



Cologne, 11 April 2024

Author's response:

Dear ACP editorial team,

thank you for extending the deadline for the submission of the revised manuscript "Environmental conditions in the North Atlantic sector of the Arctic during the HALO-(AC)³ campaign". According to the comments of the reviewers (see line by line response below) the manuscript has been revised thoroughly. Its focus has been refined and now emphasizes on how the meteorological conditions of the HALO-(AC)³ campaign time period relate to the long-term climatology. Because of the shift of the focus of the study, we have changed the title to "Unusual atmospheric and sea ice conditions in the North Atlantic sector of the Arctic during the HALO-(AC)³ campaign".

To address the main concerns of the reviewers the revised manuscript

- has been shortened significantly with a more concise description of the methods, and synoptic sections,
- now focusses on the main meridional atmospheric transport processes (moist and warm air intrusions, and cold air outbreaks),
- has an improved structure, which better separates the synoptic description from the climatological comparison, and
- includes a new analysis to better relate the marine cold air outbreaks to the long-term climatology

Neither this manuscript or substantial parts of it have been published elsewhere in English or any other language, nor is it presently under consideration for publication by any other journal.

Sincerely,

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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. We also improved the structure to avoid repetitions and unclear definitions. 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 is a revised version of a manuscript that is expressly intended to describe the meteorological conditions during the HALO-AC3 campaign. It started out as a very unfocused manuscript, where now some parts have been transferred to a series of Appendices. What remains, however, is still problematic and cannot be published in this form. I've been struggling with what to recommend this time, and as the "unfocusedness" from the previous version was not really reduced, it tells me the author needs more incentive to think anew; hence I now recommend a rejection hoping that the material behind the manuscript can be reshaped into something coherent.

- We have taken the point of the reviewer very seriously. The manuscript has been revised thoroughly to address a single focus, i.e., the question of how representative the conditions during the HALO-(AC)³ campaign were compared to the long-term climatology. By analyzing the meridional heat transport by Atmospheric Rivers and marine cold air outbreak events in the Fram Strait in the context of climatology, we are able to conclude that the conditions were suitable to pursue the campaign's research goals. The description of the conditions is now only a short but necessary part of understanding the setting of the atmosphere. The manuscript, significantly reduced in size, should be now be much more coherent.

Major concerns: I think the main question here is "why". Why write this paper and why with this content? Like I mentioned in my earlier review I do sympathize writing a summary paper from a field campaign; I've been there myself. It is very useful to have one as a reference for all future papers from the campaign! But it will still have to stand by itself as a science contribution. A campaign description – which this is not – should hold something about strategy and thinking; the "how" and "why" and not just the "what". A description of what happened is even more difficult; the science has not yet been done and just an analysis of the meteorology during a time period, just because there happened to be a field campaign during that time, is not really a good enough reason to write a science paper about. Unless some real science is actually revealed in the analysis, which is unfortunately not the case here. So while I can see the usefulness for future reference, I do not see enough science here to warrant publication in a science journal. As a project report, yes; as a paper no. Moreover, most of the data analyzed – sometimes even cleverly analyzed – is ERA5-data and not observations at all. So I struggle with why! Why should this paper be published, except for its values as a useful future reference?

- Thank you for the detailed description of your concerns. As outlined above, we strengthened the focus of the study. As also reflected in the new title, our detailed analysis clearly reveals the extraordinary meridional atmospheric transports (Atmospheric Rivers and marine cold air outbreaks) and their impact on the sea ice concentration in the Fram Strait. Without knowing how the specific conditions of the

campaign relate to the long-term climatology, the interpretation of the airborne observations might become misleading.

In addition to this, I still find the paper poorly organized and confusing to read. Section 4 is mostly a repetition of Section 3, with the addition of the Polar Low and cirrus analysis, although both are mentioned also in section 3, and are both mostly superficial in Section 4; both sections are not needed and represents somewhat different writing structures. Above all, there is a lack of a narrative and reason; there is no connection between the developing meteorology and when the aircrafts were deployed – and how they were deployed.

