The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-262-RC2, 2019

Interactive comment on "Estimation of turbulent heat flux over leads using satellite thermal images" *by* Meng Qu et al.

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Received and published: 4 March 2019

A review on "Estimation of turbulent heat flux over leads using satellite thermal images"

The focus of the paper is the estimation of turbulent sensible and latent heat fluxes over leads using high-resolution satellite thermal images. The heat transfer over leads play important role in the heat budget of the atmospheric and oceanic boundary layers and affects many processes in the Arctic climate system. However, there is a large uncertainty in the estimates of turbulent heat flux over leads due several reasons: i) the insufficient resolution of satellite images used in models, ii) sparseness of in situ observations and iii) uncertainties in parametrizations of turbulent heat transfer over inhomogeneous sea ice surface. The paper provides new estimates of such uncertainties using satellite images of various resolution and shows the necessity to use high-resolution images and also more adequate parametrizations. To some extent, the paper follows the line of the Marcq and Weiss (2012) paper, but uses realistic surface and air temperatures, as well as wind speed for their case study and also using different satellite data. Therefore, the study adds to the current knowledge and provides revised estimates of the heat flux calculation uncertainties and thus is relevant and valuable. However, the quality of the paper is low and has to be strongly improved. It concerns the choice and description of methods, the analysis of results and language. The paper cannot be accepted in its current state. I suggest major revision with resubmission.

Major comments

1. The two methods are used for the turbulent heat flux estimates: the traditional bulk formulae and the fetch-dependent model proposed by Andreas and Cash (1999). The bulk formulae and their application have to be described in more detail. First of all, it is potential temperature that has to be used in the formula for the sensible heat flux. Second, the heat transfer coefficients depend on height, surface roughness lengths for momentum and heat (z0m and z0t) and stability. Which height, which values for the roughness lengths and, finally, which universal stability functions are used? The authors say that they use the air temperature at 2m height, but wind speed at 10m height from the reanalysis data. Since these heights differ from each other, the bulk formulae cannot be used in their classical form. The authors need to describe in detail how they solve the bulk equations. Do they use z/L or the bulk Richardson number as a stratification parameter in the stability functions?

2. Concerning the fetch-dependent model. In lines 5-10 at page 5, the authors claim that the heat transfer over large leads is less efficient because the temperature (and humidity) difference between the lead surface and air is decreasing with fetch. This mechanism is present in the Renfrew and King (2000) model for heat fluxes over polynya (Renfrew, I.A. & King, J.C. Boundary-Layer Meteorology (2000) 94: 335. https://doi.org/10.1023/A:1002492412097) and in the model of Chechin and Lüpkes for cold-air outbreaks over the marginal sea-ice zone (Chechin, D. and Lüpkes, C. (2017), Boundary-Layer Meteorology, 162:91-116 , pp. 1-26 . doi: 10.1007/s10546-016-0193-

2). The authors should refer to these papers. However, in the basis of the Andreas and Cash model there is a different physical mechanism of how fetch affects turbulent heat transfer. Andreas and Cash suggest that the thicker the thermal boundary layer is, the closer the conditions are to the free-convective limit. They claim that in the free-convective limit the heat transfer is less efficient than in the forced convection. I suggest, that the authors review the existing physical interpretations of the effect of fetch, e.g. by Andreas and Cash, by Alam and Curry (1997), which are different. Also (!), in the Andreas and Murphy (1986) paper, different physics is described (e.g., the effect of a more rough sea ice, for example, and a different interpretation of the free-convection contribution). Also, refer to the Esau 2007 paper (Amplification of turbulent exchange over wide Arctic leads: LargeâĂŘeddy simulation study, J. Geophys. Res., 112, D08109, doi:10.1029/2006JD007225.)

3. As already mentioned, there is another model which takes into account the dependency of heat flux on fetch over leads, namely, the Alam and Curry (1997) model. This model has different physics and more processes are taken into account. It is not clear why the authors prefer the Andreas and Cash model and do not consider the Alam and Curry model. This has to be explained.

