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The Cryosphere Discussions

Interactive comment on "The emergence of surface-based Arctic amplification" *by* M. C. Serreze et al.

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Response to reviewer's comments on "The Emergence of Surface-Based Arctic Amplification"

Referee #1 (R. Graversen)

Major suggestion: Take a look at results from other reanalysis, such as JRA-25 and make some comparisons with results from NCEP.

At the reviewer's suggestion, we have undertaken detailed comparisons between results from the JRA-25 and NCEP reanalysis over the period 1979-2007. ERA-interim, the next-generation ECMWF reanalysis, does not cover the whole 1979-2007 period and so we have not included this product in our comparisons. As discussed in the revised text, and clearly evident in the new Figure 2, which includes results from JRA-

25, the seasonal evolution of surface air temperature anomalies over Arctic Ocean as depicted in the two reanalyses is very similar. Notably, both show emergence of strong autumn warming in recent years. In turn, the two reanalyses are very similar in their depiction of changes in the vertical structure of temperature anomalies. In particular, and as shown from side-by-side comparisons in the revised Figure 6 (and discussed in the revised text), both show that the strongest autumn warming in recent years has occurred at the surface. This is exactly what is expected in response to a growing surface heat source, namely vertical heat fluxes from anomalous open water.

The reviewer argues that one of the reasons we expect the response of temperature in NCEP to sea ice loss is that the warming signal over the retreating ice "resembles that simulated in CCSM3". This is not quite right. As articulated in the introduction, we expect surface-based warming because of the relevant physics. One has open water in autumn where it didn't used to exist. This has to result in a strong heat loss from the ocean to the atmosphere. True, from the earlier version of our paper, one might argue that, despite the fact that results from NCEP make physical sense, we just don't have any faith in NCEP. That we get basically the same results (a surface based signal) using JRA-25, arguably the state-of-the-art of reanalysis, argues that NCEP, despite its shortcomings, is giving basically the right results.

Minor Comments:

1) Page 602, lines 22-24, Land trends vs. ocean trends: Our assertion that trends in surface air temperature over land are smaller than trends over the ocean is indeed based on climate model output. As outlined in the first paragraph of the introduction, stronger surface temperature trends over the Arctic Ocean, as compared to land, is a common feature of climate model simulations (see IPCC-AR4, Chapter 11, page 902 and Chapman and Walsh [2007]) and this makes sense for the reasons discussed above. The reviewer is also correct that the observations point to substantial positive surface temperature trends over land, especially during winter when the albedo effect is not directly at work. As pointed in a number of papers, including Serreze and Francis

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[2006] this can be linked, at least in part, to atmospheric circulation. While direct observations are of course sparse over the Arctic Ocean, both NCEP and JRA-25 point to much stronger warming over the ocean than land, a phenomenon that is entirely consistent with loss of sea ice. That, as also pointed out by the reviewer, JRA-25 assimilates surface temperature from drifting Arctic Ocean buoys, argues further for the veracity of our results.

2) *Page 603, line 9*: The sentence was amended to emphasize that the surface is the dominant anomalous heat source.

3*) Page 604, lines 10-12*: The section in question has been revised. September 2008 had the second lowest ice extent recorded over the satellite era. While not discussed on our revised paper, as expected, autumn 2008 has seen extreme positive SAT anomalies over the areas of summer ice loss.

4) *Page 605, lines 5-6*: The text has been altered to more accurately reflect Graversen's findings. Apologies are offered for the loose wording.

5) *Page 605, lines 24-25*: Our understanding from both the NCEP reanalysis documentation and from conversations with NCEP personnel is that surface temperatures over the ocean are available for assimilation by the NCEP system. What is actually assimilated is unclear. We are of course aware that the JRA-25 features a surface temperature assimilation. This has been noted in the new paragraph at the end of Section 2 that discusses the JRA-25 effort.

6) *Page 608, first paragraph:* Since we are looking at NCEP 2-m temperatures, for consistency, it makes sense to use the NCEP sea ice fields. However, as discussed in the text, the time evolution of sea ice extent from NCEP and NSIDC is very similar.

7) *Page 608, last paragraph and Figure 3*: We could include ice extent anomalies in Figure 3, but the figure would then look very busy. Hence we leave the figure as is. Note that Figure 5 specifically shows the relationship between autumn warming and

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sea ice anomalies.

8*) Page 612, lines 17-20*: We have changed the wording of the awkward sentence. We are aware that Sortenberg et al. (2007) found a bigger increase in the turbulent fluxes than in the longwave, but this is based on the CMIP3 models and is not necessarily right. Furthermore, it may well be that the dominant heat flux transfer mechanism changes with time. This is something that could be addressed in a subsequent paper. That we see a substantial summer change in the net surface heat flux, and not in autumn, is explained by the fact that in summer, the surface temperature (hence the longwave loss) can't change much (you are melting ice or putting heat into the ocean mixed layer). This was already discussed in the text. The reviewer is right that overall, the Arctic Ocean is gaining heat. This is part of the reason why the ice cover has thinned.

9) *Other processes of Arctic amplification not discussed*: The issue of low-level stability in limiting vertical mixing is now mentioned in the introduction. We also now mention the possible contributions of altered cloud cover.

Text suggestions:

These are all suggested minor edits that have been incorporated in the revised text.

Referee #2 (anonymous)

Paper Overview: More clearly delineate competing explanations and why via differential diagnosis Arctic amplification is the key factor in autumn sea ice extent reduction. Specifically demonstrate why NCEP is better than ERA-40.

There seems to be some misinterpretation. The reviewer seems to imply that the autumn ice loss is forced by Arctic amplification. It is other way around - the ice loss drives the amplification. As for NCEP versus ERA-40, part of the problem with ERA-40 is simply that it ends in mid-2002. As outlined in response to Referee #1, the revised manuscript includes comparisons with results from the much newer JRA-25 reanalysis.

