General Comments

The dramatic decline of Antarctic sea ice in Austral Spring 2016 was in marked contrast to long-term positive trends, and the current authors join a small group of previous authors who have attempted to shed light on this. They provide some interesting additions, in particular the presentation of rank maps for understanding the long-term context, the use of lagged correlations with local climate variables (rather than indices) and the discussion of October preconditioning, and the consideration of moisture transport.

However, I judge that the paper in its current form does not provide sufficient insights or conclusions to merit publication. In particular, the main focus and novel feature of the paper is the analysis of water vapour transport but the authors do not justify (from prior work or physical arguments) why water vapour transport would be expected to impact sea ice, or demonstrate in their results that it gives novel information over temperature alone. In addition, the substantial long term context discussion on SIC and circulation overlaps with previous work and more acknowledgement of this is required. I am also not convinced that the presented analysis of the SAM adds value to the paper.

I do think that a substantially re-written paper building on the current moisture transport analysis could be a beneficial addition to the literature.

In terms of method, the stability maps method is clear; I would like to see some further justification of its robustness in the context of the short time series available in the Antarctic. More clarity is also needed on the water vapour variables used.

Regarding presentation, the structure of the paper is clear and the title and abstract are appropriate summaries of its content. However, the introduction does not appropriately introduce the paper. The discussion is also rather general, but I hope that if the authors make edits to the content of the paper, they will be able to draw some more specific conclusions in the discussion section.

The figures and captions also need some improvement.

All these points are detailed upon below.

Specific Comments

- Introduction: This is a general introduction to the importance of sea ice and to the differing observed and modelled behaviour observed at the two poles. It does not introduce the specific questions addressed in this paper. The authors need to introduce:
    - The work of previous authors (as already referenced in the discussion) on understanding the 2016 Austral Spring anomalies, and the gaps in these analyses which the authors address in this paper
    - Why they address moisture transport. Presumably the physical argument concerns downwelling longwave radiation? Some work has addressed this in the Arctic (e.g. Mortin et al, Yang and Magnusdottir) and it would be helpful to cite these.
    - The SAM and other circulation drivers and their relationship to sea ice
- P2, ~L12: ‘In this respect’: this is a slight leap from the argument for understanding physical processes behind the increase, to this paper addressing the dramatic opposite behaviour in Austral Spring 2016. Please reword.

- Data, P2L22: clarify that the linear interpolation is a standard part of the sea ice product (i.e. it is not an addition that you have made, therefore you do not need to justify it)
- Data, P3L2: How exactly is water vapour transport calculated from the ECMWF variables described, and which variables are used at which stage of the analysis?
- Data, P3L7: please expand on the reanalysis' performance as relevant to the current paper; e.g. Bracegirdle and Marshall showed this reanalysis gave good trend/variability of SLP and T at coastal locations. As far as I know there is not an evaluation of reanalysis IWV, due to lack of observations; perhaps clarify this as an irreducible uncertainty on your results?
- Data, P3L12: please give a brief description of the SAM and this SAM index.
- Methods, Stability Maps: The stability map method could be very beneficial in the Antarctic context. However, I am concerned about the shorter record (under 40 years compared to 100 years) and by necessity shorter moving windows (21 instead of 31) compared to your previous work. In particular, given 1979-2016 timeseries there must be 18 21-yr windows, none of them independent? Presumably this affects the interpretation of the stable correlations? Please comment. I was unsure about the defensibility of using 80% significance, but reassured by the fact you only pursue analysis where significance is over 95%; perhaps clarify this in the introduction to the method.
- Sections 3.1-3.3: the long term discussion overlaps significantly with previous work. More care is needed in citing these other papers (and perhaps shortening your description accordingly) e.g. BAMS state of the climate 2016 report sea ice section and relevant points from papers you already cite in the discussion. In particular at pg 5, line 2 (end of section 3.1) reference Turner et al 2017, who explicitly show the anomalous sea ice retreat in November 2016; and at page 5 line 20 cite BAMS state of the climate: Antarctica: Atmospheric circulation.
- P4 L24: I don't think WPO contributes notably to September SIE anomalies (Figure S2)
- P4 L27-28: Figure S2 shows WS does not become negative until the second half of November?
- P5 L23: I do not think you can argue the westerlies resulted in positive temperature anomalies; rather, all associated with same patterns of variability.
- Section 3.2: Be careful in your use and discussion of the SAM, if retained.
    - It's not clear which behaviour you are saying 'projects onto the positive phase of the SAM'; the wavenumber 3 behaviour or the zonally symmetric annular structure. The Marshall index describes primarily zonally symmetric behaviour, whereas EOF based Antarctic Oscillation indices do capture the 3 centres of action.
    - I'm not sure it's useful; in some months (September and November) the circulation does have three centres of action in the expected places. However in October in particular it is not 'SAM-like' at all (either in the zonal-mean or zonally asymmetric sense). I would therefore suggest writing the discussion without reference to the SAM, and concluding the section with a short section, perhaps referencing a map of the SAM's typical behaviour (e.g. Spring SLP regressed onto the Marshall index, as a supplementary figure) and noting the months in which the circulation looked very SAM-like and the remarkable values of the index in this month. It would also be worth checking whether the SAM rankings are broadly robust to use of a different index.

