“PERICLIMv1.0: A model deriving palaeo-air temperatures from thaw depth in past permafrost regions” by Tomáš Uxa, Marek Kˇrížek, and Filip Hrbáˇcek.

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 I_{ta} alone (this is also relevant to the next issue).

2. Functionality of “annual air temperature range” A_a

When deriving MAAT from Eq (5 to 7) under the given value of I_{ta}, it is trivial that MAAT decreases as A_a increases (leading to increase in the absolute value of I_{fa}, decrease in MATCM and MATTS, and increases in L_f), in which the value of A_a 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 A_a. 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℃ in case of Central European lowland).

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).

Minor issues/technical issues:

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

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

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

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

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"?

Section 5.1: It should be mentioned in the preamble that this section considers the results of Section 3 (present-day application).

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

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

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

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.

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

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.

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”?

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

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

l. 401: “a more complete” to “an abundant”?