

Reviewer #1 reply:

We thank the reviewer for the detailed comments. They helped us to get the message of the manuscript in a more concise and focused way without removing necessary details. Below, we repeat the reviewer's comments in black and write our responses in blue. The line numbers in the line-by-line responses are valid for the revised manuscript.

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 suffer 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 analyzes were obtained, minimizing the repetition of unnecessary information and streamlining the language.

- We moved details from the methods section to the appendix because we think that having some details available might be helpful for full reproducibility of the study while keeping the main body more concise. We also reduced the length of chapter 3.

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.

- We agree that the manuscript was too unfocused and described too many small details. Therefore, we reduced the text length in the revised manuscript to bring the message across more efficiently. We would like to keep the description of all relevant weather events as this manuscript aims to be a comprehensive overview of the HALO-(AC)³ weather conditions. Duplicates of descriptions have been removed.

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.

- Thank you for identifying sections where details can be removed. We reduced the length of the methods section and kept only the most important descriptions. We would like to keep the Ny-Alesund measurement descriptions so that the reader gets a brief overview of the measurements used in the analysis. The Atmospheric River tracking algorithm has been replaced by references to literature.

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.

- We agree that a description of the development day-by-day is too much for a scientific journal. We therefore condensed the weather development description without losing the core information. To avoid adding more length to the manuscript, we did not add information here. However, due to the reduction of the synoptic description, the balance should be improved. We also removed details in the Ny-Alesund but keep it in the manuscript as the additional measurements from this research station might be included in future HALO-(AC)³-related studies. Then having the connection of the weather conditions between the central measurement region of HALO-(AC)³ and Ny-Alesund would prove beneficial.

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

- We agree that the section is also too long and tried to shorten it.

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.

- We changed it to "a strong cyclone" (line 10 of the revised manuscript).

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

- True, we rephrased it to: "due to the strong influence of the ARs " to set the focus more clearly on the effect of ARs (line 12-13).

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

- Here, we wanted to express, that SIC was not extremely low or high, but rather normal. We rephrased it to "the sea ice concentration (SIC) was well within the climatological variability, staying within the 10-90th percentiles over the campaign duration" (line 13-14).

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.

- Agreed, "possibly" has been inserted (line 28).

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.

- We rephrased it to avoid the confusion with the Arctic cold air dome: "When the warm air is pushed upwards over cold Arctic air masses, deep cloud..." (line 37-38)

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

- Agreed, "extreme" certainly sends a wrong message here and is not appropriate. We replaced it by "strong" (line 42).

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.

- We were unsure how to respect this comment in the revision. We would be grateful for an elaborated comment.

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...

- Agreed. We changed it to: "To observe air mass transformation processes along their meridional pathway in a Eulerian view, multiple research stations that are exactly aligned with the wind direction would be needed." (line 61-62).

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

- For full reproducibility, the exact coordinates might be helpful because i.e., the southern limit of the southern region (70.6 °N) cannot accurately be determined from the figure. We shifted this table to the new Appendix A.

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

- We agree that this can be erased and therefore removed details the specification of measurement accuracies (line 93-96).

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

- In general, we agree. But, we think that at least the product names should be included in the main text. The description of the sensors has been removed from the main text (line 106-115).

Line 141: IWV is not really a "basic variable".

- To reduce the length of the manuscript, we removed this subsection (former 2.4.1) as the data processing was minor and already written in section 3.1. Nevertheless, we removed "basic" in line 184 (section 3.1).

Line 142: Strictly speaking this means that all grid points were excluded, since " >0 " means "larger than or equal to zero". So if land fraction is zero, implying ocean only, it would also be excluded.

- Well spotted typo. We meant " >0 ", instead of " ≥ 0 " and therefore changed it (line 125).

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.

- IVT_{north} (or sometimes also vIVT because of the meridional wind, which is often termed "v") is a common expression for meridional moisture transport in the northern hemisphere. With "north", we just wanted to say that in this case, we did not consider zonal transports, just meridional. We rewrote it to "Woods and Caballero (2016) detects moist air intrusions into the Arctic when the vertically integrated meridional moisture flux (IVT_{north}) at 70 °N exceeds ... " (line 130-131).

Line 155 & 161: Why different units?

- We directly used the IVT product provided in ERA5 data, which is in $\text{kg m}^{-1} \text{s}^{-1}$, while they used a similar but not identical product. When converting the $T_g \text{ day}^{-1} \text{ deg}^{-1}$ to $\text{kg m}^{-1} \text{s}^{-1}$, the threshold Woods and Caballero 2016 used is $60.6 \text{ kg m}^{-1} \text{s}^{-1}$. We added this information: "... at 70 °N exceeds 200 $T_g \text{ d}^{-1} \text{ deg}^{-1}$ ($60.6 \text{ kg m}^{-1} \text{s}^{-1}$) over a duration ... " (line 131).

