Review for **"Inter-annual, seasonal and diurnal features of the cloud liquid water path over the land surface and various water bodies in Northern Europe as obtained from the satellite observations by the SEVIRI instrument in 2011-2017"** by Kostsov et al., submitted to Atmospheric Chemistry and Physics

Synthesis:

This paper presents satellite observations of cloud liquid water path over coastal areas of northern Europe. The authors describe distinct features of cloud cover difference over land and water during different seasons.

General comments:

- The motivation of the study and the review of previous work is rather poor. An introduction into typical land-sea contrasts of clouds, wind and temperature and related studies would be necessary in the introduction.
- To me, the word *"gradient"* does not describe what you are analyzing. A gradient is the change of a quantity over a distance. In this case it would be *LWP* m^{-1} > hence the unit would have to be *kg* m^{-2} m^{-1} = *kg* m^{-3}. This would not be very useful, therefore I suggest to change the wording to *"LWP difference".* Also, your direction of the gradient is wrong. If you call it "*Land-Sea gradient*" your values would have to be negative.
- The limitation to 7 years of data is probably due to large amount of data. However, SEVIRI data are available for the time back to 2004/05. Why didn't you include some years before 2011?
- Trends over 7 years have no statistical significance (e.g. Fig. 9). Excluding just the last year (2017) would already show completely different trends. By using a larger dataset (e.g. 2005-2020) you could test your hypothesis. I can imagine that single outliers can be caused by a dry/wet summer (low/high soil moisture), more or less sea-ice/snow cover, or windy conditions (sea/lake temperatures are less stratified). Therefore, you have to show more proof for your "most important finding" (p.16, l. 474ff and p. 17, l. 515ff) . In the current version, to me there is no proof that another selection of years would not produce totally different trends.
- For the comparison with reanalysis data, I wonder whether the model grid boxes that you chose are really fully placed over sea or land, respectively? In addition, the effective resolution of processes in an atmospheric model is always coarser than the nominal grid spacing.
- I would consider analysing more surface variables for specific days, such as air temperature at some coastal stations, sea surface temperature, diurnal wind patterns (sea breeze), etc. With that, your hypotheses, such as the "August anomaly" could be strongly improved.
- The number of figures should be reduced, or more figures should be combined to one large figure (e.g. Figs. 4-6).

- Please provide scales to your maps (esp. Fig. 17)

Specific comments:

p.3, l. 78: In addition, during winter/spring, (dark) forest areas can absorb considerably more solar radiation than surrounding snow-covered ground or ice-covered water surfaces. This can also lead to updrafts and eventually cloud formation.

p.4, l. 121-122: What do you mean by “simultaneously and not simultaneously”? Do you want to say "cases where both land and water or any of them are clear sky"?

p.10, l. 281 ff: Which time zone did you use for “local time”? UTC+2? If so, please mention it here!

p. 13, l. 380 ff: I am missing some information about the setup of the ICON-LEM model: What is the resolution? Which initial profiles did you use? What is happening at the domain boundaries?

Table 1: What's the percentage of days which were used for the analysis? Is there a significant inter-annual difference?

Summary

To summarize, I cannot recommend publication of this manuscript at this stage. The manuscript needs major revisions by carefully considering all the above-mentioned points.