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The Cryosphere
Discussions

Interactive comment on "The impact of heterogeneous surface temperatures on the 2-m air temperature over the Arctic Ocean in spring" *by* A. Tetzlaff et al.

Anonymous Referee #3

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Review of "The impact of heterogeneous surface temperatures on the 2-m air temperatures over the Arctic Ocean in spring" by Tetzlaff et al.

This work describes 3 methods coupled with different Arctic surface (ice concentration and surface temperature) and wind analysis data sets to explain the influence of the ice surface heterogeneity on the 2-m air temperature variability at 3 locations in the High Arctic during late winter and early spring conditions under mostly clear skies. It addresses the link between the surface station temperature and the surrounding sea ice conditions which is becoming an important issue in the light of the decreasing amounts of sea ice. It is found that most of the temperature variance can be explained by the

ice surface conditions and radii of influences were also determined. The paper is well written and clearly organized. However, I felt a bit disappointed that stronger conclusions were not found considering the scope of the work and amount of measurements and analyses that were used. Aside from some quantitative details the findings in the paper could have been deduced from a simple back-of-the-envelope calculation. As a technical paper describing the details and results of different procedures the paper is good, but as a scientific paper providing an improved insight the paper is somewhat lacking. I recommend that the paper not be published until the authors have addressed the following major points.

1. My main scientific concern deals with assumptions on the nature of the BL. In late winter and early spring, especially under clear skies, the High Arctic BL is often a strong surface-based temperature inversion with very high static stability. As a result the wind and temperature profiles show large gradients very close to the ground which are not captured by the reanalysis data sets. This calls into question not only the wind speed and wind directions used in the back trajectories but also the validity of the assumed boundary layer depths which become the mixing heights as I understand their approach. I think it is important to anchor the reanalysis data with the station radiosonde measurements of the boundary layer profile. Was this done? If not then I suggest that this be used to reduce the errors introduced from the reanalysis data. Under strong stability conditions there is a decoupling between the surface air and the air just above it. It might be better to compare your model predicted temperatures with the temperature above 2-m at the station if the boundary layer is very stable. This issue needs to be discussed. The results should be stratified according to the surface-based inversion strength to test for a dependence on stability.

2. This also relates to the BL assumptions. On page 3018, line 18, it states that "the BL is assumed to be well mixed with a constant potential temperature above a reference height of 10 m". While this may be true when the surface winds are strong, I doubt it is true for cases of light winds and so this assumption needs to be checked using the

radiosonde measurements at the 3 stations used.

3. The scaling functions and parameters used in the Monin-Obukhov similarity theory should be updated using the results from the SHEBA experiments. I am aware of at least one paper from that field project that improves the characterization of sensible heat fluxes in the Arctic (Grachev et al., 2005, Boundary-Layer Meteorology, 116, 201-235). The authors should use that improved parameterization.

4. The real test of the results is the RMSE from the model predictions. The RMSE values are 3 to 4oC which are disappointingly large. The authors do a good job explaining sources of error but the reader is left wondering if a better job could not have been done in isolating the main error source. I think it is necessary to perform a detailed case study where some of the error sources from the reanalysis and microwave satellites can be removed. Why not pick a case of completely clear skies and use MODIS to deduce openings in the sea ice. In March the visible images can also be used for half of the day. Also there are frequency overpasses every day at high latitudes which allow higher temporal resolution. This approach may fail if the sea ice is highly broken at a scale less than 250 metres but I think good cases can be found. Also pick a case with a simple synoptic-scale weather pattern so that the regional wind field will be similar to the radiosonde measurement at the station. By performing a few case studies where many of the error sources are reduced you might be able to make a stronger conclusion on the quality of your model.

5. The radiative cooling of the boundary layer is being ignored in the model. Why not include a fixed radiative cooling rate (it probably does not change much if the skies are clear) to the model?

6. How did you handle cases when the measured surface winds were calm at the station even though the reanalysis had a non-calm wind which was used on the back trajectory calculations? This could be another source of larger RMSE.

A couple of minor points:

7. The results are restricted to mostly clear sky cases. Quite different conclusions are possible, particularly under low altitude cloudy skies when large downward IR radiances occur and if stronger winds are mixing the BL to greater depths. I suggest adding “clear skies” somewhere in the title of the paper.

8. In Figure 4 please plot the actual measured air temperature at Tara, the plots only show model output.

Interactive comment on The Cryosphere Discuss., 6, 3011, 2012.