Author's responses

Dear Dr. Wickert,

On behalf of the authors, I am pleased to submit the second revision of our manuscript ID gmd-2020-5 entitled "PERICLIMv1.0: A model deriving palaeo-air temperatures from thaw depth in past permafrost regions" compiled by Tomáš Uxa, Marek Křížek, and Filip Hrbáček.

We responded point-by-point to all referee #1 comments and made corresponding adjustments in the manuscript. The referee #2 provided comments and suggestions inserted directly in the manuscript. We also followed most of them, which can be seen in the revised manuscript with tracked changes. The reasons why we did not take some of them into account in the text are outlined below for these specific comments and suggestions. Finally, we made several other minor modifications in the manuscript, which you will find using the tracked changes.

All the co-authors have read and approved the manuscript prior to its resubmission to Geoscientific Model Development. No part of the manuscript can infringe upon existing copyrights in any way. We have no conflict of interest to declare.

Thank you very much for reviewing the revised manuscript.

Yours sincerely,
Tomáš Uxa

Author's response to comments of Referee #1

AC: We thank the referee #1 for the detailed of our manuscript.

RC1: This manuscript introduces and demonstrates the validity and sensitivity of a new simple inverse modelling scheme based on Stefan's equation to calculate palaeo-air temperature characteristics from the palaeo-active-layer thickness observed in the past permafrost regions. The revised manuscript has substantially been improved from the original, responding in a sufficient manner to the comments and suggestions raised by the referees. In my opinion, the manuscript is close to acceptance for the publication at the GMD journal after the following issues are adequately addressed and clarified.

Major concerns:

1. "Driving parameters"
It would be strongly suggested to avoid usage of the inclusive term "driving parameters". From a numerical modelling point of view, the variables collectively categorized as "input" in Table 1 or mentioned in the text are categorized into qualitatively different groups, i.e., those to set up a model, those to control the model, and those fed to the model to produce an output [although it may appear just one group from a field scientists' perspective as they are measured and observed at the same time at the field sites, except for "annual air temperature range")]. Nevertheless, lack of clear distinction between these groups appears to lead to confusions in the analysis and/or interpretations in Sections 4 and 5.
Of the variables listed as "input" in Table 1, moisture content to thawing n-factor provide the site-specific information in this model's framework, and actually determine the condition of the (under)ground at which a periglacial feature occurs. They deserve to be called "parameters" as they determine the shape and functionality of the model. To the contrary, active layer thickness is a result of action that occurred at such a place as set by the above "parameters" under a certain climate condition (i.e., thermal, in this model's case), which is the targeted output of the model. Thus, it works as the "input" or driving term of the model.
Temperature range may be called a controlling parameter to the MAAT as the two of them cannot be determined uniquely by Ita alone (this is also relevant to the next issue).

AC: We have encountered the terminology used in some other articles as well. Nonetheless, we accepted the referee's arguments and substituted the term "driving parameters" with "input variables" throughout the whole manuscript.

RC1: 2. Functionality of "annual air temperature range" Aa
When deriving MAAT from Eq (5 to 7) under the given value of Ita, it is trivial that MAAT decreases as Aa increases (leading to increase in the absolute value of Ifa, decrease in MATCM and MATTS, and increases in Lf), in which the value of Aa directly controls the output. From this point of view, the arguments in Section 5 (namely, ll. 367–372) look off the mark. In contrast, the modelled MATCM (or other variables related to the freezing or cold season) can be used to evaluate the plausible value of Aa. For example, the argument shown in section 5.2 (ll. 348–350) could be reversed to discuss possible inference on the annual air temperature range that best explains the value of MATCM (or similar variables) derived from other proxies (e.g., -27 to -16.5°C in case of Central European lowland).

AC: We agree that the described relations are obvious from the equations themselves, but it may not be clear to everyone. Moreover, even though it is evident that the annual air temperature range controls the outputs, we do not want to stay on that statement, and we intend to quantify how it affects these outputs and what are its relations to other inputs. This is why we left the section (ll. 367–372) as it was.

As for the annual air temperature range, its scenarios were limited from above on the basis of maximum values indicated by other proxy-records. From this point of view, the scenarios should be plausible (or cannot be made more plausible based on these other proxies).

RC1: 3. Evaluations on the empirical reconstruction methods.
(This is more or less a diplomatic suggestion.) In Abstract and Introduction section, the authors state that their model is aimed to overcome the "flaws" of the empirical methods which are "far from reliable". Yet, the evaluation of the model performance did rely on the outcomes from the empirical methods (ll. 11–12, Section 5.2). Although an assertion of novelty and superiority of the new method is understandable, it doesn't appear fair. The spirit of the new model should lie in its capability to provide more verifiable reconstructions "in a replicable and subjectivity-suppressed manner" (l. 419).

AC: We agree, which is why we completely removed the word "flaws" and "flawed" from the text or changed the statement so that it tells that the empirical methods are problematic. Likewise, the phrase "far from reliable" was substituted with "have a/be of limited validity".

