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Comment

***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 #2

Received and published: 10 September 2012

General comments:

This contribution investigates the near- and far-field impact of Arctic sea-ice conditions on near-surface air temperature at particular locations where such has been measured. The strategy is to use trajectories based on reanalysis wind fields together with satellite-derived ice- surface temperature and ice concentration to estimate their impact on the local temperature measurements. Three methods have been used, all giving somewhat different answers. Similarly, the results differ for the three measurement points being considered, and the wind field analyses being used. The impact of differences in ice concentration due to different algorithms has also been investigated

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and was found to be relatively small. The results emphasize the crucial role of accurate wind fields in determining local near-surface air temperature.

This manuscript is generally well written, and suggests new methods on how to estimate the near- and far-field impact on local temperature in the Arctic Ocean in late winter/early spring. Before being acceptable for publication, I think the manuscript still needs several clarifications, and could benefit from rendering it a bit more concise. The methods imply crucial limitations. Most of them have been discussed, but there are additional limitations that need to be addressed (see below). The following is a list of (mostly minor) comments the authors should consider in their revision.

Specific comments:

Page 3016, lines 1-3: What if $A = 80\%$ within a $4 \times 4 \text{ km}^2$ MOD29 pixel? I guess, in this case we are not talking about actual “ice surface temperature”? Could the separately involved passive- microwave derived ice concentration be employed to correct T_i in such situations? In any case, this seems like another source of error to be addressed.

Page 3019: While the “Box model” includes standard ABL calculations, subgrid-scale effects seem to have been ignored. What about the role of pressure ridges and ice concentration in determining the (effective) roughness length (see e.g. Stössel and Claussen, 1993)? This should be added to the list of limitations or sources of errors with the chosen methods.

Page 3021, lines 21-23: Since errors accumulate along a trajectory, the impact of the “large uncertainties” of the far-field remain after passing the near-field, where “the uncertainties are” (presumably) “much smaller”. Also, according to Fig.2, on 20. April, there is a near-field difference in wind direction (or angle of trajectory) between ERA and JRA of about 60 degrees, which most likely amplifies the uncertainties considerably.

Page 3022, lines 7-16: For this example, it would be interesting to investigate the im-

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pact of ice concentration by setting it to a fixed value, e.g. 100%, and redo this for ice surface temperature to separate their contribution to the air temperature variability, respectively. It would also add to the value of this paper if the corresponding near-surface (2- or 10-m) air temperature of the ERA and JRA analyses were looked at for comparison. This could lead to an insightful discussion on the impact of the specified surface boundary conditions on the near-surface air temperature in the respective analyses.

Page 3028, line 22: Whether “thin ice” is counted toward ice concentration or not should actually make a large difference for the sensible heat flux calculations, so I would suggest to elaborate on this topic. The next question is then what the ice surface temperature looks like when thin ice is around?

Page 3031, lines 20-26: You tested 4 different “ice concentration products” using the same “erroneous trajectories positions” for each. Why should the latter then be “masking the inaccuracy of the ice concentrations”? Next sentence: Assuming that a 4x4 km² pixel contains leads, the “prescribed ice surface temperature” somehow depends on ice concentration, though not on the one used in equation (1) (see above). Next sentence: I am not sure what this sudden switch to a “fully coupled model” is all about; again, the satellite-derived ice surface temperature is affected by subpixel-scale ice concentration. When it comes to listing reasons for mismatches one should also include possible effects arising from subgrid-scale heterogeneities that could enhance the heat flux (effective roughness length, etc., see above).

Fig.10: The statistics for Tara is based on data from only one month of one year, whereas that of the other 2 stations is based on 2-3 months of several years. One should thus not put these side-by-side on the same figure, unless perhaps separated by a thick line or explained in the caption. The same holds for Figs.7-9.

Technical comments:

Page 3012, line 20: “Explained . . .” I suggest: ‘There, explained . . .’

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Page 3013, line 17: "...can cause a temperature change of up to 3.5 K." What horizontal scales are being considered here?

Page 3013, lines 17-21: "Reducing the ice cover from 100 to 50 %..." in an uncoupled atmosphere GCM simulation leads to rather unrealistic results. I don't think this citation fits to the other ones presented in this paragraph.

Page 3014, line 20: I suggest to replace "in order to explain" by 'for local'.

Page 3015, lines 10-12: How do the "surface wind fields" of JRA differ from the "10m-wind fields" of ERA? Or is the former a typo?

Page 3015, line 14: "..., both are available...", rephrase to arrive at a reasonable sentence.

Page 3016, line 25: I suggest to replace "regarded" by 'accounted for'.

Page 3017, lines 11-14: This sentence doesn't make sense to me.

Page 3017, lines 18-19: I suggest rephrasing to something like 'The radius of impact can also be derived from temperature changes along the trajectories due to sensible heat flux.'

Page 3018, line 26: Is this "wind speed" at 2 m?

Page 3021, line 19: "750 km after 30 h". According to Fig.2, the "separation" is at most 400 km.

Page 3024, line 17: should read 'of the same order' rather than "in the same order".

Page 3026, line 27: should read '...decrease of r_2 with decreasing trajectory length...' or, to be consistent with corresponding sentences in the previous paragraph: '...increase of r_2 with increasing trajectory length using JRA, and even a decrease using ERA.'

Page 3027, line 17-18: "uncertainties in trajectory positions are relatively small." Not

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for the April 20th case shown in Fig.2 (see above).

Page 3028, line 29: remove “only”.

Page 3029, line 6: ‘surrounding’ sounds better than “environment” in this context.

Page 3029, line 7: behind “ice concentration” one should add ‘ice thickness’, because it affects the ice surface temperature as well, in particular in the thinner range.

Page 3039, line 21: replace “be of” by ‘have’.

Page 3030, line 5: swap “affect” and “also”.

Page 3030, line 6: remove “also”.

Page 3031, line 5-8: It sounds like this summary sentence on the AT and the TV method is not consistent with what is described in section 3.

Page 3031, line 13: remove “where the numbers...” and put ‘(Barrow)’ behind “60 %” in line 11; same for Alert.

Page 3031, line 14: replace “on” by ‘at’. Start a new sentence after “ERA”, and say something about JRA at Tara.

Page 3031, line 18: replace “those” by ‘the’.

Page 3031, line 28-29: “but with...” should be in brackets and start with ‘though’.

Page 3031, line 29-page 3032, line 1: suggested rephrasing: ‘This points to the fact that the spatial variability of the far field contributes noticeably to a local temperature that is otherwise dominated by the near field surface conditions.’

Page 3031, lines 1-5: Not sure what these sentences are all about? It looks like this whole section has been written in haste.

Page 3031, line 14: “pronounced” should probably read ‘robust’.

Page 3031, line 12: replace “seen” by ‘viewed’.

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Fig.6: is there any reason for the banded structure of the model temperatures?

Other minor comment: English comma rules need to be adopted.

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

TCD

6, C1495–C1500, 2012

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