"PERICLIMv1.0: A model deriving palaeo-air temperatures from thaw depth in past permafrost regions", by Tom.š Uxa, Marek K˘r.žek, and Filip Hrbá˘cek

General comments

The discussion paper describes a modelling scheme to inversely estimate the near-surface atmospheric thermal states from the thaw depth information under permafrost conditions, primarily employing the idealized relationship derived from the Stefan formula, and discussed the applicability of the model to infer the past thermal conditions from the relict periglacial features, namely, active layer thickness. The authors demonstrated the efficiency of the scheme to inversely estimate the temperature characteristics, such as mean annual air temperature, and mean air temperatures of coldest and warmest months, from the active layer thickness observed at the Antarctic and Arctic sites.

The relationship expressed as the Stefan solution, Equation (1) in the text, is widely known for its useful simplicity but also tendency for biases when applied to real observations, as partly stated in the discussion paper. Still, the author proposed a new and intriguing idea to apply the relationship as a modelling framework to infer the paleo-thermal conditions at the formation time of the currently relict periglacial feature, which can be very relevant to geoscientific modelling within the scope of GMD, as well as of paleoclimatology and cryosphere-related science. In the current form of the paper, however, the explanation and evaluation of the modelling scheme are confusing, or poorly written in term of model description, and the title and the target of the paper show substantial mismatches with the current structure of the paper. Thus, the reviewer believes that the manner conducting the model evaluation, as well as overall organization of the presented text need substantial revisions (appreciating the effort that the manuscript went overall rearrangements for a model description paper), as well as the way the model application was conducted and presented in the Result section.

Two major suggestions are:

1. Move the subsection 5.4 to the Introduction section to describe the previous studies, or motivation of the study, and reconstruct the whole text to fit to a description paper of a model to be used for paleo-temperature reconstructions.
2. Add a model validation case. Use only those terms and variables that would be available and used in paleo application cases (for example, seting P to 365 days, and Aa as described in 5.2.3, etc.), and see how the computed atmospheric thermal states compare to the observed. Also, sensitivity tests for parameters (eg, physical properties; thawing n-factor) to evaluate the range of variations in the computed temperatures would be very informative addition to discussions in 5.2.

Specific comments

Abstract:

The title and abstract claim that this paper introduces a presented model is to be used for paleo-temperature reconstructions. The reviewer feels that the authors' intention and the current structure and the way the model was run and evaluated have a large gap, and suspect that the current evaluation against modern temperature records could not serve properly to judge the model's ability when applied to the past periglacial features. The performed evaluation seemed to use information that could not be available for the paleo cases.

P. 2, ll. 46-48: This comment is related to the above one. If the paper is intended to introduce a model, it should show clear structure of the model, preferably with a simple schematic diagram, to show, for example, what are the input variables to the model; what are the parameters to be set or assumed; and what are the output variables that the model produce. The current paper appears merely to demonstrate a Stefan-based calculation scheme using the observed temperature and relevant data. Thus, if it is intended to evaluate the model to infer air temperature characteristics from past periglacial evidence, it should demonstrate the way the model would perform when applied to paleo cases (See the related comments in the "Result" section). An example of a modelling scheme for paleo application would be like:

[Input] active layer thickness

[Parameters to be determined, assumed, or deduced] thermal conductivity (thawed), wetness, thawing n-factor, length of the period (fixed at 365 days), annual air temperature amplitude

[Outputs] thermal conditions and related information (MAAT, MATW/CM, MATT/FS, Lt, Lf…)

P. 4, l. 84 (and others), "amplitude":
This is merely a suggestion. Use of a word "range" to denote a margin between the lowest and highest values, maybe useful to distinguish *Aa* and *Aa*/2 in the text.

P. 5, ll. 98-106: Estimation of MAAT and *Aa* appears the key, or the central part of the model when applied to the paleo settings when no a-priori thermal knowledge is available. Under the current modelling framework, when one assumes a sinusoidal annual temperature change, and has the *Ita* value as the area under the curve for the positive values, MAAT and *Aa* can be determined independently. In the current form, it is not clear if *Aa* is a parameter or an output variable in the model. So, it definitely needs more elaboration to describe how to calculate (or estimate) MAAT and/or *Aa* (with this in mind that this modelling scheme is to be used for the paleo applications).

