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Interactive comment on "Challenges associated with the climatic interpretation of water stable isotope records from a highly resolved firn core from Adélie Land, coastal Antarctica" *by* Sentia Goursaud et al.

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Summary

In this study, the authors examine the accumulation, isotopic (δ18O and d-excess) and chemical records (MSA, nssSO4) from a shallow firn core (TA192A; 66.78°S; 139.56°E, 602 m a.s.l.) from Adélie Land, Antarctica. The 21.3 m core was retrieved at a coastal high-accumulation site. Local meteorological station data, accumulation stake data, an isotope-enabled GCM and back-trajectory analysis are utilized to examine the signal

that is captured in the core. These highly-resolved records only cover the last two decades, coindiding with Interdecadal Pacific Oscillation's recent negative phase, but provide a rare opportunity to examine climate dynamics from the East Antarctic sector. A sector that in general suffers from a dearth of observations and where historically recovered cores, in general, are from low accumulation (low resolution) sites from the East Antarctic plateau.

I recommend that this study will be accepted but with major revisions.

Comments to the authors

Major comments:

The TA age scale

There is a significant correlation between ECHAM5-wiso δ18O and DDU SAT, but the correlation between TA δ18O and DDU SAT is insignificant. As the lack of correlation consistently appears to be associated with the TA isotope records one can suspect that an error in the age scale introduces an offset between the TA isotopes and station data, and between the TA isotopes and the ECHAM5-wiso data.

"No significant isotope [d18O or d-excess]-temperature relationship can be evidenced at any timescale, ruling out a simple interpretation of in terms of local temperature." (Abstract L.15). "ECHAM5-wiso produces a significant relationship between δ18O and temperature, a feature which is not identified in the TA record." (P22 L17).

I encourage, the authors to investigate if the lack of significant relationship for the TA isotope records with SAT station data, with ECHAM5-wiso SAT and with simulated isotopes can be due to an error in the TA age-scale. The age-scale can be susceptible towards this type of error as there appears to be a lack of age constrains for the core. If one annual layer count is missing this would result in isotopic data being assigned climate data that is one year or more too current. For example, looking at one single year, if a layer-count is missing, values that are truly 2011 would be assigned as 2012 and thus aligned with too contemporary climate data. These concerns prompted me to try to understand the TA isotopic data, that is, to check if there is an offset associated with the age scale. I created a new depth-age relationship, which includes one more annual count. You didn't include this layer, but you had flagged it as ambiguous (~0.68 m depth, dashed black line in Fig. 3).

Correlation maps

I correlated the TA isotopic data (δ18O and d-excess; estimated from Fig. 7) with ECMWF ERA-Interim (Dee et al. 2011) geopotential 500-hPa (z500), surface air temperature (SAT), and meridional (v850) and zonal (u850) winds, as well as with HadISST sea ice concentration (SIC) (Rayner et al. 2003). These correlations were done both with the original and updated new depth-age relationship. I made plots where the upper set of panels show the modified age scale and the lower panels shows the original (Figs. R1–6).

Here is why I think the updated age-depth relationship, with the ~0.68 m layer count, supersede the original age-scale.

*The nssSO4 peak is in the 100 ppb range and the nssSO4 peak is associated with a well-defined δ18O peak.

*The correlation maps between δ18O and the monthly reanalysis fields display higher significance when the updated age-depth relationship is used (Figs. R1–6). (This is a somewhat circular argument, but it provides enough of an indication that the age-scale needs to be better constrained.) The higher significance for the updated age-depth relationship is particularly clear for sea ice (Fig. R3). Figure R3a indicate that a negative sea-ice anomaly towards the Southern Ocean in the 120°E–150°W sector and off the coast of Princes Elizabeth Land (65°S, 75°E) is associated with a positive δ18O anomaly. Note that no significance pattern appears in these sectors when the original age-scale is used (Fig. R3b). It is well-known that water isotope records from coastal locations are significantly influenced by regional sea ice conditions (Noone and

Simmonds 2004; Küttel et al. 2012). Sea-ice advance and retreat are affected by (and interact with) large-scale atmospheric circulation modes.

*The significant pattern with the updated relationship indicates a contemporaneous, in sync, relationship between δ18O and the monthly reanalysis fields. Positive δ18O anomalies tend to be associated with a low-pressure system (clockwise rotation) centered over Tasmania, Australia. The low appears to conduit wind and warm air south (Figs. R1a and R2a). Onshore meridional winds (negative v850) are associated with positive δ18O anomalies, see green shading close to the site. Vapor transport associated with this path tend to be linked with a positive δ18O anomaly as the air comes from a warm open water source region. This is a common correlation pattern for coastal Antarctic ice-core sites (Abram et al. 2011; Thomas et al. 2013). However, commonly with a larger significant region compared to the pattern shown in Fig. R2a. Note that the SAT pattern is significant just off the coast of the site (Fig. R1b), however, only over a limited extent. The v850-wind anomaly close to the site provides an indication that the age-scale and climate data is in sync as the anomaly occur in the atmosphere and thus isn't associated with long memory effects (lead/lags), as can be the case for sea ice and SST. If onshore winds drive warm ocean air toward East Antarctica (positive TA δ18O anomaly) the warm air can potentially linger and cause SAT anomaly on the plateau the following year. Thus when the reanalysis data is misaligned with the isotope data, a significant positive SAT anomaly pattern can appear over the plateau (Fig. R1d). (Alternatively, if the original age scale is correct, you didn't see this positive anomaly over the plateau as you used the DDU station SAT time series instead of reanalysis fields.)