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

- For brevity, we removed the AR detection algorithm description because it is sufficient to refer to literature here. In the old manuscript, we only described the way the revised version of the Guan and Waliser algorithm works. It makes sure that not every wide blob of WAI is detected as AR, which is supposed to have a certain geometric shape. A strong WAI may embed an AR, which can be found when increasing the water vapour transport percentile threshold.

Line 173: Excluding land points again.

- We kept this information because this sea ice mask is different than the others to keep it consistent with Dahlke et al. (2022).

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

- We kept the sea ice mask consistent with Dahlke et al. (2022). We asked them why they decided for skin temperature instead of the ERA5 sea ice model output. It was merely for convenience reasons as the skin temperature is used in the computation of the index anyway. They found that the differences between the skin temperature based and sea ice concentration based mask were negligible.

Line 183: And excluding land points a third time.

- Has been removed.

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.

- We agree that there were too many details regarding the methodology. Therefore, the formula-based description of the vertical potential temperature gradient has been removed (line 154-159). We considered vertical mean, max and min to get an idea of the range of the vertical potential temperature gradient but showed the vertical mean only for brevity. Indeed, the vertical minimum of the gradient is < 0 in a large fraction of the 200 km circle around the Polar Low's centre, indicating convection.

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

- Here, we wanted to say that the ERA5 10 m mean wind might not capture the maximum wind speed of the Polar Low well because of ERA5 has got a relatively coarse resolution. Therefore, we rather considered the gust to have a more realistic view on winds on sub-ERA5-grid scale. We did not intend to have it understood as if the gustiness depends on resolution. It has been changed to: "We decided to use the maximum 10 m wind gust instead of mean wind to get a better estimate of the near-surface wind field that might be hidden due to the coarse resolution of ERA5." (line 160-162)

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

- We cut the details in the text and moved the table to section 4.3, where we added another column indicating whether a condition is fulfilled or not. The presence of the table in section 4.3 might be convenient as the reader is reminded of the meanings of the acronyms C1-C6.

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.

- It is correct that the vorticity is not used in the synoptic description. However, in section 4.3 we compare dropsonde vorticity estimates to ERA5 model output (line 445-455). We reduced the details of the description in the manuscript and refer to literature (line 166-168).

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.

- We dropped the argument.

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

- This part has also been rewritten for brevity and now reads as: "Simultaneously, the latitude-averaged IHT_north and IVT_north exceeded the previous maxima from 1996 ($9.44 \cdot 10^{10} \text{ W m}^{-1}$ vs. $9.32 \cdot 10^{10} \text{ W m}^{-1}$, and $388 \text{ kg m}^{-1} \text{ s}^{-1}$ vs. $384 \text{ kg m}^{-1} \text{ s}^{-1}$, Fig. 5)" (line 210-211)

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

- We wanted to mention that we only focussed on certain regions (those boxes), but not the entire Arctic. Other regions may have experienced stronger MWAs in that time period but we didn't have a look at those other regions. We dropped the caveat as it does not provide information relevant for the key message of the manuscript.

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

- Agreed. We rephrased it to: "After the AR, much drier but still relatively warm air followed, leading to a strong reduction in IVT_north and a slight reduction in IHT_north (Fig. 5)." (line 216-217)

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

- We understand that not every reader might be familiar with the term but during the campaign, this event was always called "Shapiro-Keyser" cyclone. In upcoming studies, it might be that this term would also be used as it represents a turning point in the campaign and would be lost in other cyclones if this classification was removed. We dropped the brief description of the Shapiro-Keyser cyclone characteristics (line 219-221) for brevity as this can be found in the literature.

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

- For brevity, this sentence has been removed as it only provided details of minor importance. Heat transport was negative but not strongly negative. Large negative values require large amounts of heat being transported southwards. If no (or extremely small amounts of) heat is transported southwards (for example during cold temperatures), we'll have only slightly negative values.

Line 326: Suggest "dissipating" rather than "being filled up".

- Agreed. Has been changed to "As the Shapiro-Keyser cyclone stayed over the Barents Sea while dissipating, IWV dropped..." (line 240).

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

- We understand that this figure might appear underutilized. However, we would like to keep this figure as it shows the longitudinal position of the meridional air mass transports and the record breaking IVT_north and IHT_north values. For brevity, the heat fluxes are only discussed around the main synoptic events. Regarding the question: Do you refer to the tilt of the contour lines? They basically depend on the wind regime: If we have weak westerlies, the tilt would be many degrees from the horizontal. For totally meridional winds, the contour lines of certain features (WAI) would be vertical (because of missing zonal propagation). For extremely strong westerlies, the contour lines of a feature would be quite horizontal.