4. One of the results of the study is that the fetch-dependent model produces larger fluxes than the bulk formulae. However, the transfer coefficients in the bulk formulae depend strongly on the roughness length for momentum and heat (z0m and z0t) and therefore, the obtained result is only valid for specific values of z0m and z0t, which are not given in the paper(!). Using other values for z0m and z0t can produce completely different results. The Andreas and Cash model, as it is described in the paper, does not show an explicit dependency on the roughness length. However, implicitly, roughness is present in their model and the authors need to describe how the roughness length is present in the model of Andreas and Cash. What are the values for the roughness length in the Andreas and Cash model and how do they compare with the ones used in the bulk formulae? Note, that the Andreas and Cash model is a refor-

mulation of the earlier Andreas and Murphy model. The latter is formulated in such a way that it is compatible with bulk formulae. Namely, the are suggesting to use a fetch-dependent "lead-averaged" neutral heat transfer coefficient. In other words, the Andreas and Murphy formulation would allow a more reasonable comparison with the standard bulk approach.

5. Describe better the case study. Which date is it, what are the synoptic conditions over the study area? Was it a clear-sky case or clouds were present? Does it represent typical conditions in the Arctic? The presented surface and air temperature distributions suggest that this is either autumn or late spring. But one would expect that the effect of leads is strongest in winter.

Minor comments

–Page 1, lines 26-27, rephrase "The rate of turbulent heat transferred", simply "Turbulent heat flux"

–Page 1, line 36. "More intensive network" needs clarification. Also, "stronger influence of leads" - influence on what?

– Page 2, line 14 - "heat flux transfer rate" - the efficiency of heat transfer?

– Page 2 line 16 – remove "More often than not"

– Page 2, lines 20-25, explain better what is meant by "Fetch limited models" and how they are using the fetch-dependence of the internal boundary layer height. Otherwise, the logic is disrupted.

– At least in the introduction the authors should cite the study by Tetzlaff et al. (2015) where the most recent observations of heat fluxes and the internal boundary layer height over leads are presented: Tetzlaff, A. , Lüpkes, C. and Hartmann, J. (2015), AircraftâĂŘbased observations of atmospheric boundaryâĂŘlayer modification over Arctic leads. Q.J.R. Meteorol. Soc., 141: 2839-2856. doi:10.1002/qj.2568

– Page 3, line 10. The actual grid of the ERA-Interim reanalysis has horizontal spacing 0.75o and not 0.125o. The original ERA Interim data is interpolated on the 0.125o grid which does not increase the resolution.

– Page 3, line 39. albedo anomaly

– Page 4, Lines 8-9 rephrase “varying air condition”

– Page 4, Line 21. “limited used of lead width” - what does it mean??

– Page 4, Line 29 “rate of turbulent heat change” - what does it mean?? Rephrase!

– Page 4, Eq. 4 – use CH instead of Cs for the heat transfer coefficient

– Page 4, Line 36 Ce is the bulk transfer coefficient for water vapor

– Page 4, Line 37 “at the surface”, this is wrong. These values are at heights z0t and z0q, which are the roughness lengths for heat and moisture fluxes.

– Page 4, Do the authors use the saturation specific humidity over ice or over water? Do they distinguish between the open water and thin ice in this respect?

– Page 5, Line 11. This is shown in figure 4, not in figure 3.

– Page 5, Line 35, Not “in spite”, but “apart from a dependency on the width of a lead”

– Page 5, line 36. Which simulation? What was used for this simulation - Bulk formulae, or the Andreas and Cash model? For the bulk model, such result is obvious and does not need to be shown. Andreas and Cash write that their model is, on the contrary, not very sensitive to wind speed.

– Page 6, Section name “Results” and not “Result”.

– Page 6, line 35. How is the length of leads calculated?

– Page 6, line 35. The Authors say that the MODIS resolution is 1000m. However, they introduce a class of small (which they call narrow in other places) leads with width less or equal 1km. Somehow, they found 13% of such leads in the MODIS image. They need to comment if 1km wide leads are resolved with 1km resolution.

– Page 7, line 4. The direction of fluxes from the ocean to the atmosphere is not consistent with the Eq. (4) and (5). The authors should modify the Equations to get the right sign and direction of fluxes.

– Page 7 line 12. In which range of width is the fetch-limited model valid? Why was it applied to Landsat only? There wide leads in Landsat as well. Write that Landsat better resolves leads, so that's why it was applied to it.

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