- Section 3.3 and throughout: please ensure you are clear and consistent in your use of the terms 'water vapour', 'water vapour transport', 'total column water vapour',' integrated water vapour' etc and their associated acronyms. They were used inconsistently to the extent of my being unable to understand what was being used at all points

- Section 3.4: To me, the conclusion to be drawn from the maps and indices here (figs 5 and 6) is that there is a lagged local effect between temperature/water vapour and SIC in the regions analysed. This is perhaps not very surprising although I'm not aware of previous lagged analysis. Some questions to address to interpret the results:

    - To what extent is this a manifestation of persistence in SIE anomalies?

    - What are the results for the Weddell Sea in December (where anomalies are greater than in the Ross Sea)? 2016 saw rapid development of WS anomalies in November, implying that at least in this year, it was not just anomaly persistence.

    - Does IWV add more (statistical) information, or more physical understanding, over T alone? Figures 5 and 6 show the same regions in the stability maps for IWV and for T, and the timeseries look very highly correlated. A physical discussion and some supporting evidence is needed: is the same circulation bringing in heat and moisture? Or are water vapour anomalies radiatively driving temperature anomalies? Since this seems to me to be the main result of the paper, it is necessary to argue either that the water vapour has some independence from and therefore added predictive power over temperature, or that it adds physical understanding to the sea ice anomalies which cannot be inferred from temperature alone.

    - You discuss a dipole of stable regions. This could be related to the ASL, which would cause co-variability in the Ross and Amundsen-Bellingshausen sea, although the footprint in the Weddell sea is larger and further to the east than I'd expect in this case.

- P7L5: note that the highest SIE over this area is broadly true even when normalised anomalies are used (BAMS state of the climate figure 6.9); i.e. even accounting for natural variability these regions are exceptional in 2016.

- P7 L7; give the main results from the maps before discussing the indices.

- Section 3.5:

    - It is unclear which water vapour variables are used. Which figures show IWV and which show IWVT? Please check units, acronyms etc.

    - I found this hard to follow. I suggest rewriting this section such that each 2-day regional mini case study, is addressed with a little more care (maybe use 'first the ABS','second the IO','thirdly the RS'). Can you link these transport events to e.g. cyclones?

    - Take care over extrapolating to 'decline in first two weeks of December'. The anomalous decline in early December was in the WS (Fig S2) so I don't think you can robustly link it to the event shown.

- Section 3.6: This analysis does not add anything to the discussion about 2016, nor much to understanding of SAM-sea ice relationships in general. Table 2 is valuable but I think this discussion could be removed and replaced with a few sentences in the discussion e.g. 'Given the dramatic SAM anomalies in some months, we investigated the long term relationships between

regional SIE and the monthly SAM. However, significant relationships were found for only three month-region combinations (of 20) and thus consistent with previous studies […], we find the SAM's role in sea ice variability is complex.' The moving window method could be used to enhance this if the analysis is felt to be critical to the paper. Unless the relationship of the SAM to moisture transport is explicitly addressed, I do not think it adds to the novel scientific content of the paper.

- P9 L14; Is this lowest for November, or lowest overall? Is it true for other SAM indices?
- P9 L31; you say you've shown moisture and temperature anomalies could 'also' have led to different anomalies. 'Also' implies it's something different; are the anomalies you've shown not manifestations of the circulation anomalies discussed? 'poleward advection of warm': you don't show heat transport so this is a slight assumption, although I think Schlosser et al do- please cite.
- P10 L13 ; 'poleward advection of moist and warm air': increased moisture is not necessarily due to increased moisture advection?
- P10 L21; The study of Woods et al was about the Arctic. It's not clear it is relevant here.