- We agree that the content of Sections 3 and 4 were similar in some parts. The new manuscript was reorganized and significantly shortened, especially Section 2 (data and methods). Sections 3 and 4 were merged and the "side stories" on Polar Low and Arctic Cirrus were dropped.

The text is often confusing and different definitions are used in the different parts. For example, in Section 4, at the start on line 357, the dates 12-13, 14, 15 and 20 March are listed as having atmospheric rivers (ARs). But in Figure 3a the 14th is not classified at all and the 20th is classified as a cold-air outbreak (CAO). Looking at Figure 2 and the definitions used for an AR (lines 134-136, $60.5 \text{ kg m}^{-1} \text{ s}^{-1}$), there is one AR starting, probably, on the 12th continuing until the 17th with two "strong" peaks (line 136, $> 100 \text{ kg m}^{-1} \text{ s}^{-1}$). The adopted definition moreover requires a duration of 1.5 days, which the 14, 15 and 20 March episodes would not fulfil being only one day each, and a width of 9deg which is not commented at all.

- There was a misunderstanding of the definitions as different criteria are used for Atmospheric Rivers (AR) and warm air intrusions. The $60.5 \text{ kg m}^{-1} \text{ s}^{-1}$ threshold and the minimum duration requirement were not used to detect ARs, but Woods and Caballero (2016) used them to detect moist and warm air intrusions. As stated in Section 2, we used the Guan and Waliser detection algorithm for ARs and define a moist and warm air intrusion by an area average ≥ 0 over the central domain.
- We improved the description of the detection algorithms for the different event types to avoid ambiguity. The different dates regarding the events were due to different regions considered in the respective sections. To avoid this confusion, we only highlight the ARs that passed through the central domain (Fram Strait) in Sect. 3 and 4. Note that also different averaging times can lead to different values.

There are simply too many things that I need to question. The definition of "aged air" is very fuzzy and seemed very "hand waiving" to me. Where does it come from, what is aging and how do you know? The use of circles flights to detect synoptic scale divergence needs a better evaluation before being used in a descriptive paper and here it doesn't really add any value. I suggest dropping it for now. The same goes for the sections on Ny Ålesund observations. Although these are the only atmospheric observations in this paper, it doesn't add any value to the content. The conclusion that there is a one day difference is too simplistic to be useful and the conclusion that one needs to be careful with easterly flow is trivial.

- We dropped the rather exclusive synoptic terms of aged air, but also Shapiro-Keyser cyclone and Polar Low. Because we refined the focus of the study, we dropped the analysis of divergence observations as suggested by the reviewer.
- The analysis of Ny-Alesund measurements has been changed. We now use the data at the beginning of Section 3 to introduce the campaign weather conditions because this is the only radiosonde station that provides long-term atmospheric observations in our study area. Further on, the reanalysis data (ERA5) is used to expand the view to the entire study domain.

Detailed concerns: Line 30: "... at most heights" is better. Done.

Lines 32-34: The argument that the upper tropical troposphere also has an amplified warming is intriguing and interesting; however, it never leads anywhere. Follow up or drop it.

- We dropped the discussion about the jet stream response.

Lines 38-43: This discussion reveals a troubling, however, unpronounced conceptual model. If the warm air was to glide up on top of the cold Arctic air (line 39), where does Arctic air come from? It can exit the Arctic in CAOs but southerly air cannot make it into the Arctic because it glides on top of the cold Arctic air. Moreover, to achieve the suggested downward long wave radiation effects, requires a low cloud base with a temperature close to that of the surface or higher and ascending air cools off. Of course this is not what happens! Southerly air masses are transformed to Arctic air masses by interactions with the new surface.

