

Interactive comment on “Stable water isotopes and accumulation rates in the Union Glacier region, West Antarctica over the last 35 years” by Kirstin Hoffmann et al.

S. Goursaud (Referee)

sentia.goursaud@lsce.ipsl.fr

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Summary This study describes new stable water isotope and surface mass balance records from six ice cores in the Ellsworth region, at the crossing point between West Antarctic Ice Sheet, East Antarctic Ice Sheet, and the Peninsula. This region is poorly understood in terms of climate variability. Thus, the new datasets provide substantial inputs for extending our current knowledge of recent climate variability of Antarctica, and the manuscript fully fits within the framework of TC. However, I have a few major concerns. First, I suggest that the paper should be re-articulated at some points, to show more explicitly the results which are robust, the uncertainties and clarify the

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underlying hypotheses. The methods used in this study, which are mainly based on statistics, should be justified, and the associated uncertainties or confidence levels should be reported. I thus recommend this study to be accepted after some revisions.

General comments This coastal region is particular, as it is located in the mountains. A spatio-temporal variability in surface mass balance could be then expected, related to wind drift, but this aspect is not mentioned.

Introduction This introduction is quite difficult to follow, and not enough explicit about the scientific questions which are addressed in the manuscript. I provide hereafter suggestions to make it more understandable (specific comments). There is limited information and citations of existing literature related to climate variability in the region of the Ellsworth Mountains, as well as for the state of the art for surface mass balance and stable water isotopes. I expect the surface mass balance and winds in the region are to be highly variable both in space and time. My recommendation is to further describe the state of the knowledge for these aspects (including knowledge gaps), referring to literature specific or relevant for this region, and to explicitly frame the question of potential deposition and post-deposition effects imprinted in firn core records, as evidenced in several other regions.

Methods - For the ice core chronology, please make sure anyone can reproduce it thanks to a more detailed description. I am not sure that this is possible from the information given in the paper. For instance, Figure 2 suggests that some peaks of MSA and nssSO₄ were not counted, contrary to dD peaks. Why? What makes water stable isotopes more reliable in this site? Other studies of coastal locations such as Goursaud et al. (2018b) have shown that water stable isotopes are less reliable than chemical signals for dating firn cores in Adelie Land. It would be also relevant to have an objective assessment of the age scale uncertainty due to ambiguity in peak detection. - Why did you use a Mann-Kendall test? From what I know, it was used to detect inflections (Turner et al., Nature, 2016). - When reporting the outcomes of linear correlation analyses, can you please systematically provide the correlation coefficient?

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Also, no need to give the exact p-value. It is sufficient to precise if it is <0.05 or <0.001 . - I do not fully understand the relevance of a composite signal based on individual, non-correlated signals. Can you please justify the robustness, or point the limit of such an approach? How confident are you that this reflects a common climate signal, rather than noise (and thus influenced by the number of underlying records)? Could you maybe consider principal component rather than mean to extract a signal which would explain the maximum variance, thus potentially more representative of a regional signal? - I do not think that you should make two composites based on standardised and non-standardized data. You should first try to explain why the individual signals are not correlated to decide then to consider standardised or no standardised data. If it turns out that the spatial variability results from deposition and post-deposition effects, it will be more consistent to use standardised data. Otherwise, non-standardised data should be used. Note that in Stenni et al. (2017), we used standardised data. If your initial idea differs, please specify as it is not straightforward.

Results - Results of not significant linear relationships (slope and p-value) should not be given. - A discussion of potential noise should be added. For deposition, why do not you compare your reconstructed BMS with stack data and the climate model you cite in the discussion (Part 4.2.1, p11 I1)? For the effects of isotopic diffusion, you could apply a simple diffusion model (Johnsen et al., 2000; Jones et al., 2017), or at a least evaluate if there is a loss of seasonal dO18 amplitude along the core and report the corresponding results.