RC1: Minor issues/technical issues:
ll. 19–20, "Commonly,… of past environmental conditions": any reference to support the sentence?

AC: We added two citations (Washburn (1980) and French (2017) [see manuscript for both citations]) that support that statement.

RC1: l. 102: "the number of inputs" should be small.

AC: We changed it to "the number of inputs small".

RC1: l. 121: Should be Eq (5 to 7) to include boundary conditions.

AC: Note that the equations (6) and (7) have a solution for $-A_a / 2 \leq \mathrm{MAAT} \leq A_a / 2$, but we target exclusively those mean annual air temperatures that are below or equal 0 °C (~permafrost conditions), and thus we solve the equations only for $-A_a / 2 < \mathrm{MAAT} \leq 0$. This is why we think that the inclusion of the boundary conditions into the equations (6) and (7) could be more confusing than explanatory. We moved the section stating that the equations are solved for $-A_a / 2 < \mathrm{MAAT} \leq 0$ (originally in the next paragraph) just after these equations, which should be enough to clarify this matter.

RC1: Table 2: It would be good to provide the number of samples (or sampling points).

AC: We tried to implement the numbers of samples into the Table 2, but because the numbers are mostly the same for both locations or for multiple variables and also because the table did not fit the page in that case, we decided not to include them into the table. Note, however, that the numbers of samples are mentioned in the surrounding text (now also for the palaeo-active-layer thickness).

RC1: l. 230, "supposed to be representative for former conditions as such": not clear. Meant something like "supposed to be unchanged from the time of cryoturbation"?

AC: The text was changed to "supposed to be unchanged since the cryoturbations developed."

RC1: Section 5.1: It should be mentioned in the preamble that this section considers the results of Section 3

(present-day application).

AC: We added a reference to the Section 3 and also slightly changed the text in the preamble as follows: "Generally, the model validation using the modern data (Sect. 3) showed …"

RC1: l. 294, l. 416: How is the "success rate" defined and evaluated?

AC: We removed this phrase (including the abstract) or substituted it by the word "accuracy".

RC1: ll. 311–314, ll. 362–365, ll. 412–415: Sentences are too long, and not clear.

AC: The first sentence was simplified as follows: "Surely, the Stefan equation (Eq. 2) might be improved by a number of correction factors, but these require additional inputs, such as frozen thermal conductivity, thawed and frozen volumetric heat capacity, or active-layer temperature at the start of its thawing (Kurylyk and Hayashi, 2016)."

The second sentence was simplified as follows: "Global sensitivity analysis using multiple regression suggested that the palaeo-active-layer thickness and annual air temperature range had a major impact on the modelled palaeo-air temperature characteristics at the Brno–Černovice and Nebanice site (Fig. 7). The palaeo-active-layer thickness importantly showed the highest values of the standardized regression coefficients (SRCs) especially for the annual and thawing-season air temperature attributes."

The third sentence was shortened to: "The palaeo-MAAT modelled for two sites in the Czech Republic hosting relict cryoturbation structures was between −7.0±1.9 °C and −3.2±1.5 °C and its corresponding reduction was between −16.0 °C and −11.3 °C in comparison with the 1981–2010 period, which is relatively well in line with earlier reconstructions utilizing various palaeo-archives."

RC1: Section 5.2: It should be mentioned in the preamble that this section considers the results of Section 4.3 (palaeo application).

AC: We added a reference to the Section 4.3 and also slightly changed the text in the preamble as follows: "The palaeo-MAAT modelled for two sites in the Czech Republic (Sect. 4.3) was between −7.0±1.9 °C and −3.2±1.5 °C …"

RC1: ll. 357–360: Additional evidence or arguments would be required to support or substantiate the claim that not the model outputs but the empirical MAAT thresholds are to be revised.

AC: The text was changed as follows: "This study thus raises questions about the validity of the previously suggested MAAT thresholds for cryoturbation structures (see Vandenberghe, 2013; French, 2017) and calls for their thorough revision."

RC1: Section 5.4: It would be suggested to modify the title, for example, "Limitations and applicability of the model".

AC: The section title was changed to "Model applicability to periglacial features".

RC1: l. 381: "However, it can also be easily adapted for seasonal-frost features": It won't be that "easily". Basically, adaptation will be a mirror image (e.g., changing the suffix *t* to *f*), but the estimation and validation of

snow conditions (or freezing n-factor) can still be complicated.

AC: As the referee also states, the adaptation for freezing conditions itself would be simple, but we agree that the estimation of snow conditions via the freezing *n*-factor would be complicated. We added it into the text, which is now as follows: "Also, it could be easily adapted for seasonal-frost features, although the estimation of snow conditions would be complicated."

RC1: l. 383, "involving natural climate as well as active-layer thickness variations": Suggested to revise, e.g., "involving natural variations in climate as well as in active-layer thickness"?

AC: The sentence was changed as suggested.

RC1: ll. 388–390, "some periglacial features,… microstructures": "small-scale periglacial features" would suffice.

