Review comments for the ESD manuscript "A stochastic model for the polygonal tundra based on Poisson-Voronoi Diagrams" by Aleina et al.

General comments:

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 observed 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!

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) ?

Specific comments:

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

- p454 l 24 : in most GCMs..

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

- 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 R_p for wet/dry summer conditions should be already detailed here, as well as an explanation for the 10mm/d value systematically chosen for the first 30 days of the summer season.

  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 ?

(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 ET_p values chosen. An illustration of the model values with superimposed observed ET values could be highly valuable.

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

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

- 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 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.

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

  - 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.

Figure 3 : isn't it rather a moist center ?
Figure 7 : increase of Wt level and increase of thaw depth

Technical corrections:

- p458 line 10 : represent represent
- p459 line 16&17 ; p467 l 4 : 2-parameter
- p460 line 5 : an ice-wedge polygon
- P464 line 6 : "when there is still little thaw depth" : you surely mean : when the thaw depth is still limited.
- P464 line 14 : of of.

- P464 : for clarity, possibly create a new paragraph : 2.4 : methane emissions.
- P465 :
    - The reference on the percolation theory should be cited at the first mention of this theory.
    - the near Lena river -> to the Lena river nearby.
- P467 line 21 : the the
- p468 line 5 : asdescribed
- p468 line 12-13 : the model captures this process as well as the water table variation magnitude.
- p468 line 14 : Model
- p468 line 17 : landscapeif
- p468 line 24 : with respect to
- p469 line 3 : '.' is missing; eq (14) could be cited.
- p469 line 11 : in the ensemble…
- p469 line 16 : to -> of
- p471, and other occurrences: Poisson-Voronoi vs Poisson Voronoi : please choose !

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.