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Results from JRA 25 confirm what NCEP is showing.

1) *Line 604, 17-25*: Delineating specific explanations (by numbering) for observed sea ice loss was found to disrupt the flow of the text. Then text remains as is.

2) *Page 605, lines 1-15*: See response to "major suggestion" by referee #1. We conducted an in-depth comparison between the NCEP and JRA-25 reanalyses over the period 1979-2007. They give very similar results. As just mentioned, the ERA-40 reanalysis ends in mid 2002, just about when we started to have extreme September sea ice minima. ERA-40 is hence unfortunately not of much use.

3) *Page 608, line 21*: The wording was awkward and unclear. Then text has been amended What we were trying to get at is that the main driver of the temperature response to declining sea ice (in CCSM3) is the change in ice extent (ice/no ice) as opposed to changes in thickness.

4) *Page 610, lines 12-15*: There is no incompatibility here. Both NCEP and JRA-25 show a link between open water anomalies in autumn and the largest temperature anomalies. It's the lack of ice that is driving the low level autumn warming, not the other way around. Certainly one can argue that with open water you warm the lower atmosphere, which then means more longwave radiation to the surface to inhibit ice formation, but this is not the dominant effect.

5) *Specific comments, page 602*: The term "Arctic amplification" is established in the literature. Adoption of the referee's well-intentioned suggestion of changing it to "Arctic surface air temperature amplification" would only served to confuse. We also think that the sentence in which we first mention Arctic amplification is quire clear in defining it as a surface air temperature signal.

6) *Page 602, line 9*: Same issue as (5): Again, we leave the text alone.

7) *Page 602, line 18*: The text has been amended.

8) *Page 603, line 13*: Again, bad wording on our part. We mean "the apparent poleward

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focus of anomalies". The sentence has been amended.

9) *Page 606, line 25*: The spectral resolution of the model used in the NCEP reanalysis is T62 (nominally 210 km). The spectral resolution of the model used in the JRA-25 reanalysis is T106 (nominally 120 km). However, cell sizes in kilometers at high latitudes will be smaller than the stated nominal resolutions. As stated in the text (page 206, line 22), the cause of “problem cells” is a mismatch between land-ocean masks in sea ice analyses and the model grid. The effects, however, are not confined to mismatched cells but bleed into adjacent cells.

10) *Page 607, line 7*: There are 52 cells identified as problem cells out of 652 cells in the analysis domain. This information is included in the revised text.

11) *Page 608, line 8*: See response to comment #6 of Referee #1.

12) *Page 609, line 3*: We don’t see the value of illustrating temperature anomalies in a table. Figure 3 tells a very compelling story.

13) *Page 610, line 7*: We admit to being puzzled by the referee’s comment. Correlation coefficients between what?

14) *Page 611, line 23*: As outlined earlier, one reason NCEP is better is that ERA-40 only extends though mid 2002, which hence misses the more recent years with exceptional September sea ice minima. As discussed in response to the "major suggestion" of Referee #1, we now include results from JRA-25, which serves to reinforce conclusions drawn from the NCEP data.

15) *Page 613, line 15*: The NCEP data have a lot to say about the importance of the Northern Annual Mode in SAT and sea ice loss. Many of the papers cited on page 604 have made use of NCEP data in such studies. Impacts of atmospheric circulation are discussed throughout the text.

16) *Page 613, lines 17-29*: This paragraph is important in our opinion and we have retained it. Are emerging surface air temperature anomalies still within the limits of

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natural variability? This is a very relevant question.

Referee #3 (M. Pelto)

1) *General comment*: The basic request is to review prior to the discussion section what the surface air temperature anomalies would be in response to different forcing mechanisms (e.g., sea ice loss versus circulation). As far as we can see, we have laid out these issues quite clearly in the introduction. We expect a surface-based amplification due to ice loss (forced by an anomalous surface heat source). The study by Graversen et al. argues that the observations show (in general) maximum warming aloft, more consistent with effects of horizontal atmospheric transport. We argue that if you look at the data including the more recent years, a surface based signal is clear. We discuss how the surface-based signal grows despite temperature changes aloft associated with circulation. We then summarize our findings in the discussion. Hence, we really don't know what more we can do.

2) *Page 609, lines 10-20*: We are not "contrasting SLP changes to SAT and heat fluxes". We are demonstrating how horizontal circulation can transport the heat so that the warming over the open water is spread to surrounding areas.

3) *Page 611, lines 21-25*: We have tried to work in a few more specifics about circulation changes, but the major point we are trying to make is that although there are changes aloft consistent with circulation, these are all superposed upon a growing surface signal. This is exactly what one expects if there is a growing surface heat source.

4) *Page 607, line 9*: Identification of problem cells is discussed in the fourth paragraph of Section 2. There are 52 problem cells out of a total of 652 cells in the analysis domain.

5) *Page 608, line 22*: See response to comment #3 of referee #2. The wording was awkward and the paragraph has been amended.

6) *Page 610, line 21*: There is a slight misinterpretation here. Yes, the period 1983-

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1992 shows mostly negative temperature anomalies, but one has to remember that the sign of the anomalies is going to reflect the base period for calculating the mean. We compute means over the entire 1979-2007 analysis period. Since it was cooler in the earlier part of this time period, these earlier years, by definition, have primarily negative anomalies, even though the period was fairly warm with respect to an earlier (e.g., 1960-1980) base period. The relevant issue is that there is a general trend towards warmer conditions.

7) *Page 612, line 6*: Indeed there were missing words. We meant to say that that the temperature anomalies strengthen from the lower troposphere to the surface.

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