The next set of figures shows d-excess correlation pattern with z500, 2mT, v850, u850 and SIC (Figs. R4–6). Positive d-excess anomalies is associated with anti-cyclone at ~55°S and 90°E (Fig. R4a) this high force winds along the coast towards the site (Fig. R5b). There is a similar pattern of significant zonal easterly winds that approach the site from the Ross Sea side (Fig. R5b). These easterlies appear to be associated with a weak high over the Ross Ice Shelf and a low-pressure band to the north of the shelf. The easterlies appear to bring vapor to the site from the eastern Ross Sea polynya and sea-ice margin (Fig. R6a). The westerlies associated with high off the East Antarctic coast (∼65°S, 90°E) appears to provide a similar conduit of vapor from polynya region (Fig. R6a). The d-excess signal originating from these regions can be anomalously positive as evaporation occur at a high-latitude low RH setting and the transport path towards the site can be associated with kinetic distillation processes as the air parcels are advected over ice, which further increase the positive d-excess anomaly. The cold atmospheric temperatures in combination with the long distance travelled by the air parcel over ice (without re-enrichment from an ocean source), can allow for a greater expression of the Rayleigh distillation and temperature dependence of kinetic effects, which is associated with higher d-excess values (Jouzel and Merlivat 1984; Risi et al. 2013). This is also consistent with your results, namely, the anti-correlation between d-excess and SMB; as the air is advected over ice shelf and sea-ice it will have experience "rainout" before reaching the site and thus the high d-excess values will be associated with relatively dry conditions. The setting with a positive SIC trend in the 110°E–160°W sector (Fig. R7) can also be important for the d-excess trend as it provide a setting with anomalously northerly sea-ice margin where air can be advected along the coast without contact with an ocean source.

Note that both the eastern Ross Sea and the sea-ice region off the coast of East Antarctica (∼55°S, 90°E) have seen a significant negative sea-ice trend over the examined 1997–2014 period (Fig. R7). This is particularly clear for the eastern Ross Sea. The negative eastern Ross Sea sea-ice trend, therefore, provides a compelling explanation to the positive d-excess trend. I think it could be interesting if you could show whether these transport pathways exist. You can do this by showing the paths associated with the back-trajectories, perhaps as clusters. Note that air originating from these sea-ice regions doesn't necessarily have to have seen a significant increase, that is the transport path can always have been there it could be that the polynyas just recently became active. However, your findings that back-trajectories from the WAIS+Pacific

region have increased is consistent with a positive d-excess trend, assuming that the air pass over Ross Sea sea ice or shelf prior to reaching the site.

The examined interval, 1997–2014, is short and almost exclusively coincide with the Interdecadal Pacific Oscillation's recent negative phase. Thus, it should be cautioned that your results and these correlation plots might look different if the examination period had been longer. I suggest that you show correlation maps similar to those that I introduced. Local conditions like those presented in Figure 6 in the paper does not necessarily show any influence on the isotopic record. For example correlation plot with reanalysis z500 fields can provide you with information about the pressure systems that are associated with air advection to the site and the isotopic signature. A pressure time series from a nearby station will not provide you with this information. Thus I suggest that you replace Figure 6 with correlation maps. Note that it can also be fruitful to correlate d-excess with SST and RH reanalysis fields, to check if any region stands out. I can share my Matlab analysis code for the correlation maps if that would be helpful? Contact the editor if you would like to get a copy.

The dating process of this core seems to be challenging. However, by not providing any reanalysis correlation maps or investigate the records relationship with standard indices (such as for ENSO and SAM), it feels like you have not exhausted the standard methods used to interpret the isotopic signature of an ice core. Combining the correlation maps with the back-trajectory analysis can be a powerful approach to test the d-excess hypothesis and thus aid in addressing the goals of the study (P7 L7). I don't think it is justified to call the research challenging (referring to the article title) before you have tried additional analysis. The record you present here is interesting so it also deserves a more exciting title.

The introduced update of the age-depth relationship, should not be viewed as a permanent update. Instead, it was meant to raise a concern. It would be good if you can find some age constrains for the core. The data for the closest stake needs to be included in Fig. 3 if it is central for the age-scale. I like the age-scale section otherwise it has enough detail to ensure reproducibility. Are there any other chemical records from the core, any other shallow cores or pits (that can provide a historic snow surface), or can you use the Acoustic Depth Gauge (ADG) record of snow surface height from the D-10 station? In your 2017 study, you used NH4 and Na, but they might not add any additional information? You couldn't match the isotopes with the S1C1 record, but is it possible with any other records? This might be helpful if the S1C1 age-scale is associated with less uncertainty. In the future, can there be something to gain in terms of resolution to measure these cores on a continuous-flow analysis (CFA) setup or increase the sample resolution of the discrete samples? We were fortunate on the project that I worked on researchers had visited there in the 1970s and retrieved cores. From their pioneering research, we got additional age-constrains from their snow surface at the time, plus beta-counts and we were able to match our isotope record with theirs (Emanuelsson 2016; Winstrup et al. 2017).