- We agree that the discussion was misleading and would require a more detailed description of the radiative processes and surface influence on the moist and warm air intrusion. As this would be beyond the scope, we dropped the discussion.

Line 57-58: The sentence "This cloud ... , 2018)" seems to be incomplete.

- We reformulated the sentence to: "This cloud evolution is difficult to capture by atmospheric models (Pithan et al., 2018) motivating dedicated measurement campaigns (Geerts et al., 2022; Lloyd et al., 2018)." (lines 51-53)

Lines 65-68: Long and complicated sentence! I suggest cutting in two: "Therefore, ... the Arctic." And then "This motivated ... (AC)3".

- Agreed. We split the sentence.

Lines 74-75: Operating in a quasi-Lagrangian fashion does not necessarily require longer range, although long range is always good to have. The idea is to repeat visits to the same airmass at multiple missions. The long range comes in handy when following an airmass in over the pack ice, as staging is from far away in Kiruna.

- Agreed. We now say that HALO "has an operating range of 9000 km in altitudes up to 15 km, which is beneficial for quasi-Lagrangian air mass observations" (lines 66-67).

Line 81: Drop "basis". Done

Line 95: This heading just must be a misrepresentation of the HALO contribution to the observations of the atmosphere!

- We changed the heading to "Atmospheric measurements" (line 87).

Line 107: So which is it; a satellite pixel or a model grid cell?

- We deleted the model as this section focuses on satellite measurements only. "Sea ice concentration (SIC), i.e., the percentage of a satellite pixel covered by sea ice..." (line 105)

Line 152: Swap: "turbulent surface heat fluxes" is better. Thanks, done.

Line 164: Confusing; what is referred to here is the fact that a model cannot resolve energy at scales below several times its spatial resolution (theoretically always > 2 ; in practice often 5-10 times). The criteria applied here all come from the model resolved scale and including more grid points doesn't add anything.

- We dropped the Polar Low discussion and therefore also the respective method section.

Line 170-172: Suggest dropping this. It is not really a proven method and the circle used is of the same scale as a Polar Low and therefore this cannot be used to detect one. The results shown here is also less than clear. Done.

Lines 173-180: No trajectories are used in the paper, so I suggest you drop this sub-Section.

- Agreed. Trajectory calculations have been used in the background for our analysis but are no longer shown in the paper. Therefore, we dropped it.

Section 3: This section moves on to the nitty-gritty much to fast. Discuss Figures 2-4 in more general terms so that the reader is familiarized with how these figures are built and can then use them for the more detailed description.

- We removed many details, compressed the synoptic description, and changed the order of the figures to improve the flow and focus of the study.

Lines 188-196: Mostly contentless rambling; use the space more efficiently to really motivate the division into the two main periods.

- We dropped the discussion of the latitude averaged integrated heat and moisture fluxes and made the motivation to split the campaign into two periods more concise (lines 191-197).

Line 198: Drop "corresponding"; not necessary. Thanks, done.

Figure 2: Are the ticks at 00 or 12 UTC. It seems to this reviewer there is only one long AR according to the definition, with a dip in strength around the 15th.

- Note that this figure shows daily averages while ARs are identified hourly. The x tick labels are at 00 UTC but the data is visualized at 12 UTC to represent the ‘middle of the day’. The dip of the IVT_{north} (daily mean and area-average) is therefore on 14 March 2022, which was partly between the two strong AR events (AR I and AR II). Our definition still classifies this day as a moist and warm air intrusion.

Line 218-221: Everything here is from ERA5 so we don't really know that temperature "was" above freezing, in fact I doubt it, and the liquid precipitation was not "documented".

- Liquid precipitation over the sea ice (up to about 80.5°N) was observed with HALO's cloud radar during research flights. Thus, the observations confirm the ERA5 precipitation phase in a certain region. This is now elaborated in the text: "Liquid precipitation at high latitudes over sea ice was also observed by the cloud radar onboard HALO as we detected a distinct bright band at about 0.5–1 km height during research flights. Thus, the observations confirm the presence of liquid precipitation at least in some regions over sea ice." (lines 250-252)

Line 224: I have a problem orienting myself here; I can't find that "low geopotential" which "persisted". Has been reformulated.

