

## **Response to Ref. #1:**

We would like to express our gratitude to Ref. #1 for the thorough and constructive review. His/her suggestions helped us to identify and resolve a number of weak or unclear points and formulations as well as to provide some additional analysis to support our conclusions. We will give a detailed response below.

### **Main points:**

*1. This paper discusses the atmospheric influences in the anomalously large variation of Antarctic sea ice observed in 2016. It claims that the early and large retreat of Antarctic sea ice was the result of atmospheric flow patterns, predominantly related to a zonal-wave three pattern until October 2016. Thereafter an atmospheric meridional flow during November, consistent with a negative SAM index, resulted in a "large meridional exchange of heat and moisture". It is generally well written with some nice analysis, although there are a number of missing references and citation errors.*

We have included the suggested references and some additional ones to better reflect upon the current state of research in literature. We also carefully checked all citations and resolved all existing errors.

*2. The paper is quite descriptive. There is no direct objective analysis of atmospheric heat or moisture and no analysis discounting the influence of ocean surface temperatures playing their role in the 2016 event (as mentioned in line 127 regarding the paper by Stuecker). The analysis of zonal-wave three and SAM is similarly rather descriptive and not conclusive. Without an objective analysis I would unfortunately suggest that the paper's claims are not substantiated. That said, I would encourage the authors to complete further analysis and resubmit the paper. More detailed discussion is included below.*

As the title and text define, our study restricts itself and focusses on the *atmospheric* influences on the sea ice behaviour. The *oceanic* influences are not a part of our study, even though they are certainly an important factor. We re-wrote the introduction and the discussion to make it clearer that the topic of our study were the local atmospheric influences on the sea ice, but that the oceanic influence and teleconnections, which we have not been analysing here, play an important role, too.

Although our study is mainly qualitative (as stated in the discussion), it is not just "descriptive". It is a non-quantitative and objective analysis of the contribution of different areas to the total ice loss and investigates the prevailing synoptic situations and processes associated with the sea ice retreat. There are clear physical relationships between the atmospheric flow patterns and the ice behaviour, which can explain the atmospheric influence on the ice melt and its initiation. Our study contains an explanation and interpretation of the interrelations and identifies processes responsible for the 2016 anomalies and is therefore more than purely descriptive. Our analysis of the meridional heat transport is also "objective",

since there is a clear physical relation between the atmospheric circulation and meridional heat transport. In order to better illustrate this relation to the reader and provide a more quantitative measure of the meridional heat transport, we added a figure with the calculated vertically integrated meridional heat advection (from ERA-Interim) and combined it with Fig. 3 and Fig. 4. Then we discuss the SIC anomalies together with the advection field and the surface pressure field. We agree with the reviewer that we did not analyse the meridional moisture transport and therefore removed this from the related sentence in the results section

Naturally, we are aware of the fact that the atmosphere-ice-ocean system is highly complex and we agree that further, quantitative (modelling) studies would be necessary to better understand the processes involved, both in the atmosphere and in the ocean, and also their relative contributions. We extended this thought in the discussion. Such an investigation is, however, beyond the scope of our study.

*Line 23 (and Line 59): "when combined with reduced Arctic SIE" – I don't think this is shown or referenced in the text at all, it just seems to be stated as a fact without proof.*

We added a reference here in the introduction section. (it is highly unusual to give references in the abstract).

*Line 30: It might pay to be cautious in referring to the November 2016 SIE as "extraordinary". The relevance of the apparent sudden variability in net SIE (2014-2016) and the record low monthly SIC for November should perhaps be place into context with longer term variability (e.g. the past variability of SIE as shown in Hobbs et al 2016 and Jones et al 2016) in comparison to the relative short period of observations examined within this current paper. Would "...unprecedented low November SIE, based on the post 1979 satellite data." be more appropriate?*

We agree that this should be formulated more cautiously. Since we exceed the desired maximum number of 250 words in the abstract already, we changed this in the text accordingly and also added some remarks about the time period that was used for comparison in the discussion.

*Line 51f: "reaching record extents in 2013, 2014 and 2015 ..." This should be "reaching record extents in 2012, 2013 and 2014...", and perhaps cite Reid and Massom, 2015 which covers all of 2012 through 2014.*

We agree and corrected this and also included the suggested reference.

