Earth Syst. Dynam. Discuss., 3, C442–C448, 2012
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Earth System Dynamics Discussions

Interactive comment on "A stochastic model for the polygonal tundra based on Poisson-Voronoi Diagrams" *by* F. Cresto Aleina et al.

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Received and published: 26 September 2012

We thank the reviewer for the comments on the manuscript. Reviewer comments are in italics, with our replies below.

The manuscript is very well written and explanative, and I'd like to thank and congratulate the authors for that. Besides, the manuscript tackles one crucial issue and current challenge for global climate modellers, which lies in the upscaling of landscape-scale processes especially relevant for high-latitudes CH4 fluxes. Such initiatives are too scarce and deserve both highlight and support. For this specific reason, and as your model seems to be very well suited for such applications, the manuscript could possibly extend the qualitative results to some quantitative comparisons between observed and modelled landscape-scale CH4 flux estimates, as well as between modelled and ob-

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Interactive Discussion

Discussion Paper

served water table depth (part 3.), which could require the use of an observed climatic forcing.. Or maybe this could at least be mentioned as a promising prospect!

We appreciate this comment. The estimated CH4 fluxes at landscape scale, as we report in figure 6, are of about $26 - 28mg/m^2 * day$ in the standard scenario. Torsten et al. (2010) measured an average methane flux of about 20 mg/m2*day over the whole landscape using eddy covariance methods. In our opinion, the agreement is very good, considering the very simple method we applied for methane emissions (equation 15 in the revised paper), and the fact that we did not do any fine-tuning about the environmental controls (precipitation and evapotranspiration).

We compared model results with water table depth data from Boike et al., 2008, and results were matching data well. Of course the limitation is that data were taken from single polygons, and therefore there are no data available on landscape-scale water table dynamics to our knowledge.

Also, the discussion could include more elements on the possibility to carry flux estimates at the scales of global climate models, considering (for instance) the following elements:

what is the proportion of low-centered polygonal tundra among Arctic lowland landscapes, and do they all share similar properties (size distribution of polygons, porosity...) ?

could the model be easily adapted to other typical types of patterned ground (and which data should be acquired to tune the model properly) ?

We thank the reviewer for the comment, and we improved the discussion section.

Low-centered polygonal tundra has not been exhaustively mapped in the Arctic. Any area estimates are based only on indirect indicators, e.g. topography and ice content in the soil. Minke et al. (2007) extimated that this environment covers an area of about 250,000 km^2. Proportion of low-centered polygonal tundra in Arctic lowland

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landscapes varies between 30 % (Lena Delta, Siberia) (Muster et al. 2011) and 70 % (Alaska Coastal Plain) (Brown 1967).

The spatial tessellation technique is quite general, and we believe that it could be usefully applied in other environments. On the other hand, for different environments we would need different models of vertical hydrology and different parameterization of processes to tune the model. In order to generalize this approach, we would definitely need information on the microtopography of the environment we would apply the method. If one looks at peatlands, for instance, a characterization of the surface elevation (namely, the amount of surface covered by lawns, hummocks, and hollows) in respect to the water table is essential. We will include this discussion more clearly in the revised version of the paper.

p 454 (abstract) l 14: "surface properties" is a bit vague, could you be more precise?

We meant soil wetness and microtopography.

p 459 eq 4: what fA(x) is, is not clearly explained, and possibly, the detail of this distribution function is not very useful except if you want to compare the modeled distribution with the observed distribution based on data by S. Muster (see the comment on part 3.1).

$f_A(x)$ is the distribution that PVD areas follow. This distribution is not theoretically derived, but has been found through numerical inverstigations. We use this function in paragraph 3.1 to compare the model output with this finding, in order to test that our model is correctly reproducing the distribution of PVD areas. In particular, we show that also the centers C, despite the random parameter q in Eq. (5) follow such distribution.

p 461-462: The parameterization choice for P could benefit from (i) a small explanatory graph (ii) additional justification on the P=f(t) function choice, although more details are given p. 464 and p468. In particular, the typical range of values for Rp for wet/dry summer conditions should be already detailed here, as well as an explanation for the

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10mm/d value systematically chosen for the first 30 days of the summer season.