Age-scale thresholds

The introduced threshold for your annual counts does not capture the older/deeper part of the record. Note how the picks at ∼12.2 m, ∼11.2 m, and ∼10.5 m (currently assign for the year 1999, 2000, and 2001) does not exceed the 100 ppb nssSO4 threshold. There, of course, has to be layer counts for this section, but they are not strictly speaking captured by the introduced rules. Perhaps this can be linked to the low proportion of air parcels that come from the Indian sector at the beginning of the record? You could might thus get less of an ocean signature. Please cite earlier studies that have successfully used nssSO4 and/or MSA for dating (e.g., Steig et al. 2005; Abram et al. 2013).

Back-trajectories

Show your results on maps too. Consider cluster analysis for the trajectories or it might suffice to show just the 5-day endpoints. The path can be important though, consider this case. The air is classified as WAIS+Pacific as the endpoint fall in this sector, but

the air advected from this sector can be re-enriched from an ocean source or polynya on its way to the site.

I also suggest that you split up the Indian sector, it covers a too vast region. In my mind, the region from 0°–90°E is likely to be associated with a different signature compared to direct onshore moisture transport. Sticklers might also object to that the Indian and WAIS+Pacific sectors intrude into the Atlantic Ocean sector. Also, why do you leave a region south of New Zealand undefined? Is it a concern that the start elevation of 1,000 m a.s.l. does not correspond to the elevation at the site (602 m a.s.l.). One of the findings in Goursaud et al. (2018a) was that the site elevation was important for the d18O-d-excess phase-lag.

"Finally, we associated each daily back-trajectory to daily precipitation δ18O and d-excess values simulated by ECHAM5-wiso in the precipitation, and classified the time series for each variables by back-trajectories sectors." (P22 L3). Did you report any of the results from the d-excess back-trajectory analysis? Is there a way to tag the back-trajectories with TA d-excess data? It would be neat, if you can tag the back-trajectories with monthly TA d-excess values and then show that high d-excess months tend to originate from (or pass over) the Ross Sea and the off the coast of East Antarctica (∼65°S, 90°E) negative sea ice trend zones. To pinpoint these area, you might need a finer sector resolution and to also look at the path not only the 5-day back-trajectory endpoints.

Post-depositional effects

I think three years is a too short period. Your argument, as you pointed out, is assuming that there is no natural interannual or inter-decadal variability. What ratio do you get if you just split the records in two parts (8 yrs in each)? If you want look at intra-annual (seasonal or monthly) isotopic resolution I still think you would have to consider diffusion. However, I think your analysis and the correlation maps (Figs. R1–6) using annual values shows that you can also obtain interesting information from the annual records.

Note that one approach would be to forward-diffuse the isotope records (Vinther et al. 2010; Küttel et al. 2012). You can use the methodology in Küttel et al. (2012) and then compare the results from using the forward-diffused record and the untouched record to evaluate objectively if there is a difference.

ECHAM5-wiso

Could the S1C1core's age-scale be better constrained than the TA core? Beta-counts were used for the S1C1age-scale, which can help to reduce the uncertainty. The significant correlations for the S1C1 with the model data would indicate that this could be the case and the lack thereof for TA could indicate that TA's age-depth relationship is not as well constrained. This provides another indication that an annual-layer count might be missing for the TA age scale. These cores are located only 14 km apart, so they will be exposed to similar climatic conditions. It thus seems to be a hasty conclusion to attribute the lack of similarities for the accumulation records of the two core solely to S1C1. "the absence of similarity between the TA and the S1C1 accumulation reflects the uncertainty in the S1C1 dating resulting from the large spatial variability and from more frequent erosion processes occurring at the S1C1 site" (P24, L1).

For the same reason, I would caution you against being too strong with your criticism against the model simulations here. The analysis that you perform requires that the age-depth is well constrained, that is, that the isotope data is aligned with the GCM data year by year. (The seasonal cycle comparison should not be as sensitive though.)

SMB

Are you using a sufficient number of points (stakes and cores) to get conclusive SMB results? Perhaps it would be wiser to leave the SMB part for a later publication? For that study, you could include all the available stakes and cores. I suggest that you home in on one or two novel ideas. I also suggest that you work on focusing and shortening the paper, clarify sections, and work on presenting some findings in figures instead of the text. (I can also become better at this). I like your short summaries at the end of

each section and they are written in a clear concise way. Strive towards getting this level of clarity throughout. Suggestions: focus on the δ18O and d-excess TA data, interpretation of the signal (explain the d-excess trend), the isotopes relationship with the ECHAM5-wiso model, and back-trajectories. These findings seem novel, robust and interesting. Remove the snow surface sampling archive part, the SMB and the δ18O/d-excess signature analysis? You can still publish these findings elsewhere.

Specific comments:

I suggest that you add a subscript to δ18O and d-excess when you refer to ECHAM5-wiso data. That way the reader doesn't have to look back in the text to check if it is the simulated or TA data that you refer to.

Article title

You can perhaps change the title. You have to show that this is true first, but here is my suggestion. "Recent positive trend in d-excess in an Adélie Land coastal ice core, Antarctica, is linked to the activation and increased distant advection from the eastern Ross Sea polynya".

Abstract

P1 L2. Provide the name of the core here too: Terre Adélie 192A (TA192A).

P1 L5. It is not necessary to include "hereafter" when you introduce a new acronym. Writing "…reconstructed surface mass balance (SMB)" should suffice. Change this throughout. In the abstract you might even consider avoiding the acronyms altogether to be brief.