Discussion - I suggest to begin by discussing potential noises (see above), before discussing the potential common climatic signal. - I recommend to dedicate a paragraph to the assessment of the dO18-temperature relationship in this region and the comparison with other Antarctic regions. Also, I do not understand why you suggest to reconstruct regional temperature based on your dO18 composite record whereas $r^2 = 0.21$. Why explains the 80% variance left? I note a contradiction between p9 I15 and your conclusion, p11 I32. - The discussion of negative slopes from the reconstructed

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SMB appears surprising given the fact that slopes are in fact very close to 0.

Specific comments Introduction P2 I12: Can you please add a transition word so we understand that you move to Peninsula, eg “In AP...”. P2 I15: The sentence is difficult to read, please reword it to: “Factors affecting mechanisms forcing” P2 I23: The references “Thompson and Wallace 2000; Thompson and Solomon, 2002; Gillet et al., 2006” are repeated in line 25. Please remove the second call to these references. P2 I28: I checked Turner et al., Intern. J of Climatology, 2013, surprised by your assertion that the positive SAM values from the 50s are partly attributed to “local confined sea-ice loss”. However, I found only a suggestion that the decrease in sea-ice extension (SIE) in AP could be linked to SAM (and not the opposite). Besides, Turner et al, Nature, 2016 shows that the changes in circulation lead to a decrease in the sea ice concentration in AP (see Fig 3). Only for the two last decades, for which a shift in circulation occurred (shifting warming in AP to cooling), a positive retroaction has been noted between the increase in SIE and changes in circulation. I suggest a careful introduction to the links between regional sea ice changes and circulation changes. P2 I34: ENSO is a mode that has a specific pseudo-periodic behaviour, I thus suppose you meant “mode” instead of “cycle”. P2 I34: Please add a comma after “scales”. P2 I15 – P3 I3: You refer to near-surface temperature positive trends of the last decades in WAIS (1st paragraph) and AP (2nd paragraph) since the 1950s, and discuss the state-of-the-art of understanding potential causes. If you want to present your drilling site as a crossing area between WAIS, EAIS and AP, you should also write a third paragraph browsing a short state-of-the-art of recent climate variability of EAIS, citing for instance Stenni et al. (2017) (last 2k temperature reconstruction of Antarctica), and emphasizing the challenge to detect any trend. Note that even a weak cooling trend is not seen in some coastal areas such as the Adelie Land (Goursaud et al., 2017 and references herein). P3 I4: Your transition is unclear. You describe changes in trends in AP temperature associated with a cooling for the first part of the 21st century, the WAIS warming amplitude being part of the natural multi-decadal variability of last 2000 years (308 in the Thomas’s study, and 2000 in the Steig’s one), but also the weak cooling in the EAIS.

I think that you rather give the limits of the comprehension of the very last decades climate variability at the end of the last three paragraphs, ie tackle with the WAIS warming in your paragraph from P2 I29. But please when giving this information, stress that it is the rapidity of this warming which is unprecedented. Then, introducing the need for extended observations and monitoring in Antarctica, just give a short sentence to resume the limits of our comprehension of the recent climatic variability, whatever the considered Antarctic region. P3 I11: There is a lack of data not only for the interior of Antarctica, but also for coastal areas, for instance in the Indian Ocean coastal region and in Adelie Land. You can refer to Jones et al. (2016) for the temperature, and the updated water stable isotope Antarctic database(Goursaud et al., 2018b). P3 I3: From “For the region”, please add a new paragraph, where you focus on this crossing point between the three main regions of Antarctica. In this paragraph, please be more explicit on the questions that you address in this study. I also recommend to report the fundamental literature related to the climate of Ellsworth region. P4 I1: change “WAIS in the south” to “southern WAIS”.

2 Data and Methodology 2.1 P4 I9: I cannot find neither in the paragraph nor in the Table 1 the ray in which the drilling sites are located. Could you please add it here? I would be also very interested in knowing here which sites are in crests or peaks, as surface mass balance should differ between these sites (Agosta et al., 2018), as well as the isotopic signature, at least at the second order. p4 I15: Please add a comma after “(2012)”. P4 I19, P4 I25: Please add a space between numbers and units throughout the manuscript. P24 I4: How can you quantify that the precision is better than a specific threshold. Please give a precise uncertainty. P5 I9: I understand that you compute a local meteoric water line based on ice core data, and especially only 6 points, which can be affected by deposition and post-deposition effects.

