

Response to Ref #2

We would like to express our gratitude to Ref. #2 for the thorough and constructive review. His/her comments 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.

General Comments

The study is well written, well organized, and a joy to read. However, I do find the study to be a bit too much on the qualitative/descriptive side with many of the claims made on how the atmosphere “influenced” the sea ice being a bit speculative. Furthermore, I am not sure if we are learning anything new here. As stated by Referee #1, many of the descriptive details surrounding the sea ice, atmospheric circulation, and SAM pattern during 2016 are already discussed in the State of the Climate in 2016 Antarctica chapter. Without more quantification of mechanisms, such as quantifying advection, melt, and the role of the ocean, I don’t see what new information is being presented here. I also strongly encourage the authors to place their findings more in context with other work, particularly Turner et al. (2017). I recommend the authors perform a major revision and resubmit at a later time.

As we stated already in our response to Ref#1, 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 between atmospheric circulation and sea ice anomalies and identifies processes responsible for the 2016 anomalies. Therefore, it is more than purely descriptive. Our analysis of the meridional heat transport is also not speculative, 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. Our previous qualitative results were strongly supported by the quantified meridional advection.

Compared to Turner et al. (2017) and the BAMS paragraphs, we not only quantify the advection now, we also analyse the temporal and spatial development of the sea ice decline in considerably more depth and detail than the previous studies and investigate ZW3 in more detail. In addition, we analyze the sea ice area change (temporal derivative) of the daily sea ice area, which provides new insights into the development of the anomalies.

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 their relative contributions.

We extended this thought in the discussion. Such an investigation is, however, beyond the scope of our study.

Specific Comments:

There is a lot of referencing to place names (particularly ocean basins and seas) throughout the study, and so I recommend the authors include a map to go along with Table 1. I also recommend giving new names to the regions (R1, R2, etc.) so there is some connection to their respective geographic place names (e.g., western Ross Sea as wRS, etc.). This will also reduce instances of referring to both the place name and the respective “region” name for clarification in the text (for example, lines 329-331), which is confusing and makes the R1, R2, etc. names seem unnecessary. If sensible region names are defined, they could be used throughout the manuscript without requiring further clarification.

Thank you for this suggestion. We agree that the suggested naming of the sub-regions would make it easier for the reader to follow. Accordingly, we changed them to EWS, WIO, EIO, WP, ERS, ABS, and WWS.

Line 116: Please add citation Meehl et al. (2016) and their finding that tropical Pacific variability also influence meridional winds and associated sea ice extent.

Done.

Line 117-118: As you mention below in lines 125-127, Turner et al. (2017) already established northerly wind/warm air advection was a major contributor to the 2016 record sea ice loss. What are we learning here that we don't already know?

The study by Turner et al, as the purpose of GRL publications is defined, provides quick information about a recent topic in a relatively brief publication. They describe in relative detail the climatological behaviour of the sea ice and then try to explain the features observed in 2016. Our study investigates the temporal and spatial development of SIA and SIE in considerably more detail and depth than a short GRL paper can do. We also added the meridional heat advection for quantification now. Turner et al. use more general phrases, whereas we give detailed information to all single sub-areas and time periods that show that the situation is much more complex than described earlier, i.e. where and why did the warm air advection occur and what was the response of the sea ice. For example, Turner et al. wrote “rapid ice retreat in the Weddell Sea took place in strong northerly flow after an early maximum ice extent in late August”. We show that the western and eastern parts of the Weddell Sea behaved differently (thus more sub-regions than in the Turner paper), and particularly the Eastern Weddell Sea did not contribute substantially to the ice retreat before November. (the western Weddell Sea not before October). Equally the BAMS chapters are rather short and less detailed than our study.

Line 245-246: How does adding two extra sub-areas compare/expand upon the results of Turner et al. (2017)? Please make these new insights clear by placing them into context of Turner et al. (2017).

Our definition of sub-areas closely followed the observed sea ice anomalies and not a climatology established earlier. This very specific definition of sub-areas allowed a detailed investigation and explanation of the observed phenomena and the evolution of the sea ice anomaly. Especially the sub-division of the Weddell Sea was necessary due to the different behaviour of the eastern and western Weddell Sea. We added this reasoning in the text.

We discussed Turner et al. in more detail in the discussion now and explained the differences to our study: Our results agree well with the general findings of Turner et al. (2017), but give more details due to the higher spatial resolution used in our study and also quantify the meridional warm air advection that was discussed only qualitatively by Turner et al. (2017). While they stress the comparison of conditions in 2016 with the climatological means of amount and timing of SIE minima and maxima as well as mean location and intensity of cyclones, our study looks more closely at both the temporal and spatial evolution of SIA and SIE, investigates the contribution of the different parts of the Southern Ocean to ice melt in more detail, discusses the role of ice drift and the relationship between sea ice decay, SAM and ZW3.

Figure 3: Please specify in the caption what the green and grey lines are. I assume green is the average SIE and grey is the 2016 SIE, but it needs to be specified.

We explained this now in the caption and also in the text.

Line 255-256: The negative SIC anomalies in the Amundsen and Bellingshausen Seas actually appear quite similar in magnitude to those in the Indian Ocean. Without quantifying this, I don't think it can be said here.

We reformulated this and referred to the position of the sea ice edge rather than SIC. The differences can be clearly seen in Fig. 3.

Line 309-310 and 314-316: Although I appreciate the schematic arrows, without quantifying advection there is no way of determining that warm air advection explained any portion of the sea ice loss. Furthermore, actual surface air temperature over the sea ice would likely need to be analysed to determine if, even in the presence of warm air advection, temperatures were actually warm enough to melt the ice as the authors claim.

See above: following also the advice of Ref.#1, in order to 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. Our arrows agree very well with the areas of warm and cold air advection from ECMWF-Interim calculations in the new Fig. 3.

Lines 332-334: This seems highly speculative.

We used a careful formulation for this, we do not say that we are talking about facts, but suggest possible reasons. We would not call this speculative.

Please add DOIs to bibliography

Done.

Technical Corrections

Line 94: SIC has not been defined. Please define it here and use SIC for the remainder of the study

Done.

Line 124: remove “were”

Done.

Line 128: change to “December”

Done.

Line 130: remove “rather”

Done.

Line 137: ECMWF is never defined

We explained it in introduction section now.

Line 163: change “today” to “present”

Done.

Line 205: please clarify what “Mio.” Means

We changed this according to the Journal style requirements.

Line 210: no longer need to continue defining SIC, SIE, SIA as they are already defined

Agreed. We changed this accordingly.

Line 259: Change “Figure 6” to “Figure 4”, and please clarify whether this is sea level pressure (as stated in caption) or surface pressure (as stated in text)

The figure numbers have changed because of the inclusion of new figures and change of Fig. 4 and we checked the correct usage of the numbers. We corrected surface to sea level pressure in the text.

Line 269-270: Already defined as the Amundsen Sea Low / ASL, so just use ASL here

Done.

Line 275: Please remove the words “masses” and “right”

We deleted ”right”, but kept “masses”, since “air masses” is a standard meteorological term.

Line 288: Would say ASL instead of “Pacific low”

Corrected.

Line 305: Just put “periods” in parenthesis

Done.

Line 307: Please change to Figure 4e

The figure numbers were changed due to the addition of new figures and are now numbered correctly.