P1 L15. Remove the space so it says: "isotope-temperature relationship"

P1 L20. Use another type of dash for intervals, that is, the en-dash: "135 °–145 °E" and "1998–2014". Change throughout.

Introduction

P3 L22. Change this sentence so it reads? ...are thus essential to provide continuous local to regional sub-annual resolution climate information spanning the last decades. You would ultimately be more interested in information about past climate conditions, not just a couple of decades back. Resent data is important for the interpretation of the isotopic signal as there is an overlap between the observational records and proxy records.

P3 L24. The SMB acronym has already been introduced on line 11.

P3 L27. The ITASE cores (Mayewski 2005; Steig et al. 2005) are not coastal (at least not all of them), unless you define everything that's not on the plateau as coastal. They are similar to coastal cores, however, in that they are exposed to synoptic conditions.

P4 L5. Change to "initiated" instead of "triggered"?

P5 L3. I would remove "until now". Otherwise it sounds like you have resolved this issue here once and for all in this study.

P5 L11. Under-documented, is not a common word (at least not according to google Ngram), replace? Can you say, "remains poorly documented and understood", instead?

P7 L19. Stick to one convention for units, so write "3.9 m s-1" here instead.

P6 L17. Remove "search" here and say something like, highlights the importance of retrieval of more

P6 L22. 139.56°E not S.

P7 L6. I would say resolution instead of scale here.

P7 L7. Remove ", and dated." And start a new sentence and say "these records were used to establish the age scale for the firn core.

P7 L10. Section references can be shortened, e.g., Sect. 2. Material and method

P7 L18. No need to write "(see Fig. x), (Fig. x) should be fine. Change throughout.

P8 L4. You write ", respectively" here but it's not clear what it refers back to. Rewrite so this sentence becomes clear.

P8 L8. Write Picarro without all the caps.

P8 L16. Use the DDU acronym.

P8 L18. When in 1959?

P8 L24. Change to "range" not "ridge". And change the dash symbol for the range.

P9 L3. Change to "... km from the TA drill site" instead.

P9 L10. Move Table 1 to the supporting material?

P9 L15. Change to "back-trajectories" to be consistent with the rest of the text

P9 L19. Change to "DDU intra-annual precipitation variability." instead.

P9 L20. If you use the 0.75°x0.75° grid data, wouldn't the grid point at 139.50°E be the closest point to the drill site, Not 140.25°E? Note that you can choose even higher resolution when downloading the data.

P9 L28. 1500 m a. s. l. not m. a. g. l.?

P10 L24. Change to "...same simulation as Goursaud et al....", instead of "...same simulation than Goursaud et al. ...".

P10 L26. Can you add one more year of ECHAM5-wiso output data to the analysis to match the analysis period of the core?

P10 L27. End the sentence with a period.

Results

P11 L7. Change to "multi-year study"?

P13 L5. "...obtain a ratio of 0.5 (see Section 3.3)". Did you want to reference the supplementary table again, Table S3? In the supplementary table, it also looks like the ratio for d-excess is 1.1, not 0.5. I think you are a bit too thorough when you write (in the Supplementary Material) for each entry. Write it one time and then after that Fig. S1 and Table S1 etc. should suffice.

P15 L5. You write nssSO4 in many different ways throughout the paper (and define the acronym at several places). I assume they are the same? Stick to one.

P15 L24. You mean $p<0.05$, not >0.05.

P15 L25. The significance level should be <0.001 than, if $r=1$? Be consistent with the significant levels throughout, that is, include the <0.001 level too.

P15 L27. Change to ".... in Fig.6. In Figures 6 and 7,..".

P16 L1. Change to " Figs. S5 and S6" instead.

P16 L5. "The sea ice trend is the largest in summer, they disappear if we discard the value observed in 2013." This sentence seems ambiguous, as it can be interpreted as it is only the trend in summer that is lost if 2013 is omitted. Split the sentences into two parts.

P16 L18. Delete the "at all" part of the sentence.

P16 L17 It is not clear what ", respectively" refers back to here.

P16 L26. It is just one value. You can present the p-value here instead of providing the range.

P16 L27. Is it anti-correlated or positively correlated? This is hard to follow.

P20 L4. "This relationship is valid for all seasons."

P20 L7 "m.s-1" the period is not needed.

P20 L12. Correct figure reference, (Fig. S8).

P20 L14. and L18. I understand why you have put the table and figure together. However, it is probably better to present them separately and refer to them as (Fig S#) and (Table S#) in the text.

P20 L14. Indeed there is a large spike on 7 May. Nevertheless, there are also other anomalously positive events during 2007 that contribute to the annual anomaly.

P20 L20. Please clarify this sentence. This is a case where I think a subscript can be handy and a table to refer to. As you have simulated and TA d-excess and DDU and wiso 2mT, it is hard to keep them apart. Especially in a long sentence like this.

P20 L24. Please provide a p-value.

P21 L12. Change to "On average,".

P21 first paragraph. You say that the calculation are based on the 1998–2014 period, on the 2nd line, that should be enough, so you can remove the "over the period 1998–2014" text from line 13, 13–14, and 15 as it is redundant.

P22 L4. "and classified the time series for each variables by", Change to "variable" instead.

Discussion

P23 L3. "slightly increasing but not significant trend". I'm afraid that you can't call it a trend if it is not significant. If it has some significance, $p<0.1$, you can perhaps call it weak.