Line 226 and elsewhere: It is unclear to this reviewer if a "Shapiro-Keyser cyclone" is a real physical feature or just an alternative conceptual mode. In either case, drop this or explain it properly. What makes it an Shapiro-Keyser cyclone if indeed that is something different to any other extratropical cyclone or Polar Low.

- For clarification: As the respective cyclone was a warm core cyclone and did not feature the typical occlusion front as a Norwegian cyclone, this could be classified as Shapiro-Keyser cyclone. However, we dropped the use of this term because it was not critical for the discussion and to reach a broader audience.

Line 235: I see no evidence of how the "Arctic inversion" looked like or was changed; this seems out of place.

- We used the anomaly maps of 2 m and 850 hPa temperatures to get an idea of the lower tropospheric stability anomaly. However, we dropped the term 'inversion' here as we did not analyze the vertical temperature structure in detail.

Lines 248-251: These seems to be very large changes in the statistics for a change in M from 9 to 10?

- Note that the M values in the figure refer to daily averages over the central domain and thus only few cases with a spatio-temporal average of M larger than 11 K occurred in the ERA5 record.

Line 254: Unclear what is meant by "sub-polar" here; what air mass is this, from where does it come and what is meant by aging? And how do you know? Has been removed.

Line 259: Confusing; isn't this easterly flow? Then downstream would be western Fram Strait which is ice covered and would be on-ice flow.

- For clarification: It was a northeasterly flow at this time and the ice edge in the western Fram Strait was also roughly southwest-northeast. In a small region, the wind flow was actually off-ice. To avoid confusion, we dropped this.

Line 260: Is "attribute" the right word here? This means finding the reason for; maybe use "classification"? Sentence has been dropped.

Line 262: What do you mean by "another weak"; the previously discussed ones were not weak?

- For conciseness, the MCAO event description has been condensed.

Lines 266-267: How can enhanced IWV predominantly due to water vapor close to the surface, where it is warm, be advected by "upper-level flow"?

- For clarification: The enhanced IWV (above 10 kg m⁻²) was due to a weak moist air intrusion over parts the southern domain. Then, in the North Atlantic between Norway and Greenland, a strong vertical wind shear was present where upper level winds transported the moist air northwards into the western Fram Strait. However, we dropped the description of this weak moist air intrusion event.

Page 15, para 1: This whole paragraph precedes the climatology discussion that begins in the next sub-section. We improved the structure.

Line 289-290: I see no reason why there should be a balance here; in fact there should not be a balance since there has to be a climatological heat transport northward to compensate for the radiative net loss at the Arctic TOA. We dropped this argument.

Line 294: Maybe I misunderstand but I don't see the lowest 500 hPa anomalies over Greenland?

- We exchanged the 500 hPa geopotential height anomalies by mean sea level pressure anomalies because this is a more intuitive variable. However, the overall picture remains the same. During the warm period (11-20 March), strongly negative anomalies can be found over Greenland (Fig. 6 b). This has been reformulated to: "The blocking is evident in Fig. 6b, showing the strong anomalies of more than 10 hPa in the MSLP field over the whole warm period with lower pressure over Greenland and the central Arctic, and higher pressure over Scandinavia." (lines 201-202)

Lines 298-299: Again, why should there be a balance and what relevance are these dates in other years? We removed this statement.

Section 3.2 Drop this; doesn't add anything to the manuscript. Done.

Line 251: What are "weather related filters" and "smearing uncertainty"?