Thank you for the useful comment. Both precipitation and evapotranspiration parameterization are chosen to represent the data collected in the field campaings of 1999 and 2003, used by Boike et al. (2008). We tuned the parameterization of the model in order to represent field data. We included this information in the revised paper.

Similarly, the parameterization choices for ET could benefit from more details: (i) what is exactly the 'summer period' ? June-July-August ? or June, 21 to Sept., 21 ? or 90 days from July on, as suggested on page 463-464 ?

For summer period we mean beginning of July - end of September. We included this information in the revised version of the paper.

(ii) the sinus choice for the ET=f(t) function could possibly be related to the seasonal variations of available (SW, SW+LW) energy, as well as the different ETp values chosen. An illustration of the model values with superimposed observed ET values could be highly valuable.

We parameterize ET with this function to reproduce field data. We thank the reviewer for the suggestion, but we think that an illustration of such a function, which we tuned with field data, would not add much information to the paper. On the other hand, we think that focusing on parameterization of precipitation and evapotranspiration would confuse the reader, since it is not our aim to focus on the parameterization of climate forcing, but rather on the answer to the forcing of our system.

We included in the paper on where to find information on the surface energy balance and heat fluxes for this environment Langer et al. (2011b).

(iii) The notation is not very conventional in this sense, the use of "and" instead would help.

Yes, we modified it.

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Regarding the evolution of thaw-depth: could you cite a reference for the mentioned observations?

As for previous comment, we parameterized the evolution tuning the model with data used by Boike et al. (2008).

P467: On the polygon statistics: the aim of the comparison between model outputs and the generalized gamma and 2-parameter gamma distributions is unclear to me, as the authors stated in paragraph 2.1 that such a result (agreement between the area-distribution of PVD such statistical distributions) is delivered by numerical investigations. Is it then just some numerical test to check that the model performs as it should, in which case this result may not be worth mentioning here?

The result is interesting because we compare also the area of polygon centers C=(1-q)Apol with the distribution. The centers are not PVDs because of the random parameter q, but they also follow such distribution.

The idea behind the whole paragraph is that, when the model is tuned with field data (Muster et al., 2012) it reproduces the area-distribution of the observed tundra polygons, whereas similarly tuned models using simpler polygons fail. Such a test is really worth doing and mentioning, however, as it is somehow the rationale for the choice of your particular spatial model. Therefore, and although the line between model description tuning, and model validation, is hard to define, especially in this case, I would suggest to relocate this paragraph into 2.1.

In paragraph 2.1 we explain the concepts behind the Poisson-Voronoi diagrams and we describe their properties (such as the area distribution). When we apply the tessellation technique and we compare the area of the whole polygons and the area of polygon centers, which are not PVDs. We think that the difference between the description of the PVD theory (paragraph 2.1) and the results of the analysis of the statistical properties of the model is enough to justify the separation of the two parts.

p468 line 15 : due to lateral runoff.. what about ET ?

It is true that ET contributes substantially to the water table drop, but in figure 5(a) we show how the trajectory P-ET (without taking the runoff into account) does not display the characteristic drop at the end of the season.

p470 : (as mentioned in the general comment) Though CH4 emissions coefficients for wet, moist and saturated polygons are fixed, your model approach constitutes a valuable first step to hint a model-based quantification of summer CH4 emissions for a low-centered polygons tundra landscape. Therefore the manuscript could benefit from a quantitative comparison with observed summer CH4 emissions (by Sachs et al., 2010 for instance). It seems that the final aim of your model is besides to deliver such kind of quantifications by succeeding in upscaling CH4 emissions where plot-scale approaches fail.

Yes, thank you for the comment, see our answer to the general comment. We included such a qualitative comparison in the revised paper.

Figure 3 : isn't it rather a moist center ?

The reviewer is right. We meant to provide a general picture for the description of polygon centers, which are usually wetter than the drier rims. We corrected the label.

Figure 7 : increase of Wt level and increase of thaw depth

Done.

Technical corrections:

We modified the text in the revised paper.

Conclusion:This manuscript constitutes a valuable contribution to the improvement of the modelling of the Arctic. I find it should be accepted for publication, pending the revisions mentioned above.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 453, 2012.

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