P23 L18. I believe it is just called sastrugi when plural too.

P24 L3. Are you referring to the wrong supplementary figure here?

P24 L16. Is this p-value correct? Do you use too many zeros?

P25 L3. The formatting of this header has been lost.

P25. L27. Did you use 10 points (see Fig. 11) or 12 (see the referenced line) for the smoothing and running correlation?

P26 L13. "As a result, the δ18O-d-excess relationship may be a fingerprint of changes in air mass origins, and particularly of the occurrence of precipitation of air masses coming from the WAIS+Pacific sector". From visually inspecting Figure 7a (and from what you mention in the text) it seems like air originating from WAIS + Pacific and the Ross Sea sectors has increased. If your cite statement above would hold up, how would I see this in Figure 11? The frequency of occurrence of significant relationships (bold line) seems quite even throughout. 2007 and 2011 stand out as anomalies, but so does 1999, 2001, 2003. Or is 2007 more remarkable because it is a negative anomaly? Or do you mean that a spike in the slope signals an abrupt change in air origin, e.g. a sudden change from Indian to WAIS+Pacific? Are there any remarkable conditions associated with the year 1999, 2001 and 2003 too?

P26 L17. "... respectively (consistently with what obtained within each year, see...). "...-1, respectively (this is consistent with the annual means, see...). Does this sound better?

P27 L7. Change to "not only with stake data closest to the drill site".

P27 L11. "However, we cannot draw any conclusion..."

P27 L12. "(Unfortunately, there are no striking features during the records common period, which makes it challenging to match the isotope records)".

P27 L18. "These findings suggest".

P28 L2. "(from ice cores, snow precipitation, and water vapour)'

P28 L5. "We compare the chemical concentrations recorded in the TA firn core with S1C1core, for their common period (1998–2006). The mean concentrations are slightly lower for the..."

P28 L15. "Air originating from near the sea ice margin may contain relatively high d-

excess". In general, I think to say just "air" is fine, that is, the "mass" or "masses" part is often not needed.

P28 L17. "Such a configuration..." this sentence is confusing as it doesn't appear to correspond to the setting described above. The presiding sentence describes a low sea ice setting and the latter describes a positive sea ice scenario, so these sentences don't seem to correspond with one another. "Such a configuration should be associated with high sea-salt concentrations due to increased sea-salt emissions when the sea-ice concertation is low in summer."

P28 L25. Chang to "probably caused by marine air advection".

P28 L26. It might be better to avoid starting sentences with a conjunction ("And" here) in formal writing, as some people might object to it.

P29 L13. Change the highlighted parts of the sentence "...insure that small scale SMB random variability caused by presence of sastrugis, dunes and barchans is negligible..." to "ensure that small-scale SMB random variability caused by presence of sastrugi, dunes and barchans is negligible".

P28 L19. "In contrast, vapour formed over the ocean..."

P29 L1. Change to "To summarize," or "In summary," instead.

Conclusions and perspectives

P29 L11. Change to "The high estimated SMB rate of 74.1 $\pm$ 14.1 cm w.e. y-1 limits the effects of diffusion and ensures that records with sub-annual resolution are preserved."

Figures

Change the figure texts so that not everything is in bold. Just keep the fig numbering (Fig. 1, etc) and the letter (a) indicators in bold. Change throughout this section. It is not necessary to write "in" when you specify units, e.g. (in ‰. Change throughout the document.

Fig. 2. Remove the 45°S boundary in the figure, otherwise, it looks like these regions have a northerly limit at this latitude.

Defining sectors:

*It is confusing that you use both signs (-, +) and letters to indicate the hemispheres. Stick to just letter.

*I also suggest that you write the intervals as ranges as you do otherwise. That way you can skip the “lat”, “lon” text too, S/N and W/E provides you with this information. Example: Ross Sea sector: 0°–75°S, 180°–240°E. You write that you don’t have any equatorward boundary. Assuming that you don’t have any endpoint in the NH you can write this way.

*Skip the dash in “Ross-Sea sector” as it is a name. I also don’t think you need all of the quotation marks when you introduce the names, see if you can write this in a clear way without them.

*Note also, that you wouldn’t usually call a sector that extends so far into the Atlantic Ocean, Indian or WAIS+Pacific.

*Apply this formatting guidance to where you define sectors elsewhere in the text too, e.g. P8 L25 and P10 L12.

Fig. 3. The δ18O record seems to be shown both as regular value and a smoothed record, but you don’t tell the reader how the record has been smoothed. You reference Fig. 3 in the figure text, but this is Fig. 3. For clarity it would be better if you list what the figure show here and describe the methodology elsewhere.

Fig 6. Local sea-ice extent with a % that is in the 124–134% range, this looks odd to me? Fig. S6 should be the same just with the standard deviation too. However, note here how the range of sea ice concertation is different. You write sea ice concentration in the figure text and sea ice extent on the figure axis, please clarify. All that said, I rather see that you scrap this figure and replace it with correlation maps.

Fig. 7. Mention the panels in order, that is, accumulation occurs last, but is mentioned first in the text.

Fig. 9. A letter is missing text inside the figure, it should say “Plateau”. Can you increase the length of the bars (a, b) so it will be easier to see differences between the regions? You could also consider splitting a, c and b, c into two separate figures and align a, c so that the years in the panels correspond to each other, that is so that 1998 and 2014 is aligned in a and c. And that the moths are aligned in a similar manner in b, d.