*Line 75: Please insert reference for Su 2017.*

Done.

*Line 81f: Please insert reference for Lee et al 2017.*

Done.

*Line 92: "Haumann , 2011" should be "Haumann, 2011".*

Done.

*Line 92: Note that Holland and Kwok's excellent study covers the relationship between wind and SIC trends for the period 1992-2010 and may not necessarily be extrapolated upon without caution. See Kwok et al 2017 for example – as referenced below.*

We included that Holland and Kwok (2012) only cover a sub-period of the satellite record. However, both the study by Haumann et al. (2014) and Kwok et al. (2017) show that meridional winds are also an important driver of long-term trends (entire satellite era; see their respective conclusions). Kwok et al. (2017) show that there are also regions where the relation between meridional winds and drift does not always hold, especially in coastal regions, but also argue that there might be issues with atmospheric reanalysis data in these coastal regions.

*Line 102: Is the term "Usually" based on data since 1979 and your Figure 1? If so then perhaps this should be stated, otherwise please cite some research showing this.*

Yes, we changed the formulation accordingly.

*Line 124f: The sentence beginning with "Ross Sea and West ..." probably needs rewriting.*

Done.

*Line 127ff: It probably should be noted that apart from Stuecker 2017, several BAMS State of the Climate sections mention 2016 Antarctic sea ice, please see the references below. In particular, Clem et al decomposes the atmospheric component, while Mazloff et al mention the ocean influence and Reid et al discuss the sea ice in general.*

We added all the BAMS citations and referred to them in the text.

*Line 182f: This sentence probably needs rewriting.*

Done.

*Paragraph beginning at Line 243: Some of this paragraph confused me. There is mention of, for example, SIE in some regions being close to the long-term average – please see below for direct examples. I think perhaps you are confusing SIE with latitudinal extent, or otherwise could you please make this clearer.*

We re-wrote this paragraph.

*Line 249f: "...with negative anomalies..." – negative anomalies of what?*

Of SIC, we changed this in the text.

*Line 249f: "Largest negative anomalies". What is this a reference to: SIC, SIA, SIE or latitudinal extent? Note that SIE, as you have defined it, and latitudinal extent are two different things. If this is a reference to SIC or latitudinal extent (the contours on Figure 3?) then perhaps there should be reference to Figure 3a? If this is in reference to SIA or SIE then perhaps this should be shown in another way.*

We reformulated this.

*Line 251ff: The sentence beginning "In the Western..." should perhaps be rewritten.*

Done.

*There is also some apparent confusion between what is showing "negative deviations" at approximately 130E and SIE – at least I'm confused!*

See above. We checked this and corrected this paragraph accordingly.

*Line 252: "approximately130" should be "approximately 130" – please insert a space.*

Done.

*Line 256: perhaps this should read, "However, in February 2017 the monthly mean SIA ..." as otherwise this is a little ambiguous.*

We don't understand the reviewer's point here, since the provided line number does not match the text. If the reviewer refers to line number 226 of the original manuscript, we agree that adding "February 2017" is less ambiguous and changed this sentence accordingly.

*Line 267: "Hall and Visbeck should be 2002, not 2001?"*

We corrected this.

*Line 276ff: Other research (Clem et al 2017) has suggested that the SIC anomalies in this region are not necessarily related to atmospheric flow, and that they are related more to weaker ocean stratification and deeper convection over Maud Rise. The opening of the polynya in this region during August 2016 caused quite a bit of media attention.*

Thank you for pointing out that we did not really address the re-occurrence of the Weddell Sea polynya in 2016 and 2017. Actually, this is a very important point and we think that our study can at least partly explain its initial development in November 2016 due to a strong surface divergence, which might have triggered oceanic feedbacks now discussed in the manuscript. We included the polynya in the discussion and quoted the suggested reference.

*Line 307: "This is shown in Figure 6e" should probably be referring to Figure 4e?*

The Figures 3 and 4 have been combined now and the numbers changed accordingly.

*Paragraph beginning at Line 314: There is a concluding summary here for section 3.2.1 that is not reflected directly by the analysis, and revolves around the words "strong warm air advection".*

We removed this paragraph.

