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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Upscaling surface energy fluxes over the North Slope of Alaska using airborne eddy-covariance measurements and environmental response functions" *by* Andrei Serafimovich et al.

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General report on Edited version

This is an excellent combination of aircraft flux measurements, machine learning algorithms extrapolated to surface response functions. It also links to large scale modeling to allow contextual interpretation with respect to hydrological and energy budget response focused on an important climate sensitive region. It provides a

significant advance in the area of airborne flux measurements and relevance to validation of surface response function dependent models. This is an important paper as this very thorough approach to airborne eddy covariance fluxes has really been missing from the scientific literature over the past 1-2 decades in general. The regional model comparison is a nice addition to capture the mesoscale variability and scales although the study is limited by the surface data availability. I do like the terminology used for model-aircraft comparison.

We would like to sincerely thank the referee #2 for the evaluation and the constructive comments on the manuscript. Our responses to the comments and explanations how we revised the manuscript are documented below. We provide a supplement pdf-file, where we marked the respective changes (please see the link at the end).

Updates

Relevant discussion on the design and implementation of the aircraft campaign is included and is sufficient for replication and addressing of issues and potential artifacts in such approaches.

The methodologies are very well described and relevant to the technique applied. These are appropriate to the conclusions arrived at with some limitations however in completing the full energy budget. These could have been discussed further with respect to the uncertainties but generally I don't think this could be improved on.

As you already mention, this paper is our first attempt to apply the methods like airborne eddy-covariance (EC) measurements, mesoscale modeling and machine learning for the projection of energy fluxes. An inclusion of additional data for the entire North Slope with high resolution in space and time requires a lot of efforts in computing time and handling of the big data. However, lessons learned allow us to do this for future studies and we are going to include more parameters from the model like other radiation components, ground fluxes, residual of energy budget.

The relevant transform scales based on the flight track described appear consistent with the approach and is explained well and are also consistent with results previously published in the literature (although these were limited in terms of surface site comparisons). The relevant edge effects associated with the wavelet analyses are always an issue but I think these are within the uncertainties when scaling to the regional observations and looks quite reasonable. Whilst data quality control is critical for such wavelet analyses and could be quantified further I think we can assume this is good based on the results. There are other transform approaches that could have been compared but likely these would not have changed the results.

The transform scales were chosen based on flight lengths as well as on spectral gap analysis (please see our response to reviewer #1, asset 2). Sayres et al. (2017) and Dobosy et al. (2017) recently published comprehensive comparisons of transforms for estimation of turbulent fluxes using airborne measurements. The other transforms may improve the quality of flux measurements. But we were mostly focused on the flux projection on a regional scale and guess, that the most significant improvement of the result uncertainty will be achieved by addition of new flights covering different areas of the North Slope and different meteorological situations.

Page 8: The addition of the distribution “rug” plot, Figure 5, is very useful.

We found “rug” plots also very useful for the interpretation of response functions.

Minor Questions and Formatting Issues

Item 1. Some brief comment on the appropriate optimization of relevant straight and level sampling altitudes for the flux measurements (discussed page 5 etc) with respect to heterogeneity scales within the flight track would be helpful but not essential here?

We added a paragraph about the heterogeneity of the Alaskan North Slope and appropriate pilot action (see page 5).

Item 2. Figure 4. Some of the arrows in the boosted regression tree figure overlap/obscure the text in the various nodes, e.g. $a>0.5$, $S\downarrow>380$ W/m^2, $r>7$ k kg^{-1}

Corrected.

Item 3. Figure 7. It appears obvious that there are two clusters within the sensible and latent heat flux (predicted versus aircraft measured) domain with significantly different slopes with under-predictions at high values in each case. Can the authors comment on this? Is there a potential bias here?

The under- and overestimation is also mentioned by the referee #1 (please see “Questions and Issues, #5”). The clouds of overfitting appear due to an insufficiency of measurements with high and low energy fluxes for machine learning in this range. Most of the data are located in the black cloud. For the sensible heat flux only 10% of the data are less than -5 and more than 80 W/m^2 and located outside of the black cloud. For the latent heat flux only 6% are less than 0 and more than 110 W/m^2. We added this remark to the manuscript (see page 9-10).

Item 4. Figure 8. Legend: Has the standard error used in this figure been defined?

We do not refer in the legend to any quality parameter of flux measurements. Figure 8 shows the median maps and the “standard error” is a statistical parameter of this average equal to the median absolute deviation divided by the square root from the size of the sample. We replaced the words “standard error” by “standard error of the median value” to avoid misunderstanding.

Item 5. As mentioned, the impact of enhanced convective conditions suggests potential under-sampling bias of all relevant scales in these conditions. It would be useful to mention the range therefore where such comparisons may break down, but this may require more detailed spectral analysis for another discussion. However I think this caveat/statement addresses the issue adequately for the work presented.

Please see our response to Item 3.

Item 6. Figure 7 is a brave plot (and we need more of them in the literature before relying overly on tower data). I think the discussion and literature references regarding the discrepancies with WRF are adequate but do highlight that there is still a great deal of work to do here.

Please also see our response to Item 3. We agree and see a lot of opportunities to improve our knowledge about surface turbulent exchange combining process-based mesoscale models, projection of regional airborne measurements and small scale EC

tower data.

Final comment: The authors are to be commended for delivering an excellent set of results.

Referencies:

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Sayres, D., Dobosy, R., Healy, C., Dumas, E., Kochendorfer, J., Munster, J., Wilkerson, J., Baker, B., and Anderson, J.: Arctic regional methane fluxes by ecotope as derived using eddy covariance from a low-flying aircraft, Atmos. Chem. Phys., 17, 8619–8633, https://doi.org/10.5194/acp-17-8619-2017, 2017.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-1166/acp-2017-1166-AC2-supplement.pdf

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