Fig. 11. Maybe start the sentence with “Ten” instead of 10. There is also one parenthesis bracket too many.

Sentence 2. Change to “Significant results are indicated by thick lines ($p<0.05$)”. It is confusing that the secondary axes exceed 1.0. Also, you might want to change so it’s not using a comma for the axis text, (e.g.1.0 instead of 1,0).

Supplementary material

The CNRS affiliation is missing for Vincent Favier, Suzanne Preunkert, and Michel Legrand. Show figures and tables as separate items. Start by list the tables Table S1, S2, S3, . . ., then continue with the figures in a new section (Fig. S1).

Table S7 “. . .and deuterium excess (“dxs”) from. . .”, change to “. . .and d-excess from. . .” to be consistent with the rest of the document.

Place the text associated with figures below the figures.

The year 2014 seems to be missing in Fig. S8. Add one more year of ECHAM5-wiso output data to the analysis to match the analysis period.

Article references

Please, format the references list so that there is wider line space between individual article entries (like in the Reference list below). It is hard for the reader to identify where one entry start or ends otherwise.

The Cavalieri et al. 1996 reference does not appear in the Reference list.

References

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Figures

Figure R1. Correlation maps between TA δ18O and annual-averaged ECMWF ERA-Interim (Dee et al. 2011) (a, c) z500, and (b, d) SAT. (Upper panels; a, b) show the correlation for the 1997–2014 period using the updated "New" age-depth relationship and (lower panels, c, d) shows the correlation for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients. Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. The black dot indicates the location of the TA192A drill site, also show in subsequent correlation maps (Figs. R2–6). Note that the TA δ18O annual values used here are not the exacts annual averages as they were estimated from Goursaud et al. (2018b) (their Fig.7) for review purposes only.

Figure R2. Correlation maps between TA δ18O and annual-averaged ECMWF ERA-Interim (Dee et al. 2011) (a, c) v850 and (b, d) u850 winds. (Upper panels; a, b) show correlation maps for the 1997–2014 period using the updated "New" age-depth relationship and the (lower panels, c, d) show correlation maps for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients. Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. Note that the TA δ18O annual values used here are not the exacts annual averages as they were estimated from Goursaud et al. (2018b) Fig.7 for review purposes only.

Figure R3. Correlation maps between TA δ18O and annual-averaged HadISST SIC (Rayner et al. 2003) for (a) the 1997–2014 period using the updated "New" depth-age relationship and (b) for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients (r, $p < 0.1$). Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. Note that the TA δ18O annual values used here are not the exacts annual averages as they were estimated

from Goursaud et al. (2018b) Fig.7 for review purposes only.

Figure R4. Correlation maps between TA d-excess and annual-averaged ECMWF ERA-Interim (Dee et al. 2011) (a, d) z500, (b, e) SAT. (Upper panels; a, b) show correlation maps for the 1997–2014 period using the updated "New" age-depth relationship and the (lower panels; c, d) show correlation maps for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients. Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. Note that the TA d-excess annual values used here are not the exacts annual averages as they were estimated from Goursaud et al. (2018b) Fig.7 for review purposes only.

Figure R5. Correlation maps between TA d-excess and annual-averaged ECMWF ERA-Interim (Dee et al. 2011) (a, c) v850 and (b, d) u850 winds. (Upper panels; a, b) show correlation maps for the 1997–2014 period using the updated "New" age-depth relationship and the (lower panels, c, d) show correlation maps for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients. Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. Note that the TA d-excess annual values used here are not the exacts annual averages as they were estimated from Goursaud et al. (2018b) Fig.7 for review purposes only.

Figure R6. Correlation maps between TA d-excess and annual-averaged HadISST SIC (Rayner et al. 2003) for (a) the 1997–2014 period using the updated "New" depth-age relationship and (b) for the 1998–2014 period using the original age-scale. Shading shows correlation coefficients (r, $p < 0.1$). Black contours enclose regions where the correlation coefficients are significant at the $p < 0.05$ level. Note that the TA d-excess annual values used here are not the exacts annual averages as they were estimated from Goursaud et al. (2018b) Fig.7 for review purposes only.

Figure R7. HadISST SIC trends (% dec-1, shading) over 1997–2014, using all months. Black dashed contours enclose regions where the regression is significant at the p <

0.05 level.

