g. why those specific values for frost cracking (-5 °C at 1 m and -7 °C/m gradient) and not others? Why extract SFIs at 0,7 m and not in the air?

Several statements need to be clarified. See detailed comments below.

Detailed comments:

Abstract:

I suggest that the abstract should state more clearly what the objectives of the research are. They could (?) be stated as: 1- to evaluate the potential of regional climate model simulations to reconstruct the permafrost distribution in western Europe during the LGM, 2-apply to modelled data the experimentally known parameters for frost cracking to LGM in Europe. 3-identify the regional climate model of the LGM that best reproduces permafrost distribution and soil thermal contraction conditions as represented by fossil ice wedge casts and sand wedges.

Lines 8-9 : “Whereas” …the meaning of this sentence is confusing. Do you mean that the global model for the LGM does not fit with permafrost distribution and ground cracking conditions worldwide but that a regional model does it for Europe? I do not understand well here. Say in a better way.

Line 11: Thermal contraction cracking occurring south of permafrost zone: This does not come as a surprise. Even nowadays, we get ground temperature conditions for frost cracking at places south of the permafrost region. But how deep is a question. It depends on the maximum depth reached by the freezing front locally.

Introduction:

Lines 17-20; current thawing of permafrost and carbon feedback. This is not pertinent for this paper. I suggest delete those lines.

Line 23; replace “under” by “against which” they are well tested

Line 38; add "a " colder, drier… period

Line 59-60: provide a better explanation for ice wedge growth and origin of ice wedge casts: annual frost cracks that reach downward into the permafrost are a few mm wide. They get filled with snowmelt water that freezes into ice veins. Repeated cracking over years at the same location add ice veins that constitute ice wedges. Wedge casts (pseudomorphs) observed from the LGM in Europe were formed when the ice wedges melted and the cavities were filled by collapsing soil materials. I suggest you cite here Harry and Godsik (1988).

Line 67-70: weird sentences here: limiting factor? This is complex language. Thermal contraction cracking is the causal factor that leads to ice wedge growth (see above); it is when the conditions for thermal contraction are not met that there is no cracking ... Ecological factors such as type of vegetation cover and thick snow cover often limit thermal contraction cracking when they prevent the cooling of the ground. Note that frost cracking also occurs widely in cold environments in roads and airport runways.

Line 81: considered

Lines 80-96: This paragraph is confusing. All approaches to map permafrost temperatures (ex. 1D gridded models, TTOP and others require that a thermal offset factor (ex. n-factors) be applied to air temperatures (freezing and thawing degree-days) to map soil surface temperatures. SFI also. At line 93, without any context, I do not understand what is the deepest ground layer and why 5,7 m deep? Deepest relative to what? the deepest depth applied in Stendel & Christensen's model? Temperature in permafrost at that depth may be good to monitor or predict changes, but it tells little in terms of permafrost type or distribution contrary for instance to depth of 0 ºC thermal amplitude.

Lines 100-103: I suggest to rewrite the objectives as suggested as above in the abstract section. For instances objective 3 should be reformulated: We already know that thermal contraction is the process that drives cracking and ice wedge development. Should not the objective be to test how the spatial distribution of conditions for thermal contraction cracking modelled with a regional model of the LGM fits with observed distribution of relict wedge casts and relict frost cracks ???

Data and methods:

Lines 110-139: I am not competent to criticize the choice of global and regional models.

Lines 140-143: The definition of permafrost zones according to MAATs at 2 m provides a good estimate of permafrost distributions. The SFI maybe does a somewhat better job to meet your objectives.

Line 149: SFI calculated from outputs at 70 and 78 cm are not the same as the original SFI concept calculated from air surface temperature. The selection of this depth in this paper needs to be justified. Why not nearer to the surface?

However TTOP is also based on DDF and DDT and of easy use over a gridded domain. You could apply some general soil data to infer soil thermal conductivities. The maps of permafrost distribution and temperature in the LGM in Europe would likely be better. See Way and Lewkowicz 2016 in Canadian Journal of Earth Sciences for an actual modelled application over Quebec-Labrador. (Maybe for another paper?)

Lines 151-154:

Why do you retain only those values of -5 °C at 1 m and -7 °C/m as thermal gradient? They occur only for one cracking event at one of Matsuoka et al’s three measurement sites in Svalbard. They have another site with values as modest as -2,8 °C and 1.1 °C/m. Their general (averaged) values for frost cracking events to occur are -20 °C at the ground surface, -10 °C at permafrost top (or 1 m deep) and a gradient Ë☐ -10 °C/m.

Did you make any calculation (interpolations) to adjust the selected 78 cm depth output of your climate models to the general or the minimal values of Matsuoka et al.?

It should also be explained in the paper that these values apply to some single frost cracking events over periods of about 3 days in a given winter.

Those values should lead to frost cracking in soils both in permafrost regions (often) and in non-permafrost regions (occasionnally). Pseudomorphs were ice wedges in permafrost. Small frost cracks may have occurred in seasonally frozen ground. A clear explanation in the paper would better support your interpretations.

Results:

We understand the WRF-MPI model fits better with field observations of pseudomorphs and fossil cracks. An interesting result.

Line 210: the 70 cm depth is representative of what? I rather understand it is the depth you selected in the model output to check against SFIs that are surface values. I suggest you write "selected" or "depth chosen as representative."

Line 243: "The SFI is suitable to infer LGM permafrost from model data". With your simulated climate data, you might have had better results in representing permafrost distribution with the TTOP model (based also on freezing and thawing degree-days). This would have allowed you to map temperatures at the top of permafrost over the regional domain and compare it with air temperatures at 2 m above ground, + calculate the surface offset. By selecting a value of about -6 ºC for TTOP, you might be close to the southern limit of ice wedges active during the LGM

Lines 255: again, here I do not understand your concept of "limiting factor". Thermal contraction cracking is the CAUSAL factor for developing ice wedges in cold enough permafrost AND shallower thinner sand wedges above warm permafrost and in the seasonal frost zone. Simply put, thermal conditions for frost cracking were present in the LGM. I suggest just avoid too much language complexity.

Line 256-258: " ice wedges would have developed in the discontinuous and sporadic permafrost zone, with the limiting factor being only the ability for ice to be preserved from year to year". This sentence lacks logics. If ice veins melt one year or another then forget about development of ice wedges. However, in limiting conditions, ice wedges may crack only every other year or become dormant in warm permafrost for periods without melting.

For ice-wedges to form we need cold enough permafrost temperatures (around -10 ºC) in winter so that the cracks propagate to depths of several meters. The coalescent veins (the wedge) do not melt BECAUSE they are IN the permafrost.

Line 256: why "densely vegetated Arctic areas"?? ice wedges are found under various tundra types (polygonal, tussocks, moss, lichens, patterns of lichens and shrubs, etc.)

Figures:

Figure 6: SFI applied at the 70 cm depth . In the original concept, SFI is based on degree-days in air temperatures (2 m above ground.) Then why not present a permafrost map based on SFI with air temperatures. Similarly why not show a permafrost map based on MAATs for comparison (as in lines 140-143). Also, it seems to me that the size and shapes of the sand wedges from Andrieux should be mentioned in the text. Large sand wedges could have developed in dry very cold environment (polar desert conditions) but thin frost cracks could have open in discontinuous permafrost or even in seasonally frozen ground. I also think that reasons why the more or less good fit between spatial distribution of fossil features and (even) the best option of permafrost map should be discussed in the paper.

Figure 7: values (months?) are needed on the time scale.