*Not once through the paper is atmospheric temperature directly analysed. There is some nice analysis and discussion within this paper, but much of the analysis that leads to this conclusion is descriptive rather than an objective analysis.*

We added a new figure with the northward heat advection from ECMWF-Interim Re-analysis to quantify our result discussed together with the surface pressure field and related winds. The meridional heat advection agrees very well with our qualitative results shown so far.

*Also, a number of papers (and you have cited one) have suggested that the 2016 Antarctic sea ice anomalies were possibly the result of a combination of atmosphere and ocean anomalies. Here you are suggesting the anomalies were the result of a warm atmosphere only, and I don't feel that you have directly shown this. You have not discounted the ocean impacts or objectively shown that a warm atmosphere specifically was responsible for the SIC anomalies. Given your concluding remarks and such a large anomaly in net SIE, surely you would be able to show that there was a large atmospheric temperature anomaly – or ocean surface anomaly?*

We did not state that the observed behaviour of the sea ice were the result of a warm atmosphere only and we agree that oceanic influences and feedback mechanisms might play an important role as well. Our study investigated the atmospheric influences only, but we also mentioned that the ocean is important, too. We reformulated the introduction and the discussion and added a number of studies addressing these issues to make this point clear, also including the effects of teleconnections with ENSO. Nevertheless, our analysis of the temporal evolution of the regional SIA and SIE anomalies imply an atmospheric origin of the anomalies in many regions that is associated with the warm air advection events and a rapid response of the SIA and SIE triggered by the event. We argue that if the ocean was initiating these anomalies, they would occur much more gradually and persist already over a longer time period. However, this gradual evolution of the anomalies occurs partly in the Western Indian Ocean sector and the Amundsen and Bellingshausen Seas, where it is likely that the ocean plays a critical role, as we now explicitly state.

*Line 330: The sentence beginning, "The start of the melt period..." probably needs rewriting.*  
Done. .

*Line 335ff: From Figure 5u it would appear that R7 contributes significantly to the sea ice decay – in contrast to what is said here.*  
We changed this accordingly.

*Line 348ff: From, "It shows generally good agreement..."*. Again, this is based on some quite subjective analysis.  
See above.

*Paragraph starting on Line 358: There is discussion here about the first third of the month of November being significant, and there is reference to Figure 4e. But there is no corresponding pattern of sea ice drift for this time period.*

We do not think it is necessary to show the ice drift for this period since melting is the predominant factor that influences sea ice decay during this period. However, we agree that the previous formulation was a bit confusing and might have implied that such a Figure existed. We rewrote this paragraph.

*Line 361: "...leading to compaction..." and "...decreasing SIC" are not really consistent. Should you split this discussion into something like and inner and outer ice pack?*  
We agree that this was confusing and not quite correct. We corrected this.

*Line 374ff: ZW3 plots are given for the years 2013-2016, but with no real explanation as to why these years were chosen. ZW3 is put into perspective with the preceding three years, but there is no suggestion here that an August-October averaged ZW3 index of  $\sim 0.4$  is significant or not. Is this ZW3 value 1, 2 or 3 standard deviations above normal for this time of the year, has it ever happened before and if so what was the consequence on the sea ice? Indeed, it would appear that for August 2014 the ZW3 was considerably well above that of 2016. In fact, looking at Raphael 2004 it would appear that there are long periods of positive ZW3, but there is no mention of the corresponding SIE, SIC for these years.*

The plots for 2013-2015 are shown to put 2016 into perspective. The index is normally so variable that it does not make much sense to calculate an average. 2016 is distinctly different from the preceding years. While in 2013-2015 the ZW3 index alternated between positive and negative values throughout the year, in 2016, the ZW3 index was almost continuously positive in the winter months. This is unusual and hints at a preconditioning of the sea ice for the later intense melt. We now added a new Figure 8, which shows the meridional heat flux anomalies of the year 2016 with respect to the period 1979-2015. The anomalies reveal a clear ZW3 pattern, which further supports our argument that the year 2016 was clearly exceptional during the period of the satellite record.

*Line 444: Please reference Schlosser 2015, or should this be Schlosser 2016?*

Yes, 2016, we corrected that.

*Line 510: There is no citation of Hobbs et al. Please remove or cite within text.*

We included this citation in the text.

*Line 517: There is no citation of Kottmeier and Sellmann 1996. Please remove or cite.*

We included this citation in the text.

