Review of Cresto-Aleina *et al*. *A stochastic model for the polygonal tundra based on Poisson-Voronoi Diagrams*, submitted to *Earth System Dynamics Discussions*.

Overview.
The authors present a partly-stochastic model of water-table dynamics in, and methane (CH_4) emissions from, polygonal tundra. Their statistical approach allows one to produce a simple model of CH_4 emissions from a complex landscape without the burden of a detailed mechanistic model. However, despite discussing the problems of upscaling from the plot to the landscape and land-surface scheme (LSS) tile scales, they don't really discuss how their approach could be incorporated into a LSS model.

Their study is novel in that it shows how polygons may be represented using a simple homogenous Poisson point process, with the points representing the centres of polygons defined using a Voronoi model. Water-table dynamics in each (but see my detailed comments) statistically-generated polygon are modelled using a simple storage model. CH_4 emissions are estimated by assuming that the wetness of a polygon is the main controlling factor. The modelling approach is interesting, and I like the idea of keeping the model as simple as possible. However, I have some concerns about how the hydrological modelling was done, and these concerns prevent me from recommending publication. For example, it is not clear that the hydrological model conserves mass. If the authors can explain more clearly how their hydrological model works and can show that it is physically sound, then I would be happy to recommend publication.

There are also some more minor problems with the paper that need to be addressed before it can be published. These are discussed in my detailed comments below, where I also give more information on my more substantive concerns (these are in **bold**).

I operate a policy of open reviewing and ask that my identity be revealed to the authors. As part of this policy, I sincerely hope the authors take my comments in the manner intended: that of scientific openness.

Detailed comments.
p. 454, l. 15. Statistically relate what to what?
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'.
p. 455, l. 10. Is there a pronounced contrast in water-table levels relative to a datum or in water-table <u>depths</u> below the ground surface? The latter can vary considerably even when the former is constant.
p. 455, l. 17. This point could be made more clearly. The reason a mean water table does not represent mean CH_4 emissions well is because CH_4 emissions are non-linearly related to water-table depth.
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'.
p. 456, l. 5. This percentages given here seem high.
p. 456, l. 9. This should read 'thermally-induced'.
p. 456, l. 17. The comma on this line is not needed.
p. 456, l. 18. I would start a new sentence after the comma.
p. 457, l. 3. I would add a line or two explaining Minkowski densities.
p. 457, l. 17. "field" should be 'fields' (plural).
p. 457, l. 23. "framework" should be 'a framework'.
p. 458, l. 4. This should read 'landscape's' (i.e. possessive case).
p. 458, l. 11. I recommend deleting the second "the" and replacing "Diagrams" with 'diagrams'.

p. 459, l. 1. I would insert an 'a' before "Voronoi" and replace (here and elsewhere) "Diagram" with 'diagram'.
p. 459, l. 8. What is meant by "thermodynamic limit" here?
p. 460, l. 5. "a" should be 'an'.
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.
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.
p. 460, equation (6). Why is it assumed that the polygons are circular? Figure 1 suggests that many are square or rectangular.
p. 461, l. 1-3. Why use three classes? Why not a continuous function of water-table depth?
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 S_t has been included as a term, the equation doesn't seem to work. Water-table <u>fall</u> is most sensibly described using the concept of specific yield or, more simply, drainable porosity. In such a case a suitable equation would be:

$$\frac{dW_t}{dt} = \frac{P - E - R}{s}, \qquad P < (E + R)$$

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.

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?

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?
p. 463, equation (11). Is it assumed that the soil interior of the polygons is unfrozen at all depths during the summer?
p. 463, equation (13). I don't understand this equation; what is its physical basis? Surely ΔS_t should be a function of S_t 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.
p. 464, equation (14). Why are CH_4 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?

p. 465, l. 5. "trough" should read 'through'.
p. 465, l. 16. "no" should be deleted.
p. 467, l. 5. Accepted in what sense?
p. 467, l. 14. "momenta" should read 'moments'.
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**.
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.
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.
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.**

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