August 10, 2021
Subject: Review of manuscript ACP-2021-325

Dear Authors

The authors employed Large Eddy Simulations (LES) to study the impacts of heterogeneous heat flux surface on flow turbulent characteristics in this study. From my point of view, the authors were able to handle this task satisfactorily and this paper is worth publishing on ACP. My decision is "Accept with minor revision".

Furthermore, I have highlighted some points of the article to be reviewed as well as I have made some comments to improve the manuscript.

General comments

(a) The figures need resolution improvement.

(b) The authors used many acronyms to refer to the different simulations. In my opinion, this can confuse the reader in a first moment and it makes the reading slower. On the other hand, I understand the necessity to use it. My suggestion is to explain the logic behind the character choice and try to simplify the acronyms, i.e. shorter acronyms. Furthermore, avoid using non-alphanumeric symbols like underscore.

Specific comments

(1) Page 7, lines 135-140 – The authors employed ERA-Interim data, with 1x1 degree resolution, to describe the flow synoptic features. Why did they chose this specific data set instead of another one with better time and spatial resolution as ERA5? A short explanation about this choice would be appreciated.

(2) Page 7, line 151 – The authors used 200-m of spatial resolution on their simulations. Is it an appropriate resolution for LES regarding the turbulence gray zone? In some papers on literature, this order of resolution size is called Very Large Eddy Simulations (VLES). Maybe the authors could clarify it better on the text. Furthermore, how about time resolution (time step) for these simulations? Please include a brief comment about it on the text.

(3) Page 7, line 152 – The authors did a vertical grid spacing description at that point. I suggest a more precise description including:

  (i) how many vertical levels were used on simulations setup;

  (ii) how they were stretched;

(4) Page 8, lines 158-161 – The authors indicated the initial conditions for the simulations. Could they mention figure 2 here? A simple indication as: "These conditions will be detailed on figure 2" would be enough.

(5) Page 11, figure 2: The initial conditions obtained from upper air sounding present a non-smooth shape with a pronounced vertical variation (mainly for the wind components), which is expected for high-resolution measurement. My question is: were they used exactly as it was showed on figure 2 as initial condition or were they smoothed to

accomplish it? If so, please, show the initial condition smoothed profile on figure 2 as well.

(6) Page 14, figure 3 – About this figure, I have the following concern/suggestions:

(i) It is not clear to me why the authors mixed different vertical levels to compare temperature and other variables at different times. Could you explain it better? For me, it makes more sense to compare same levels at different times.

(ii) The lines for geopotential height are not clear on these plots as well as its labels (values). Could you improve that?

(iii) Each wind vector seems to be plotted at 0.25 degree. Is it an interpolation for the ERA Interim 1x1 degree resolution data? If so, please remark it. Again, if the resolution for the synoptic wind field is an important feature, it seems to me that another reanalysis dataset would be more interesting for this work.

(7) Page 21, figure 7 – About this figure I have the following concern/suggestions:

(i) What day time are these profiles related to? It was not clear on the text.

(ii) I suggest using the designation "Buoyancy production/destruction" instead "Buoyancy flux" to refer to the turbulent kinetic energy (TKE) budget equation term to avoid any misinterpretation. Furthermore, "Buoyancy flux" is not precise to describe it on a physical sense.

(iii) The buoyancy production/destruction profiles showed a similar behavior for wind/no wind simulations in a homogeneous surface. However, for heterogeneous surface simulations, it is clear that resolved heat flux reaches a lower maximum and decreases differently from pure-convection (no wind) simulation. Could you briefly explain it on the text?

(iv) I suggest a new figure, like figure 6, to describe the effect of buoyancy TKE budget term. It could be included on the main paper or on the supplementary documentation. It would clarify the interaction of lake patches with atmospheric flow and how it impacts the TKE balance.

(v) Is the wind shear the source of SGS shear production peak at $z/z_i = 0.6$ or is there an unusual feature on the momentum flux profile? Again, a plot with the momentum flux profile (resolved and SGS) could be presented on the supplementary documentation to clarify it.

(vi) The wind profiles presented an interesting feature on heterogeneous surface simulations. In the homogeneous case, the wind profile seems to be log-linear close to surface and showed a clear mixed layer above it. For the lake simulations, the wind profiles exhibit a feature similar to a stable boundary layer, with a maximum local wind. It is an interesting feature that could be better discussed on the text. I suggest plotting the potential temperature profile associated to these wind profiles to better understand the PBL vertical structure at this time. One could say that an internal boundary layer process would be occurring here. Furthermore, I suggest plotting a log-linear law and the geostrofic wind components with wind profiles to better visualize and discuss these wind profiles.

(vii) Regarding the possible internal boundary layer formation, an extra plot for potential temperature, similar to figure 6, could be made.

(8) Page 22, line 408 – I wonder if the weaker updrafts could explain the buoyancy TKE budget term features highlighted previously. What do the authors think about that? If these two characteristics are related, please detail it on the text.

(9) Page 23, figure 8 – I think it would be interesting to add an extra plot here with the homogeneous cases. It helps to evaluate the lake patch impact on the local circulation.

(10) Page 432, figure 10 – What time is it on the simulation? Is it on the same time of wind profiles from figure 7? I am asking it because the negative heat flux on the wind-simulations, above the lake patch, could be decreasing the turbulent viscosity and increasing the wind speed consequently. What do the authors think about that? It is important to note that the minimum flux value (negative) is in a magnitude so strong as it is close the surface. Furthermore, it happens around the same height of local maximum wind. I suggest plotting the potential temperature associated to these heat fluxes to better understand it.

(11) Page 26, figure 11 – This figure shows a clear transition between the land-lake PBL. I would like to see the wind speed of homogeneous simulation to compare it with the heterogeneous ones, as well as a comparison for potential temperature and heat flux. It could be interesting to understand a possible internal boundary layer formation.