*Line 545: Please remove or cite Peng et al.*

We removed it.

*Line 557: Please remove or cite Schlosser 1988.*

We removed it.

*Line 566ff: Please remove or cite both Simmonds papers.*

We cite both papers in the text now.

*Line 576ff: Note that Turner et al 2009 has been referenced twice – please remove one.*

Done.

*Line 581ff: Note that Turner et al 2014 should be Turner et al 2015.*

Corrected.

*General comments: Is the term Mio, used throughout the text in reference to "million", suitable for this Journal?*

We replaced “Mio.” by “million” accordingly to the journal’s guidelines.

*I suspect that within the text there are some instances where reference to Figure 3 and Figure 4 get confused, or that perhaps that there should be reference to both Figures 3 and 4? For example, Lines 269, 281 and 291 discuss SIC anomalies but both times there are references to the figure showing MSLP (Figure 4).*

Since we combined Fig.3 and Fig. 4 in the revised version, we had to check all references to those figures and corrected them accordingly.

*"Figure" and "Fig" seem to be variously used through the text. Chose one of these that is appropriate for this Journal and be consistent through the text.*

Here we followed the journal’s rule that „Figure“ is not abbreviated when it occurs at the beginning of a sentence, but it is abbreviated when it occurs in the middle of a sentence (see [https://www.the-cryosphere.net/for\\_authors/manuscript\\_preparation.html](https://www.the-cryosphere.net/for_authors/manuscript_preparation.html)). We made sure that all occurrences follow this rule.

*Table 1: There appear to be some discrepancies in the longitudes within this table.*

Thanks for pointing this out. We corrected that. (Some 1s had disappeared due to change of column width.)

*Figure 3: What do the coloured contours represent on these figures?*

The green line represents the monthly mean SIE 1979-2015 the black line the SIE in 2016 for the corresponding month. We added this information in the figure caption and in the text, where it occurs for the first time.

*Figure 4: I have some concerns with this figure. Are the arrows hand drawn and coloured – there is no mention in the text or in the figure caption as to how they are derived? They look rather subjective than objective.*

Since our publication was submitted to The Cryosphere, we address a readership that not always has knowledge about atmospheric dynamics and even meteorologists get a bit mixed up sometimes when it comes to clockwise or anti-clockwise rotation on the Southern Hemisphere, we tried to facilitate the reading of the figure by adding arrows that indicated the warm or cold air advection. The arrows agree well with the blue and red areas in our new figure of meridional heat advection, the latter not explaining the reason for the advection, though. Thus, we would like to keep the figure with the pressure fields, including the illustrating arrows.

We added an explanation of the arrows in the discussion of the new Fig. 3.

*Figure 7b: There is no mention of the hatching in this figure.*

We added this information.

*Suggested References:*

*Clem, K.R., S. Barreira, and R.L. Fogt: Atmospheric Circulation [in “State of the Climate in 2016”]. Bull. Amer. Meteor. Soc., 98 (8), S156–S158, 2017.*

*Jones et al, 2016 Assessing recent trends in high-latitude Southern Hemisphere surface climate. Nature Climate Change 6, 917–926 (2016), 2016.*

*Kwok, R, et al 2017 Sea ice drift in the Southern Ocean: Regional patterns, variability, and trends. Elem Sci Anth, 5: 32, DOI: <https://doi.org/10.1525/elementa.226>*

*Mazloff, M.R., Sallée, J.B., Menezes V.V., Macdonald A.M., Meredith, M., Newman, L., Pellichero V., Roquet F., Swart, S., Wahlin, A.. State of the Southern Ocean in 2016, BAMS, 98 (8), S166-S167, 2017.*

*Reid, P. & Massom, R. in State of the Climate in 2014 (ed. Blunden, J. & Arndt, D. S.) Spec. Suppl. Bull. .Am. Meteorol. Soc. 96, S163–S164 (2015).*

*Reid, P., S. Stammerjohn, R. A. Massom, J. L. Lieser, S. Barreira, and T. Scambos, Sea ice extent, concentration, and seasonality [in “State of the Climate in 2016”]. Bull. Amer. Meteor. Soc., 98 (8), S163–S166, 2017.*

**Thank you for the additional references. We included all of them.**