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The Cryosphere Discussions

Interactive comment on "Present and LGM permafrost from climate simulations: contribution of statistical downscaling" *by* G. Levavasseur et al.

G. Delisle (Referee)

georg.delisle@bgr.de

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There are two recurring statements in the paper, I have trouble with.

1) Several times the authors consider permafrost to exist "in equilibrium" with the current climate or to have existed in "equilibrium" during the LGM. Neither during pre-industrial times, today or during the LGM did we have equilibrium. All we can say is that there was and is a quasi-steady state of some sort. Example: the climate signal of the Little Ice Age has certainly not yet reached in some places the base of permafrost, let alone is equilibrated into the overall system. Likewise the LGM was preceded by a long episode of steady cooling. It would take thousands of years of constant air temper-

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atures (especially in Siberia) during the LGM to reach equilibrium. Those thousands of years of constant temperatures during the LGM never occurred.

It would be better to formulate the assumption that the "NEAR-SURFACE permafrost" has reached "equilibrium" or "quasi-equilibrium" with the "climate". Actually this is what the authors are really after with their paper. They compare their computational results with surface observations (Vandenberghe, et al., 2010) and those relate only to "near-surface" manifestations. (At least I assume this to be the case, the Vandenberghe et al. paper has not yet been published and I have not seen it.)

2) The second statement refers to the proposition that we can somehow provide a statistical tool on top of climate models (CM's) to predict past or current permafrost type distributions. Mean annual temperature is indeed a strong indicator for presence/absence of permafrost. Unfortunately, the nine PMIP2-models (as well as others) are known to perform rather poorly in Polar Regions as we can see in Chapter 8 of the Assessment Report 4, WG1 of the IPCC 2007 report, herein Figs. 8.2 (a, b), page 609 (English version). The calculated temperatures for Siberia today are in part off the true value by several degrees (it is worse in Greenland). If the CM have difficulties to simulate the present, how can we be certain that they do better during the LGM? The CM's have difficulties to handle mean annual air temperatures in Polar Regions and, in particular, on ice surfaces – see the deviations between CM's and reality in Antarctica and Greenland. Now, how does this affect the calculations for the LGM (lots of ice surfaces back then)?

The Levavasseur et al paper refers to this aspect on page 2254, where we read: . . .nine CM from PMIP2 cannot simulate a cold enough climate . . . warm bias that cannot be completely corrected by one of the two tested SDMs. . .

The central weakness of this paper is in my opinion the use of the CM's, which are known to "misbehave" in Polar Regions. From the outset there should have been little hope to arrive at a "correct" result.

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The statement (page 2254) "... warm bias that cannot be completely corrected by one of the two tested SDMs..." is very problematic, because it seems to me that it is a rather dubious approach to correct an incorrect calculation by application of "statistical medicine". We do not even know much about the nature of this bias – is it a constant value or otherwise? I have therefore little hope that further refinement of the statistical methods as discussed at the end of the paper (page 2256) will help to improve the situation.

Anyway, the result is that both approaches (GAM-RV-downscaled and MLR downscaled) end up with a rather poor performance with respect to %DP (the percentage of discontinuous permafrost (DP) in agreement with data) for current climate and end up with a dismal performance for LGM with respect to %DP (Tables 2 + 3). This indicates in my view a major flaw in the adopted approach. It is my clear impression that the only result this paper provides is a clear signal that CM's cannot properly reconstruct temperatures in Polar Regions for today and, in particular, for the LGM.

This situation puts any reviewer into a difficult position, because reviewers are not supposed to rewrite a paper. But in this case I think, a publication should only go ahead if it is clearly stated as key result that CM's need to be improved considerably before it can be hoped that such an analysis will be successful.

Additional comments:

Abstract: line 19-21: The sentence ...Nevertheless, this also proves that..." could be deleted. What is stated here has been known for a long time. No new result.

Last three sentences in Abstract: see also my point under 2):

The failure of the SDMs to "improve permafrost distribution" is probably not due to shortcomings of the chosen statistical methods "... which deserve further studies", but due to shortcomings of the CM's. I suggest a more specific formulation to the effect that we deal here with a CM problem and not so much with a statistical "mishap".

Page 2235, Line 6: The paper by Lawrence and Sclater (2005) adds very little to the question of permafrost reaction to climate change. The paper explicitly states in chapter 2. Model, that the applied CCSM3 – GCM includes a 3.43 m-deep soil model. Delisle (2007) has shown that such an approach is not appropriate, since permafrost is usually a lot deeper.

DELISLE, G. (2007): Near-surface permafrost degradationâĂŤhow severe during the 21st century? Geophysical Research Letters (GRL) paper 10.1029/2007GL029323.

Page 2239, line 10 and follows: “the gridded temperature climatology by CRU” . . . ”permafrost is probably not in equilibrium with present climate”. . .

CRU considers a rather short time span (1961-1990). There is (my impression) general agreement of a cooling period in Siberia from 1930 – 1960 (about -2°C) and warming of +2°C in 1960 – 1990 (see e.g. Permafrost Temperature Dynamics Along the East Siberian and an Alaskan Transect (Extended Abstract) VLADIMIR E. ROMANOVSKY, T.E. OSTERKAMP, T.S. SAZONOVA, N.I. SHENDER and V.T. BALOBAEV, Tohoku Geophys. Journ. (Sci. Rep. Tohoku Univ., Ser. 5), Vol. 36, No. 2, pp. 224-229, 2001 or other sources) and subsequent cooling at least until 2003 (Fig. 1c in Comiso & Parkinson, Satellite-observed changes in the Arctic, Physics Today, Aug. 2004). So the near-surface permafrost today is probably in rough equilibrium with the average climate of Siberia in existence since 1930 apart from its reaction to the ± 1°C changes during one 60 year cycle.

Page 2243, lines 17-23: The lengthy explanation to thermal conditions at the base of glaciers should be deleted. The authors rightly observe in the follow-up sentence that their statistical methods do not add anything to solve this problem and cannot be used for above problem in any way.

I had initially difficulties with Tables 2 and 3. They are easier to understand by following the detailed explanations in the text. I would appreciate to add a sentence e.g. as follows: “For detailed explanation see text.”

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In this context it is not helpful to use expressions such as “area of about...” Be specific: page 2244, line 24: the correct numbers are 1.1 x 106 km^2 for model 4 and 0.8 x 106 km^2 for model 7.

Table 3: Under “GAM-RV Downscaled” %DP is explained to represent “the percentage of discontinuous permafrost (DP) in agreement with data”. For model 6 the percentage is 0, the other models are hardly better. The calculated area of DP by model 6 is 4.8 x 106 km^2, the true value (DATA) is 4.5 x 106 km^2. As I understand it the calculated DP is completely somewhere else as the DATA – DP (I could not work it out from the Figs. 7-8.). Hard to believe – could you explain in the text where the calculated DP ends up geographically in relation to the DATA-DP or what these numbers mean in reality?

If we look down to the %DP- line “MLR downscaled”, the situation hardly improves. Apparently about 80-90 % of calculated DP is somewhere else as supposed.

Once again I believe the value of this paper would be enhanced, if the authors would flatly state in their conclusions that this type of analysis should await the availability of vastly improved CM’s.

Interactive comment on The Cryosphere Discuss., 4, 2233, 2010.