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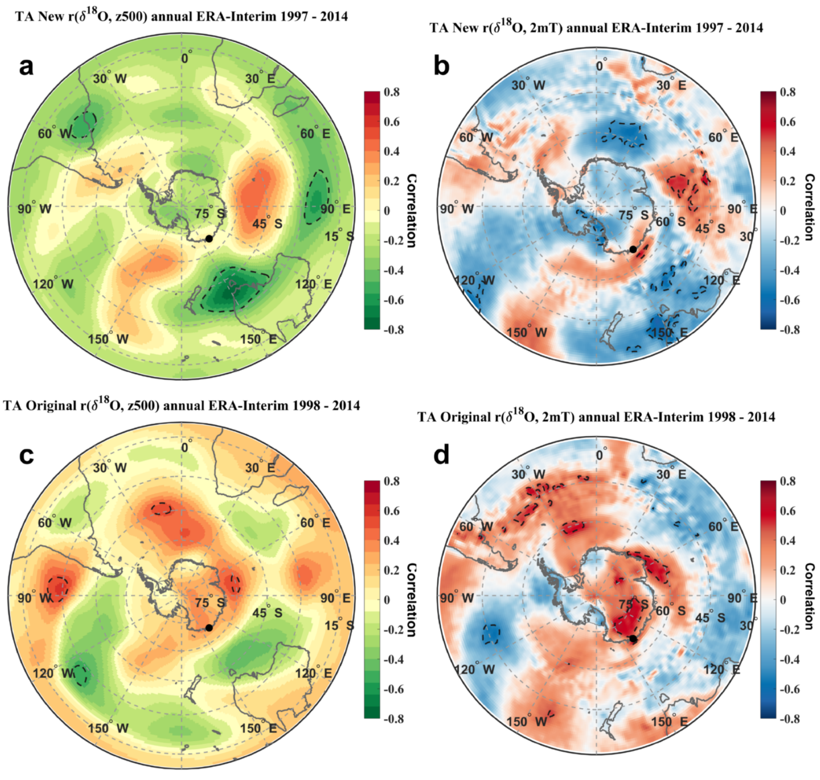
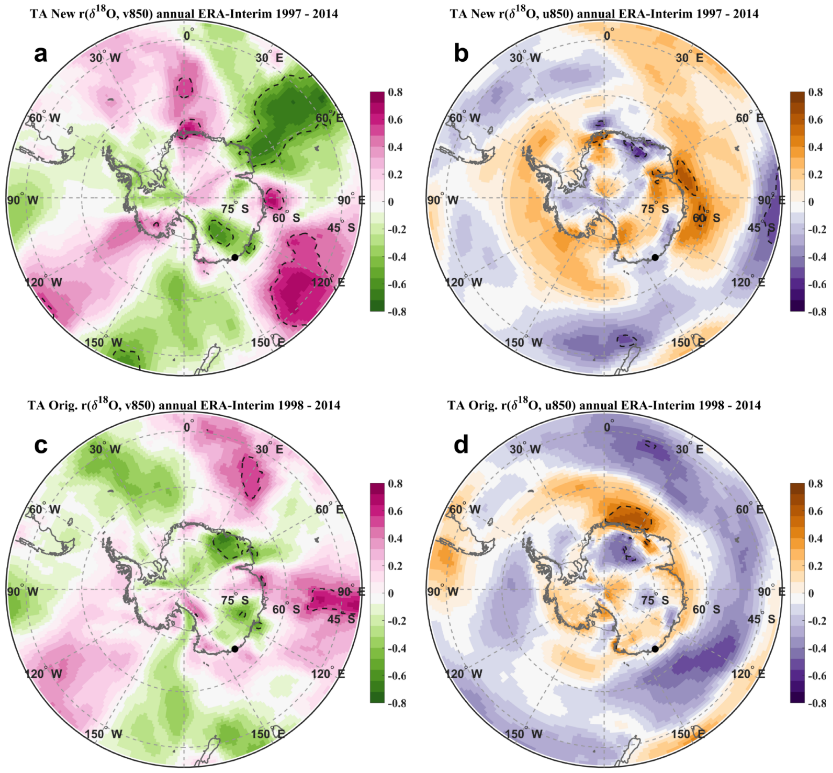


Fig. 1. R1



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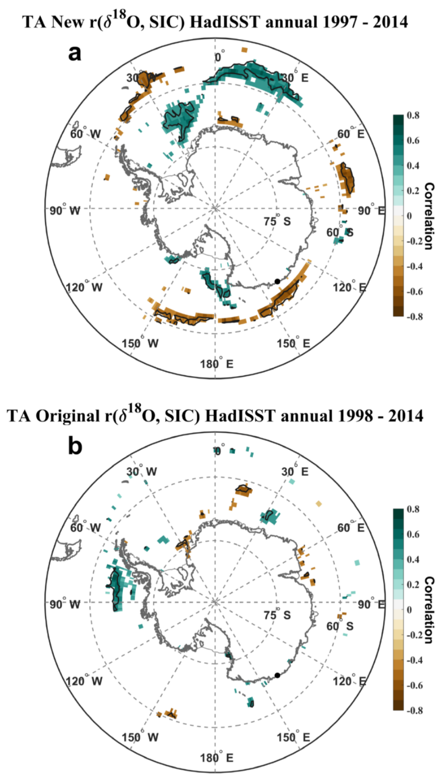

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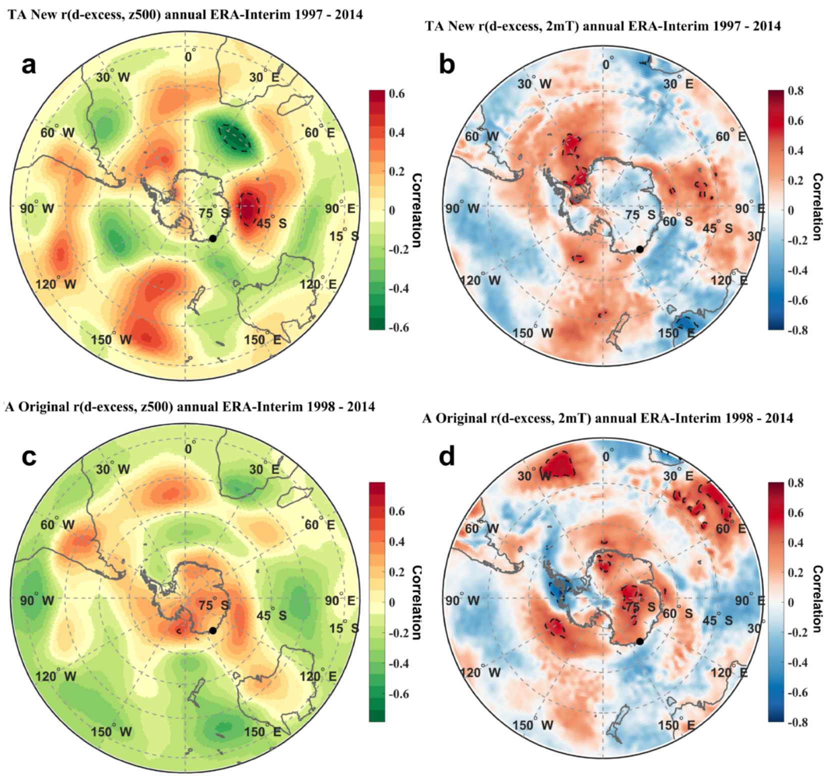