Technical Corrections

- Abstract, Line 13: 'lowest daily sea ice concentration anomalies' -> 'largest magnitude negative daily sea ice concentration anomalies'
- Although it's almost certain readers know what SIE is, give the abbreviation at first mention (Pg 1 L25) or at first use in methods section (Pg 2 L23) [rather than at P2 L8 as done in this version].
- Page 1 Line 23: Artic -> Arctic
- P2, L7: Colins -> Collins
- P2, L11: clarify from offset what months 'Austral spring' is.
- P3, L20: on time-> in time
- P3, L21: Bracketed 'e.g.s' unnecessary and make text more confusing.
- P3, L31: Ionita et al (2017) reference should be Ionita (2017)?
- P4 : there is inconsistency between use of abbreviations ('WS') and full names ('weddell sea')
- P4 L4: 'problematic' a little emotive. Try 'challenging' or 'confusing'?
- P4 L5: '8'-> 'eight'
- P4 L6: 'positive SIE anomalies over the whole Antarctic region' -> 'positive pan-Antarctic SIE anomalies'. I don't like the phrase 'pan-Antarctic' much, but 'whole' implies 'everywhere'.
- P4 L20-21: rewrite e.g. 'In December the whole RS, and most of the ABS and IO were characterised by negative SIC anomalies'
- Pg 5 line 5: 'to the zonal' -> 'of zonal'.
- P5 L13: 'were'-> 'where'
- P5 L19: 'warming' -> 'warm anomalies'

- P6 L5: ‘particular’ -> ‘exceptional’
- P6 L7: delete sentence ‘The rank maps are computed…’
- P6 L10: ‘to be able to clearly capture…2016 was’ -> ‘for clarity’
- P6 L13: delete ‘Figures 3a,b and c indicate that’
- P6 L19, L23, L28: ‘southern’, ‘eastern’ and ‘north-west’ should be ‘northern’, ‘western’ and ‘north-east’? Please check!
- P7L6, 16; delete ‘respectively’
- P7 L9: ‘considered’->’consider’
- P7 L24; ‘December TT’-> ‘December TT2’.
- P7 L27; delete ‘a’ before ‘predictive’
- P8 L2 ‘there’->’three’
- P8 L3; ‘employing’ -> ‘spatially averaging’?
- P8 L4 ‘region’->’regions’
- P8 L5: delete comma after ‘IWV’
- P8 L10: reference Fig 7d
- P8 L13: ‘BAS’->’ABS’.
- P8 L24: ‘trimming’->’timing’.
- P8 L17: Reference Figure S2.
- P9 L15 ‘led to’->’associated with’ (the SAM is in part expressing the behaviour of the westerlies. It is not a physical driver.).
- P9 L31 ‘lead’->’led’.
- P10 L16 ‘this’->’these’
- P10 L16 ‘moist’->’moisture’.
- P10 L21; ‘In recent’ ->’In a recent’.

Corrections: Figures

- Figure 1: Optional, but inclusion of topography and lines of latitude could make this a more useful reference figure for readers not as familiar with the Antarctic.
- Figure 2: The vectors are too small to be legible, and are not described in the caption. Repeating sector names in-plot is unnecessary here and the legibility is poor, so I suggest removing.
- Figure 3: As Figure 2 for sector labels. Remove ‘2016’ in subfigure label titles. I also suggest you change the caption text to ‘Ranking of 2016 monthly: sea ice concentration (SIC, first column, ‘1’ means lowest SIC in period); integrated water vapour (IWV, second column, ‘1’ means wettest

month in period); temperature (TT, third column, '1' means warmest month in period). The period is 1979-2016. Rankings below 5 appear white."

    - Technical question; how do repeated '0's rank (i.e. if several years have SIC=0?)
    - The colour scale is probably not colour-blind friendly. Please improve.

- Figure 5 and 6
    - The correlations in e) and f) would be clearer if the SIE anomaly were negated.
    - Add title to colour bar.
    - Simplify caption text for panels a-d to (Fig 5 example): 'The stability maps between November Indian Ocean (IO) sea ice extent and a) October IWV, b) November IWV, c) October TT, d) November TT.
    - Please check panel refs in caption to Fig 6; both time series panels referred to as c).
- Figure 7
    - Check journal standards on colour scale (avoiding use of green-red for example).
    - It would be helpful to show again in this figure the squares used to define regions.
    - What are the vectors?
    - Please clarify the dates shown in timeseries (are all days shown or only October and November) and check use and consistency of IWT/IWVT etc (see my general comments) and units.
- Supplementary Figure 1: In caption, mention 2016 just once, and state what the red number in each panel is (the ranking of that month's regional SIE).
- SF2: It would be helpful to include full regional names once more and to either change colours or find another way of making them clearer (IO, ANT and WS are very hard to distinguish) and of separating the full Antarctic time series from the individual regions. Y axis: Sie-> SIE.
- SF3: Interpolation of contours here is unhelpful as there is no propogation of anomalies in the x-direction. I suggest using a block rather than interpolated contour. Also please change colour scale and use less classes for legibility.
- SF4: See comments on (main text) Figure 3 caption and colour scale.

References

Mortin et al (2016): Melt onset over Arctic sea ice controlled by atmospheric moisture transport. DOI 10:1002/2016GL069330.

Yang and Magnusdottir (2017): Springtime extreme moisture transport into the Arctic and its impact on sea ice concentration. Doi 10.1002/2016JD026324.