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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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We thank the reviewer, Prof. Andy Baird, for his comments and suggestions. Reviewer's comments are in italics, our answers below.

p. 454, l. 15. Statistically relate what to what?

To the main small-scale processes within the single polygons. We modified it in the text.

p. 454, l. 25. stressed out is the wrong expression to use here. It implies psychological stress. I suggest writing as highlighted by recent studies.

Done.

p. 455, l. 10. Is there a pronounced contrast in water-table levels relative to a datum or in water-table depths below the ground surface? The latter can vary considerably even when the former is constant.

We assume though the surface inside the polygon centers (i.e., where we compute the water table depth) to be flat. Therefore, we take as reference datum the ground surface level.

p. 455, l. 17. This point could be made more clearly. The reason a mean water table does not represent mean CH4 emissions well is because CH4 emissions are non-linearly related to water-table depth.

We thank the reviewer for the comment, we modified the text according to the reviewer's suggestion.

p. 455, l. 28. I would insert a but after the comma on this line. Alternatively, replace the comma with a semi-colon and write model; however, the real world.

Done.

p. 456, l. 5. This percentages given here seem high.

We thank the reviewer for the comment. He is right, and it was a mistake. The polygonal tundra covers about 3-4

p. 456, l. 9. This should read thermally-induced.

Done.

p. 456, l. 17. The comma on this line is not needed.

Done.

p. 456, l. 18. I would start a new sentence after the comma.

Done, thanks.

p. 457, l. 3. I would add a line or two explaining Minkowski densities.

Minkowski densities and density functions are measures for quantifying arbitrary binary patterns. We added the description in the text.

p. 457, l. 17. field should be fields (plural).

Done.

p. 457, l. 23. framework should be a framework.

Done.

p. 458, l. 4. This should read 'landscape's' (i.e. possessive case).

Done.

p. 458, l. 11. I recommend deleting the second "the" and replacing "Diagrams" with 'diagrams'.

Done.

p. 459, l. 8. What is meant by thermodynamic limit here?

In statistical physics, the thermodynamic limit is reached as the number of elements in a system approaches a very large number. The thermodynamic behavior of a system is asymptotically approximated by the results of statistical mechanics, and boundary effects can be neglected.

p. 460, l. 5. a should be an.

Done.

p. 460, l. 8. How is the thaw depth prescribed? Surely, it is affected by the water-table depth and will vary from polygon to polygon. It would be helpful if more detail were given here. Okay, I see this information is given later but that later information should be referenced here.

Done. We referenced the information.

p. 460, l. 10. Is a water table assigned to each polygon? It would be useful to know what field data were available for the tuning of the initial condition of the model.

Yes, the water table is indeed assigned for each polygon. We tuned the initial conditions with data from field campaigns in 1999 and in 2003, which have been used in Boike et al. (2008).

p. 460, equation (6). Why is it assumed that the polygons are circular? Figure 1 suggests that many are square or rectangular.

Polygons are not assumed to be circular, polygon centers are only assumed to be circular for simplicity in the computation of angle alpha.

p. 461, l. 1-3. Why use three classes? Why not a continuous function of water-table depth?

The water table is a continuous function in our model. We distinguish three emissive classes, because field studies on which we base comparisons with our results also distinguish different classes of soil types (see Sachs et al, 2010). We are not aware of more continuous measurements for this study site.

p. 461, l. 14. This equation and its use do not make sense to me. The authors note that ΔS_t may be defined as the amount of water stored in the unsaturated terrain that does not contribute to water table variations, but what does that mean? They seem to be referring to unsaturated zone storage, but it is not clear how they account for water movement to and from the unsaturated zone. Also, the dimensions of ΔS_t are not clear. For the equation to be dimensionally homogenous, the dimensions should be L T-1, but the authors seem to imply here that S_t has dimensions of L. Even though St has been included as a term, the equation doesn't seem to work. Water-table fall is most sensibly described using the concept of specific yield or, more simply, drainable porosity. In such a case a suitable equation would be:

$$\frac{dW_t}{dt} = \frac{P - E - R}{s} \tag{1}$$

where s is the drainable porosity (dimensionless) defined as the volume of water that has to be removed from a volume of soil to cause a unit fall in the water table. This equation could also describe water-table rise, if it were assumed that water storage above the water table is not affected by P and E. If that assumption did not hold, a submodel would be needed that described such storage and exchanges to and from that storage.

We improved the water balance equation.

