

This paper presents a detailed account of the weather and sea-ice conditions experienced during the HALO-AC3 campaign. I sympathize with writing such an account; it is very useful as a reference for future work, but at the same time in a scientific journal – rather than a data journal – it should have some science in it to motivate the publication in a scientific journal. This often becomes a compromise and the factor that often suffers is the length and the scientific narrative. That is also the case with this manuscript, which is much too long and unfocused; it is unclear if the paper is describing methods and measurements or – as is claimed in the title – the meteorological conditions during the campaign.

Therefore, I am recommending a major revision focusing on reducing the details on how the different analyses were obtained, minimizing the repetition of unnecessary information and streamlining the language.

#### Major concerns

The most stressing concern is the length, the degree of detail and lack of focus. There are too many details that would be better suited in special papers dealing with the different aspects whether that be analysis methods or measurement details. Combined with the rather “flowery” language, where the same thing is not rarely and unnecessarily described in more than one wording makes the reading tiresome; I must confess I gave up reading around page 30 or so. It just has too many details that are better suited in topic specific analysis papers.

The data and methods section (5.5 pages) is much too detailed for this scope of this paper and should be shortened by 50%; I’m sure just condensing the language could do at least half of that. It has an “everything but the kitchen sink” character. For the most important measurement asset – the HALO aircraft – only the dropsondes are discussed (lines 100-104) while the measurements at Ny-Ålesund are twice that long and not really needed; I’m sure these are described elsewhere and can be referenced. The fact that only sea-points where used in various analyses are repeated at least three times; once is enough. Definitions of ARs and MCAOs is also much too detailed and the discussion of the circles flown to estimate vorticity is not nearly enough to really understand how but way too much given how this is used in the upcoming sections.

The painstaking day-to-day-account of the synoptic development on page 11-24 (14 pages!) should be condensed to its main components and shortened to 30% of the present length. The only section that should actually be longer is the comparison to climatology; this is very useful for papers to come. The Ny-Ålesund section is much too long; I think this paper does not really need it and it could be dropped all together.

The section on specific events is what saves this paper; still at 8 pages also this could probably also be shortened.

#### Some detailed concerns:

Line 11: Mentioning “Shapiro-Keyser cyclone” in the abstract is complete overkill; I bet less than a third of all potential readers have any clue what this means for the results.

Line 14-15: Isn’t it natural that conditions during *any* AR would be warmer than climatology?

Line 15: What is significant in the statement that the SIC was within the 10-90 percentiles; that covers almost everything, doesn’t it?

Line 31: The connection between a slightly weaker jet stream and a more meandering flow is far from well established; suggest inserting “possibly” somewhere in this sentence.

Line 38: The statement about warm air gliding up on a cold dome is very popular in some circles, yet I would say it is false. If it were true, what happens to the air under the dome over time? I presume it can flow out of the Arctic during MCAOs, but apparently not be replenished by ARs? Wouldn't that be contrary to having a dome in the first place? Instead – as what the hole campaign was designed to study – warm air flowing into the Arctic is transformed to Arctic air by interactions with the surface.

Line 43: All ARs are not “extreme”; suggest using “large” instead.

Line 56: This is a problem not only for climate models; moreover, the Pithan reference argues for the Lagrangian methods applied in HALO-AC#, but provides no evidence for how this is modeled – poorly or otherwise.

Line 62-63: The wording “does not permit” is too strong. A Lagrangian method does not by itself ascertain proper observation of the transformation and multiple Eulerian observations along a trajectory may provide some transformation information. Its not black or white...

Table 1: With the figure, this table is not necessary.

Line 100-103: Why this degree of detail for the dropsondes? Not necessary in this paper.

Line 119-126: Too much detail; surely there is a reference!

Line 141: IWV is not really a “basic variable”.

Line 142: Strictly speaking this means that all grid points where excluded, since “ $\geq 0$ ” means “larger than or equal to zero”. So if land fraction is zero, implying ocean only, it would also be excluded.

Line 144-145: The “north” subscript is confusing and probably unnecessary. The way this is calculated makes northward transport of excess heat or moisture by definition positive; southward negative. Including this subscript raises the question of you ignore southward fluxes.

Line 155 & 161: Why different units?

Line 164-166: Don't understand; if the bar is too high for an event, then you raise the bar?

Line 173: Excluding land points again.

Line 175: Why use temperature to indicate sea ice? There is sea ice in the model output.

Line 183: And excluding land points a third time.

Line 204-206: Unclear: First, the definition of the gradient is pretty obvious and doesn't have to be described. Second, the potential temperature can increase in the layer even if the average is zero, since the gradient is probably  $< 0$  close to the surface or there wouldn't be any convection.

Line 212-213: The gustiness parameterization has nothing to do with the resolution; it is does to turbulence, which you need an LES to resolve.

Table 2: With the text, this table is not necessary; alternatively use the table a do not repeat the details in the text.

Line 218-224: Do we need this description? I can't see that vorticity is used in the description, and moreover, this description is not enough to really understand what you did but way too much for this paper.

Line 268-270: This sounds a bit too simple to be the whole truth, that the delay in surface warming is just because of the slope of the warm front; at least you show this is the case – or drop the argument.

Line 289-281: Drop “records”; this is not a championship.

Line 283-284: Don't understand the caveat; ist it or isn't it and why?

Line 295: I suggest “indicating” instead of “illustrating”, since you don't show this.

Line 300-307: Why bring in the Shapiro-Keyser classification? Is it relevant an if so, how is it relevant? I bet a majority of readers doesn't even know what this is.

Line 306-307: Don' t understand; if the heat content is low, why is the meridional heat transport not negative?

Line 326: Suggest “dissipating” rather than “being filled up”.

Figure 5: It strikes me that Figure 5 is underutilized; drop it or use it more. Why the change in tilt on 21 March?