P. 5, ll. 105-106, "However, potential drawbacks can be easily handled if exclusively permafrost-related features are examined.": It is not clear what is intended to say.

P. 6, Figure 2. It would be very user-friendly to describe how to draw relevant information from the figure, for most of the GDM reader won't be familiar with this nomogram.

P. 7, Table 2, and "Model validation": Please comment on the applicability to "Berry Hill slopes" in the text (other James Ross sites appear on the flat locations).
Applicability of the Stefan solution may be limited to those sites with high vertical heterogeneity, and it is mentioned in the text for the Alaskan site. Applicability to sites with large lateral flows of heat or water would also be limited.

P. 7, ll. 128-129, "The Stefan equation": Does it mean Eq. (1)? If so, please add the notation (similar to p. 10, l. 212). Also, it is not clear, what are the difference between the results of what this sentence means, and what the later demonstration of the model in "Result" section.

P. 8, ll. 162-163: It is curious if the results from the "successive wet and dry weighing" and the TDR probes are consistent to each other, or were independently done and not comapred (eg, Alaskan sites were solely done by the former, and the James Ross by latter). Is it possible to mention the representative of the results to be applied to the entire thawed layer?

PP. 8-10. Although not clearly written, it seems that the results shown in this section used some of the temperature information obtained from the observations (for example, *P* and *Aa* as shown in Table 2, which claims "model-driving" parameters). If the purpose of the paper is to demonstrate the ability to reconstruct the temperature conditions derived solely from the geomorphological evidences (that is, depth of the active layer), the evaluation should be done in the same manner as to be intended for the paleo cases. This means to run the model with the input (depth) and assumed parameters (thermal conductivity, wetness, thawing n-factor) only. Otherwise, it appears just a mere application of the calculation scheme using advantage of the present-day observations.

Subsection "5.1 Model uncertainties,…":
This subsection needs to restructure the organization, and clearly reformulate sentences. There are many long sentences with unclean meaning (for example, ll. 222-224, 230-233, 235-237). Also, the discussion sometimes goes back and forth, right and left, with reservation and euphemism. One suggestion for a re-organization would be to first divide the discussion to "strength of the model" and "weakness of the model", and prioritize the issues under each of the categories.

P. 10, l. 214, "Also, it assumes that the frozen layer is at 0 ^{o}C before thaw.": This looks pre-assumed in the derivation: that is, what is at stake is the temperature at the freezing interface between the thawed and frozen layers, which should be 0^{o}C.

P. 11, 225-227: It seemingly depends on whether MAAT or MATWM (or MATTS) is at question (cf. Figure 3).

P. 11, l. 231: It is not clear what is meant by "which"

P. 11, l. 236: Not a clear sentence. Are the sites "far from being saturated" those in James Ross Island, and sites of "two-layer" those in Alaska? What (or which) does "there" actually mean?

"5.2 Driving data": Maybe a source of confusion in reading the discussion paper is that it is not clear what are the input (driving) data, what are the parameters or boundary conditions that set the calculations, and what are the output of the model to be applied for the paleo setting. What is discussed in this subsection is either parameters (ie, ground physical properties, thawing n-factor) or a part of the output (temperature amplitude), otherwise it does not make sense if the target temperature information is a driving data.

P. 13, l. 278, "it is possible to assess the extremes, between which the moisture likely occurred": Not clear what is meant.

P. 14, l. 288, "these correlations": which correlations between what? Please describe clearly.

“5.3 Implications for paleo-temperature reconstructions”: It would be suggested to rename the subsection title, something like “Applicability to periglacial features”.

P. 15, ll. 329-331, “we hypothesize that their depth probably rather reflects the position of a transient layer where the contact between the active layer and the uppermost permafrost at the time of their formation oscillated”: It is not clear what this sentence is meant.

P. 15, l. 331 “the latter”: It is not clear what is indicated.

P. 15, l. 347, “random-sampling methods”: how the methods work in the context? With no information, it is not possible to judge the adequacy of the methods.

“5.4 Progress over previous attempts”: the content of this subsection should be placed as “motivation” or “previous studies” in the Introduction, and the whole paper should be structured to “introduce and evaluate” a model to infer air temperature from the paleo-periglacial feature. It is strongly suggested that the overall organization and structure of the paper should be revised.

The authors gave proper credit to related work and clearly indicate their own new/original contribution. And the number and quality of references appear appropriate.