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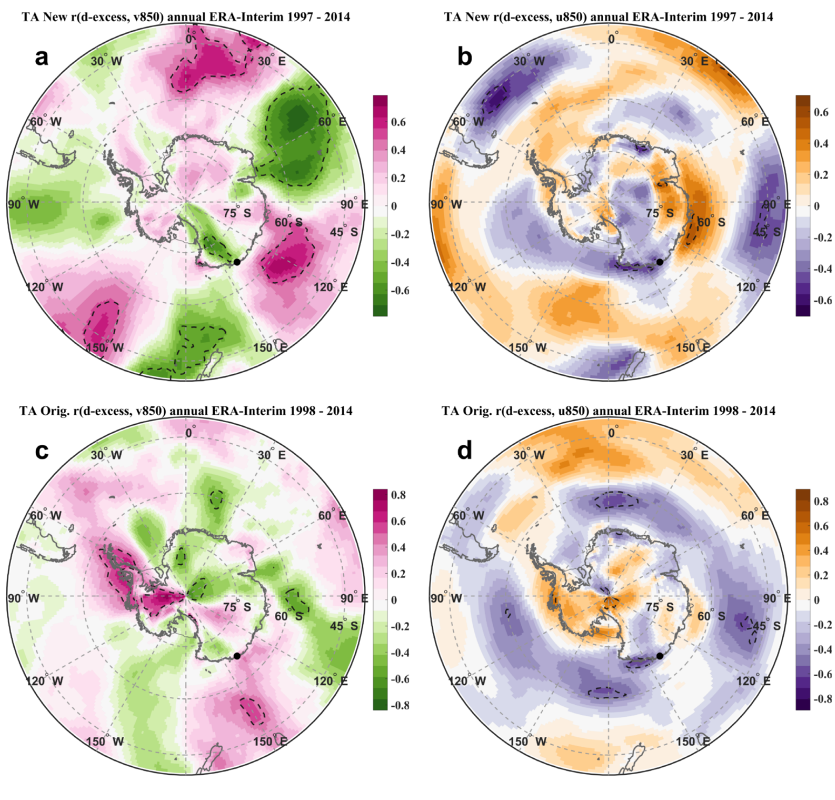

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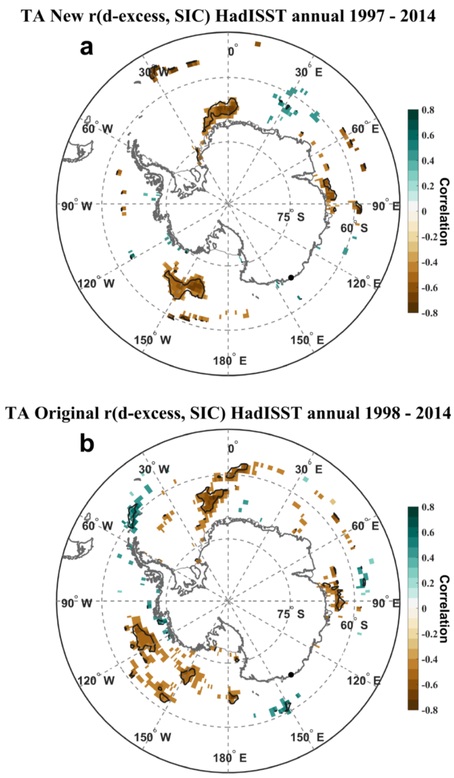

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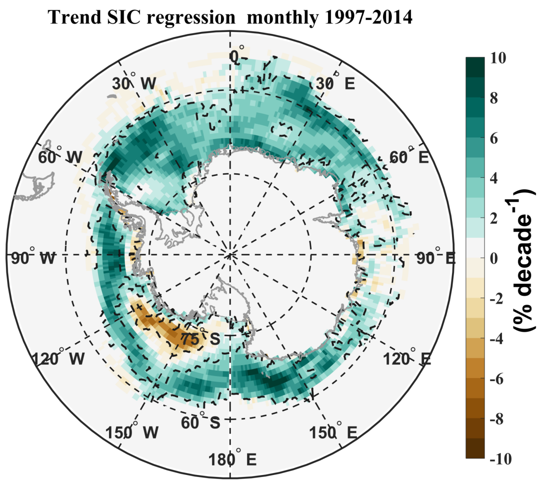

Fig. 7. R7