2.2 P5 I13: which ratio did you use to compute nssSO₄? Did you use summer or winter ratio (Jourdain and Legrand, 2002)? P5 I15: Please provide references for the seasonality of the aerosol signals preferentially for your region. For instance, in Adelie

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Land, there is no seasonal cycle in ssNa, so please check. You can also cite recent studies which apply such an annual layer counting to date firn ice cores and discuss the uncertainties (Vega et al., 2016; Caiazzo et al., 2016). P5 I18: How did you estimate your uncertainty? I would have liked to see on the figures 1 and 2 what constitutes this age scale uncertainty (e.g. peaks that you did not count). P5 I17: I do not fully understand the rationale behind the method used to match GUPA, DOT, SCH-1 and BAL-1 dating to SCH-2: are the isotope records highly correlated, justifying such a method? (what is the implicit hypothesis and can you test it)? P5 I23: Why did you use non-standardized data for composites? Please take into consideration the general comments. P5 I24: Why did you use the Mann-Kendall tests? It is usually used to detect inflections. Is it the case here? Please justify.

2.3 P6 I4: please add a line break after “isotopes and accumulation.” to split observations from climatic modes.

Results 3.1 P6 I24: what initial point (coordinates and height) did you indicate for back-trajectory simulations? Which drilling site does it correspond to? P6 I29 and P7 I1: change “was” to “is”. P7 I1: you choose in the manuscript the convention $m\ we\ a-1$ for accumulation, so please change “m/s” to “ $m\ s^{-1}$ ”. Also please change “was” to “is”.

3.2 P7 I8: “the longest record” P7 I11: what kind of extrapolation did you apply? P7 I11: please replace “furthermore” by another word as repeated from previous sentence. Could you give the uncertainties associated with your dating for each firn core at the end of this paragraph? It is actually results and not methods.

3.3 p7 I16: As you give the dO_{18} mean range, I do not think it is necessary to also give dD . I would also rather go for mean and standard deviation, which give more information about the variability. P7 I21: These values are not so low. If you refer to Figure 6 in Goursaud et al. (2018a), that shows the spatial variability of d , you will notice that d can reach minimum values of 0 to 4 per mille in Ross sea, and Amery sector, but also close in the Ellsworth sector ! P7 I25: Please change into the brackets to “range values

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from ... to...". I am not convinced by the method used to estimate the LMWL obtained based on ice core data. P7 I27 to 30: there is a low spatial variability of your mean reconstructed SMB, and thus you could just give the mean and standard deviation of the SMB averages, instead of describing the core with the highest and lowest SMB. Details are then given in Table 1. Then, your next message is substantial as you show that particular high (low) values are not concomitant between the firn cores.

Discussion 4.1.1 p8 I5 and p8 I8: I do not see the point to report minimum and maximum values. What is your message from such information? You could first discuss the differences in the mean dO18 values from one ice core to another noted in the results, and consistent as you write with continental effect. You could then discuss the lack of similarities in inter-annual variabilities (remaining results?), and confirming here by testing the correlation between the different dO18 over the overlapping time period 1998-2013. Here, you could write that 2002 is a maximum value in all firn core data. There is some ambiguity on the structure of the manuscript, where some key results are reported in the discussion, and not in the section on results. P8 I14 to I16: relationships which are not significant, are useless. Please remove it. The two positive trends are results and not discussed here, so it should go to the "results" part. P8 I17: I do not find that dO18 firn cores data are well correlated. Some are not correlated at all, eg PASO-1 with DOTT-1 etc. . . ., and the highest r is 0.658, ie r^2 of 0.433, so I really would not go for a regional signal by a simple average of the time series. In the assessment of potential trends, the discussion should be explicit that only two of your firn cores present such trends (P8 I21). P8 I22: Why did you prefer Sen slope rather than linear simulations? P8 I23: You cannot conclude an increase in near-surface temperature from a positive d18O trend (which is I think not robust, see what I wrote before), whereas you did not test the multi-year dO18-T relationship in your region. P8 I23 to I32: You discuss here the inter-annual variability in temperature. It should be in another paragraph dedicated to it, and using the results of dO18-temperature relationship you find in your data, and that should be cited in the results. P8 I33 – P9 I6: Is it Sen slopes of from linear regressions? If linear, please give the correlation coefficients for each