- We now explain both terms in the manuscript: "Note that uncertainties of derived SIC in the marginal ice zone are especially large as a result of temporal and spatial interpolation (smearing uncertainty), and due to so-called weather filters. Weather filters remove the atmospheric contribution from the satellite signal to remove false sea ice in open-water regions but run the risk of removing true sea ice as well, especially in the MIZ." (lines 313-316)

Line 365-370: It is unclear if these results and the discussion refers to ARs that start at a given latitude and ends in the Arctic or if some of them in fact ends much earlier. Colder air more northward holds less water vapor and naturally must have lower IWT.

- The definition of ARs is a tricky issue, especially due to the moisture decrease with temperature. Percentiles are used to circumvent this. Here, we do not want to get into a discussion of AR algorithm performance but only explain how to interpret the figure (decrease of IVT with latitude) and why higher latitudes feature lower IVT.

Line 373: What do you mean by time steps?

- For clarification, we reformulated this as: "AR I and AR II both represent strong cases in terms of mean IVT as they partly lie outside the 25th percentile in latitude-IVT space [...]." (lines 231-232)

Line 375-377: Unclear sentence starting with "While..."; why "while"? Has been changed.

Line 381-382: And hence their length becomes dependent on a somewhat arbitrary definition.

- Correct, but this is how the Guan and Waliser algorithm works.

Figure 10: Doesn't make any sense to me! The definition of the "deviation" by necessity makes it zero whenever the 2022 precipitation rate is zero. Presumably, the deviations can be substantial also when this is the case.

- The figure has been simplified for clarity. Deviations are now absolute anomalies.

Reviewer #2 reply:

We thank the reviewer for the supportive review of the manuscript. We appreciate the concerns regarding specific synoptic terms that are not needed for the discussion. Dropping them led to a clearer focus of the study. 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.

The revision has improved the manuscript, the writing flows better and the efforts to reduce the length has also improved it. That said it is clearly not written by someone whose first language is English, and it would benefit from edits by one of the more experienced writers on the long authorship list. The introduction is improved but could still use more polish. I also strongly object to the continued use of the term 'Shapiro-Keyser' without including clear language on what distinguishes a 'Shapiro-Keyser' cyclone from a regular cyclone. This work will rightly appeal to more than just the AC3 research cohort and should be written for a larger audience. Some readers may think differently about cyclones.

- The paper was thoroughly revised emphasizing on the climatological aspect. We dropped the term 'Shapiro-Keyser' cyclone for simplicity and also deleted the part about the Polar Low.

Abstract:

Line 16: what is 'aged subpolar warm air' - in particular what does the 'aged' refer to? I suspect the adjective can be removed without loss.

- For clarification: 'Aged' is a synoptic term that refers to the aging of an air mass when it propagates away from its origin (e.g., aged subpolar air mass refers to an air mass that originated from the subpolar regions but warmed or cooled when it was transported to lower or higher latitudes, respectively). However, we dropped this term to avoid confusion.

Line 19: agree w reviewer 1 that 'staying within the 10-90th percentiles' can be removed, you already say 'within the climatological variability', that's enough. Done.

Line 27: 'temporal shifts up to one day' - not sure what this means. Temporal shifts in what?

- We dropped the term.

'Potential future studies' (line 22) and 'future analyses' (line 31) is redundant. Would suggest removing the first mention although I personally think the last sentence can be removed. It is what we call in the US a 'motherhood and apple pie' statement - something so general, bland and well-accepted that it doesn't need mention. Done.

Introduction:

Line 40: 'the tendency' -> 'the jet stream's tendency'

- Thank you for the suggestion. It would make the sentence clearer. However, we condensed the discussion about the jet stream responses and therefore dropped this term.