We substituted the precipitation term with an effective precipitation term, which takes into account also the influence of water income from the polygon rims. This term, which we neglected before, has been indicated by recent measurement campaigns as very important in the water balance. We inserted the storage term in the effective precipitation term too. The storage term is now meant as the amount of incoming water, which stays in the rims at each time step, not flowing from the rims to the polygon centers. The effective precipitation term is the input term in equation 7, which now looks as the one the reviewer suggested. We increased the modelled runoff to 3 mm/day, in order to mantain the balance. Also, we checked the dimensions of the storage term, which were correctly LT^{-1}, as the reviewer suggested. We made the description of the term clearer in the text. We repeated the simulations according to the new water balance, and inserted the new results, which are generally in accordance with the previous ones. We also discussed the drainable porosity parameter, see our answer to comment on page 462.

It is not clear to me how equation (7) is applied to different polygons. Is it applied to different classes of polygon, to individual polygons, or to none? I cannot see how the equation is used with the statistical model to simulate the different classes of polygon (wet, saturated, and moist). These different types are declared as initial conditions in

the model, but how does the water table level in the polygons change thereafter?

Water balance (Equation (7) in the text) is applied indeed to each polygon, in order to compute at each time step the area covered by different soil types (wet, saturated, and moist), as a function of the water table position in respect to the ground surface level, which dynamically changes and responds to climatic forcing. Therefore, such classes are not prescribed, but they change dynamically according to water table variations.

p. 462, l. 12. The authors seem to be discussing the concept of drainable porosity here, and appear to assume that the drainable porosity in part (at least) of the soil profile is 0.5. On what do they base this assumption?

We changed the value to 0.7, basing this hypothesis on recent measurements performed by Langer et al. (2011).

p. 463, equation (11). Is it assumed that the soil interior of the polygons is unfrozen at all depths during the summer?

Yes it is. We assume permafrost to be present (and therefore to thaw) only in the polygon rims.

p. 463, equation (13). I don't understand this equation; what is its physical basis? Surely ΔS_t should be a function of St and not directly of time? Am I missing something here because the equation seems to be suggesting that the law of mass balance is broken. What happens to the P that is not taken up by the unsaturated store? I suspect the problem here is not with the model itself, but with the way the authors describe it in this paper using equations (7) and (13). However, there could be some deficiencies with the model. Until the model is explained more clearly, it is not possible for readers to judge its usefulness.

We thank the reviewer for this comment. We improved our description of the water storage, which takes place on the unsaturated soil in the rims. See also our reply to comment on equation 7.

p. 464, equation (14). Why are CH4 fluxes greater from saturated polygons, where the water table may be as much as 10 cm below the ground surface, than from "wet" polygons where there is at least 10 cm of water ponded at the surface?

We followed previous studies on the subject (Couwenberg, J. Fritz, C. 2012, Zona et al., 2011, Sachs et al., 2010, Strack et al., 2004). Such studies show that the CH4 emissions decrease with increasing the water table level after a certain threshold, which for this tundra ecosystem we fixed at 10 cm.

p. 465, l. 5. "trough" should read 'through'.

Done.

p. 465, l. 16. "no" should be deleted.

Done.

p. 467, l. 5. Accepted in what sense?

The K-S test accepts the null hypothesis, that the modelled distribution and the empirical one are the same.

p. 467, l. 14. "momenta" should read 'moments'.

Done.

p. 469, l. 15. This conclusion is interesting, but I have concerns that the model may not be that physically realistic in terms of how it represents water-table fluctuations. Therefore, while this finding may be broadly reasonable in a qualitative sense, I am not convinced that the real system has the same approximate sensitivity as suggested by the model.

We improved the description of the water table, and since in the new simulations the system still displays the same behavior, we are more confident in our conclusions.

p. 471. l. 5. This is an interesting finding. It would be interesting too to use a high-

resolution DTM and to see what degree of connectivity there is in a real landscape. I am not suggesting the authors do that here, but I recommend they consider it for future work.

We are not aware of such high-resolution databases for this environment. We nevertheless believe that interconnectivity could be further studied through applications of flow-network theories. We inserted this as possible development in the discussion in the revised version of the paper.

p. 472, l. 11. I agree with the authors here. However, it is important that their representation of physical processes such as water-table rise and fall are reasonable and that there are not problems with mass conservation, for example. They need to explain the hydrological part of the model more clearly so that readers can judge the soundness of this part of their model, and, therefore, of the model simulations as a whole.

We changed the hydrology parameterization, therefore we are more confident about our statement. The new experiments confirm our previous findings.

p. 472, l. 20. Could the approach really be applied to other environments? It would be helpful here to know of its limitations. A basic premise of the model is that the generating points are independent and random in space (i.e., that they come from a homogenous Poisson point process). Would other landscapes have such a pattern, or would other point process models be more suitable? Indeed, what happens in polygonal landscapes with a gradient, and how does water seeping through the wedge network affect the hydrological balance and relationship between polygons? It would be useful if the authors entered into a more meaningful discussion here about the wider implications of their simple model.

We thank the reviewer for this comment: we improved the discussion in the text, and we also think that this is an important point in the paper, which could be addressed in more details in the following studies.

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