simulated linear relationship. I am very surprised for such different trends. Either the relationships are very weak or trends can be neglected, or other effects than change in moisture origin might act here to explain the differences, as your drilling sites are relatively close to each other. Once more, I do not find it robust at all to make a mean for time series showing opposite trends, and where only two firn cores are correlated (DOTT-1 and SCH1).

4.1/2 This paragraph should come before discussing a potential temperature reconstruction.

P9 I12: You could compare near-surface temperature from the wx7 and Arigony AWS with ERA-interim. If the correlation is strong enough, you could test the linear relationship between dO18 and the near-surface temperature over longer period than for observations, thus using ERA-interim temperature. You could also test the relationship with each of your firn core. Have you considered that local processes could affect the signal, and could be more important at some locations? P9 I15-16: You cannot use dO18 to reconstruct the temperature: the linear relationship is much too weak (I10), and this sentence is not consistent with I11: "However, a proper inference of near-surface temperature from dO18 values of precipitation in the UG region is not yet possible".

4.1.3 This part is very interesting and show the potential of the isotopic signal to provide information about the Weddell sea ice extent. It would be valuable to describe these results with due care, as they are based on composite analyses from individual series that are weakly correlated, challenging the confidence in a strong common climate signal. What is the likelihood of obtaining a link with sea ice extent using pure noise with a given frequency range? p10 I5: why do you suggest this correlation to be an artefact? It is very weak ($r = 0.315$, ie $r^2 = 0.0992$!).

4.2.1 p10 I22: you do not need to give the precise value of p-values. Just write $p < 0.001$ or $p < 0.05$. Change throughout the manuscript. P10 I24: Remove the insignificant relationship for firn core DOTT-1. P10 I22-26: Deposition effects related to wind should

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be mentioned at the beginning of your study, as it can significantly modify your signal. P11 I2: Precise how far are the stake measurements from the firn cores, and the grid resolution of the model. What are the covered periods? P11 I3: Can you also precise the location of Patriot Hills and the shortest distance between the closest ITASE ice core and your firn cores. P11 I6: Could you compare the inter-annual from your firn cores with the stake measurements and model outputs? We have shown in our paper under review, the added value of such time series 1) to make the dating more robust, and 2) extract a regional signal.

4.2.2 P11 I22: you suggest that ERA-interim fails to capture the effects of orography. You can show it by giving the surface height of the grid covering the drilling sites, while these ones differs from each other. You suggest also at the very end that your data could be affected by post-deposition effects. But couldn't you test it, for instance by looking at the evolution of the seasonal amplitude along the cores (see again Goursaud et al., 2018b), or applying a proper diffusion model as done in Jones et al., J of Geoph. Research., 2017.

Conclusions P12 I10: Remove the slope for non-standardized data.

Can you make the data available, either in supplementary material, on any public access depositary?

Figures and tables Figure 1b: Names of the drilling sites are hardly readable (except GUPA-1 and DOTT-1). Figure 2: What does "smooth 2p" correspond to? It is not specified in the legend or caption. Specify that depth is in water equivalent in unit (as well as for the following figures). Figure 3: Why did you use nssSO₄/ssNa? What does it correspond to? This is not explained in the paper. How can you justify that you did not count peaks ~ 1.2 and ~ 5.2 m w.e.? Figure 4: Monthly mean from Arigony AWS is hardly readable. Figure 5: I suggest to move these figures to supplementary material. Figure 6: for all figures displaying annual-scale data, can you draw it with cityscape vectors? Once more, slope is almost equal 0. Thus, the discussion of negative trends is not a ro-

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bust finding. Figure 7: Please remove the composite based on non-standardized data or standardised data, and use cityscape vectors. Table 2: Periods for reconstruction are given in Table 1. Please move this table to supplementary material.

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