Line 45: "the frequency of meridional transport": what does this mean? Transport of what? Frequency measured how? At what altitude ? **Has been dropped.**

Line 50: 'meridional transport' of what? Probably also need to relate transport to a budget, e.g. if the meridional temperature gradient decreases while the tropics warm up, the meridional transport of temperature could increase but still have less impact on the high-latitude temperature budget than when the gradient was stronger. **Has been dropped.**

I am not an expert on the larger-scale perspective either, and a larger lack of confidence is helping to drive Arctic research. I would suggest attempting to communicate that 'questions remain' as opposed to the current more assertive comments, e.g, 'xxx hypothesize that weaker, meandering jet streams will result in....' Then end the paragraph with something on 'most research has relied on global models unable to resolve mesoscale features well, leaving open questions on....'

- Agreed. Due to the open questions regarding this topic, we replaced the discussion by: "Especially the role of linkages between Arctic and mid-latitudes is still under debate. " (line 30)

Perhaps the more senior authors can step up here, to help polish the introduction a bit further? The first paragraph on p.4 is now confusing because the authors communicate instead a focus on MCAOs as opposed to the ARs of the previous paragraph. I think the paragraph may just need an additional connecting sentence to make the segue.

- We reformulated the paragraphs to make our motivation more clear.

p. 9 top of page: please recognize somewhere that comparing a recent time period from ERA5 will assimilated soundings to earlier years with less data assimilation introduces a form of bias.

- We now discuss the potential bias caused by the different amount and type of assimilated data in the summary: "However, it must be noted that the ERA5 climatology may have systematic differences in 2022 compared to previous years as measurements from HALO-(AC)³ dropsondes were assimilated." (lines 347-348)

p. 13 line 323: please remove the presumptive description 'Omega block' here . Such terms get thrown around colloquially in weather discussions but unless they are a major focus of the writing they should not be used. Just describe what you have to say.

- We understand your point that using specific synoptic terms may lead to confusion among some readers. However, as the blocking situation was relevant for the Atmospheric Rivers' path through the North Atlantic (Woods et al. 2013), we would like to keep describing it as 'blocking'. However, as a compromise, we deleted the term 'Omega block'.

p. 17, top of page: sorry, not okay to just throw out the term ‘Shapiro-Keyser’ without describing what it is. Again it may be something that was tossed around within the AC3 weather discussions by a small group of people, but the audience for this manuscript can be anticipated to be larger, some of whom are likely to think about cyclones differently from the AC3 weather forecasting team. Currently what this writing communicates is an author list hiding behind a shorthand they can’t explain, which then suggests that perhaps they also don’t understand. Give this another try and also try not to rely on this term quite so much, it still appears in many places.

- We agree that the type of cyclone is not really relevant to our analysis and, therefore, dropped the term.

Line 423: what makes air ‘aged’ ? I find this an odd term. Does it matter?

- We dropped the term to avoid confusion.

p. 20-21, lines 445-450: again this reliance on a short-hand - Polar Low - that just comes across as slang. How is this mesoscale cycle a ‘Polar Low’ as opposed to simply a mesoscale cyclone? Do circulation features need to meet a quantitative criteria? This is all discussed in more depth in section 4.3, I would suggest just calling it a mesoscale cyclone at this stage in the manuscript.

- We dropped the discussion about the Polar Low to enhance the focus of the manuscript on the main meridional atmospheric transport events.

p. 24 line 526: again this strange habit of calling air ‘aged’. What the heck does this mean? Aerosol can be ‘aged’ but what distinguishes ‘aged’ air from ‘fresh’ air?

- We dropped the term.

p. 28 line 609: nice to see some comparison between ERA5 and the dropsondes, showing that even with the data assimilation ERA5 isn’t getting the full moisture flux. This is worth mentioning I feel. Thank you for your comment.

Section 4.3: so fulfilling 5 of the 7 (or 4 out of 6) conditions put forth by Radowan qualifies a cyclone as a Polar Low? I would suggest stating that explicitly if so. As written the authors are appearing to presume the cyclone is a polar low and then just characterize it using Radowan’s criteria. The discussion between lines 709-715 is nice and I’m fine with the system being called a Polar Low but would suggest rewriting the language so that it is not so initially presumptive. Has been dropped.