AC: We changed to "Indeed, some periglacial features may be produced …"

RC1: l. 398: What does "co-occurring periglacial features" mean? Periglacial features occurring side-by- side?

AC: Yes, it should mean periglacial features that occur side-by-side. We changed it to "coexisting periglacial features".

RC1: l. 401: "a more complete" to "an abundant"?

AC: We left this part of the text in its original form as it should mean that periglacial assemblages of different ages could provide a longer history of past temperatures (that is, a more complete [comprehensive] record).

Author's response to comments of Referee #2

AC: We also thank the referee #2 for the review of our manuscript.

RC2: l. 20: Suggestion to remove "…, or emerged at different times, …"

AC: We left this section in the text because permafrost-related features frequently form in extreme environments where vegetation can be sparse or absent (Washburn, 1980; French, 2017; Ballantyne, 2018 [see manuscript for all citations]). Consequently, there may be a limited amount of, for instance, biological palaeo-indicators for periods when the features formed, although these bio-indicators may be abundant for younger periods.

RC2: l. 30: Agreement with that the presence and distribution of periglacial features also depends on such factors as ground physical properties, hydrology, topography, or ground-surface cover: "Yes. The problem is that modeled permafrost temperature relates closer to air temperature, but the correlation between MAAT and ALT is much weaker. See paper of Wang et al., 2019: Wang, K., Overeem, I., Jafarov, E., 2019. Sensitivity Evaluation of the Kudryavtsev Permafrost Model. Science of the Total Environment. DOI: 10.1016/j.scitotenv.2020.137538"

AC: Given that the referee only agreed with that statement, we did not make any changes in the manuscript based on this comment.

RC2: l. 55: Suggestion to remove "palaeo-environmental significance as well as".

AC: This is largely subjective, but we left this phrase in the text because we believe that active-layer features (~permafrost features) are indicators of at least former presence of permafrost and temperatures below 0 °C, whereas seasonal-frost features frequently occur in places where seasonal freezing is still active today and they also have no uppermost temperature threshold.

RC2: l. 64: Comment to the statement that active-layer thawing is governed by ground-surface temperature at the surface boundary in the Stefan equation: "this is a big shortcoming too, ignores vegetative top layer and snow effects".

AC: Subsurface heat transfer in perhaps all analytical as well as numerical models is driven by ground-surface temperatures, which can be obtained by in situ measurements or via additional model components solving the energy exchange between the ground surface and atmosphere in various ways. This is done using so-called *n*-factors that convert between ground-surface and air thawing index in our model. Similarly, this simple but reliable *n*-factor approach was extensively utilized elsewhere mostly by analytical but also numerical models (e.g. Etzelmüller et al., 2011; Westermann et al., 2015 [see manuscript]; Obu et al., 2019). It is thus definitely not true that our model ignores surface effects.

Etzelmüller, B., Schuler, T.V., Isaksen, K., Christiansen, H.H., Farbrot, H., and Benestad, R.: Modeling the temperature evolution of Svalbard permafrost during the 20th and 21st century. The Cryosphere, 5, 67–79, https://doi.org/10.5194/tc-5-67-2011, 2011.

Obu, J., Westermann, S., Bartsch, A., Berdnikov, N., Christiansen, H.H., Dashtseren, A., Delaloye, R., Elberling, B., Etzelmüller, B., Kholodov, A., Khomutov, A., Kääb, A., Leibman, M.O., Lewkowicz, A.G., Panda, S.K., Romanovsky, V.,Way, R.G.,Westergaard-Nielsen, A.,Wu, T., Yamkhin, J., and Zou, D.: Northern Hemisphere permafrost map based on TTOP modelling for 2000–2016 at 1 km^2 scale. Earth-Science Reviews, 193, 299–316, https://doi.org/10.1016/j.earscirev.2019.04.023, 2019.

RC2: l. 240: Comment to the statement that treeless landscapes dominated in South Moravia and West Bohemia at the time when cryoturbations formed: "you may want to emphasize this approach a bit more in the discussion. Perhaps local pollen records can always be part of the puzzle and give better idea of snow thickness and vegetation effects?"

AC: We agree that pollen records could give a better idea of vegetation type and coverage, which might be useful for thawing *n*-factor estimation. However, the analysis should ideally be done on the same profile where cryoturbations occur, which is a good suggestion for follow-up detailed studies, but we did not do it in this study, which should chiefly describe and evaluate the model. Instead, we relied on a general lack of organic remains within the cryoturbated horizons (this argument was added into the revised manuscript) and regional pollen-based reconstructions (Kuneš et al., 2008 [see manuscript]).

RC2: l. 403–405: Suggestion to remove "Nonetheless, this shortcoming is also increasingly being suppressed by improved dating methods that bring more reliable periglacial chronologies (e.g., Andrieux et al., 2018; Nyland et al., 2020; Engel et al., in print)."

AC: We left this sentence in the text because it can show readers thinking that periglacial features cannot be dated that it is becoming more reliable than before. Indeed, dating now provides meaningful ages that can be combined with derived palaeo-air temperatures.