Review of manuscript cp-2020-19: “High-frequency climate oscillations in the Holocene from a coastal-dome ice core in east central Greenland” by Abigail Hughes et al.

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Hughes et al. present a new oxygen isotope temperature proxy data set covering the Holocene from an ice core drilled on the Renland Peninsula ice cap in eastern coastal Greenland. The isotope record is obtained on a high nominal resolution from Continuous Flow Analysis coupled to Cavity Ring-Down Spectrometers. Originating from this high-resolution data set, the authors use spectral analyses to estimate diffusion lengths and to investigate the extent to which the isotope (climate) signal of certain frequencies and frequency bands survives the diffusional smoothing throughout the Holocene. By means of deconvolution, they reconstruct the seasonal summer and winter isotopic time series. Subsequently, the authors discuss the relationship of the isotope records' decadal variability with regional sea ice and ocean circulation changes as well as potential causes of the observed decrease in winter isotope values over the last 2.6 ka.

The paper presents a new proxy data set at high resolution and discusses its relevance to assess regional climate information. This is an important contribution since the interpretation of isotopic data from coastal ice cores is an up-to-date topic and because the investigated site has only been studied previously at relatively low temporal resolution. The paper thus fits well within the scope of Climate of the Past. I would like to congratulate the authors on their elaborate analysis of the isotope data; the methods are sound, the paper is well written and structured and the figures overall adequately present and support the findings of the paper. However, my major concern with this work pertains to the interpretation of the results in terms of “high-frequency climate oscillations” and regional oceanic conditions (sea ice, ocean circulation). In the current state, I see only relatively weak evidence in the presented data supporting the claims made in title and abstract. Thus, I would recommend publication of the paper after a major revision of these general issues, which I outline below. In addition, I give a list of specific as well as technical comments you might want to address.

General comments

A first major point is that the main conclusions the authors give split up in two seemingly separate issues, i.e. the relationship of the decadal variability with sea ice conditions and/or ocean circulation, and the explanation of a decreasing winter trend by increasing sea ice. These two interpretations are presented rather disconnected throughout the manuscript, partly due to the combined structure of results and discussion, but also in the final overall conclusions section. As a result, the reader is somewhat left puzzling what the overall essence of this paper is, how the two presented main results connect with each other and what we in general learn about interpreting isotope records from coastal regions.

This problem is also reinforced by the presented evidence to support the interpretation, which is rather weak in my opinion.

The title of the paper alludes to observed climate oscillations with a high frequency. Firstly, I would not denote decadal variations as being high frequency, but more importantly, the authors present a rather simplistic and too deterministic view on climate variability. Strictly speaking, an oscillation is defined as a periodic variation in time with a fixed frequency, or in a coupled system, a superposition of several frequency modes. For climate variability, this deterministic view is not necessarily the case. The Atlantic Multidecadal Oscillation might indeed appear to be a quasioscillatory phenomenen, but very likely it does not exhibit a strict 20-year cyclicity, as suggested in the paper (P3 L45). By contrast, the spectrum of the NAO is essentially white, so these are rather random variations than "subdecadal climate [or] multi-year cycles" (P3 L46-48). This also applies to the "Bond cycles". There seems to be no single deterministic cause (Wanner and Bütikofer, 2008), the 1500-yr periodicity is likely an artifact (Obrochta et al., 2012), and the variations could be simply compatible with internal climate background variability (see also the discussion of the statistics of D-O events as their glacial counterparts; e.g. Lohmann and Ditlevsen (2018)). I would suggest that the authors critically revise the text passages where these phenomena are discussed; especially focussing on toning down statements of Bond events having a 1500-yr periodicity (P2 L35, P11 L208, P17 L318) an rewriting the respective introductory paragraph (P2 L35 – P3 L42). Here, maybe use AMOC as a starting point and then develop its influence on local climate and sea ice?

The comparison of the 15–20 year variability with the Bond curve (Figs. 7 and 8) is also critical in this regard. While the title of the paper suggests that you observe oscillations in the isotopic time series, what the authors actually show in the figures here is the time evolution of the isotopic variability (if I understand correctly, either the average spectral power or the variance; see specific comments) in the frequency bands. While this is certainly a very interesting analysis, the difference to "normal" variations in the time series should be made clearer to the reader in the text and the title of the paper. Moreover, a discussion of a possible mechanism would be welcome: what could link temporal changes in drift ice to changes in isotopic variability, i.e. non-stationarity, within the 15–20 year frequency band? Alternatively, what is the significance of the curves in Figs. 7 and 8? Are the variations maybe just within the curve's uncertainty range? An error shading including spectral estimation error and diffusion-correction uncertainty would be helpful. From Fig. 7, I would say that visually the 15–20 year band exhibits correlation with the 20–30 year band and to a lesser extent also with the 7–15 year band; can you comment on this? This could either arise from correlation in the spectral uncertainty, or the speculated link to climate is not confined to the 15–20 year band. Stacks of Greenland ice-core records show stronger variability compared to the background around the 20-year period in general (Chylek et al., 2011), so a comparison with the full RECAP isotope spectrum (either a diffusion-corrected or, if this is tedious, the raw one) would help to better place the new spectral data into context with existing data from the region.

Finally, I have some concerns regarding the interpretation of the seasonal isotope data. In section 3.2.3 the authors conclude that the decreasing winter trend in isotope data could be explained by increasing sea ice which correlates with decreasing winter temperatures. In this regard, the authors also cite the paper by Noone and Simmonds (2004). However, as far as I understand this study, the effect of changing sea ice on isotopic composition is rather through controlling the influence of local oceanic moisture in comparison to long-range transport, instead of a direct influence on local temperature. It would thus be worth to discuss the influence of sea ice on isotopic composition in more detail here.

Furthermore, the authors also state as a main conclusion that this finding "is a valuable demonstration of the additional regional climate information contained in coastal ice cores" (P18 LL328-329). However, I would be more convinced if you could explicitly show that the RECAP spectral (see above) and seasonal signals are clearly distinct from other Greenland ice cores. I don't see this being convincingly presented. The GRIP seasonal data basically shows the same features (stable summer, decreasing winter; cf. Appendix A) – isn't the fact that the resulting increase in GRIP seasonal amplitude appears "muted" compared to Renland (Fig. A1) simply explained by the difference in axis scaling ($\sim$ 1.5‰ axis range for RECAP compared to $\sim$ 3‰ for GRIP)?

Specific comments

- P1 L7: "The strength of the interannual frequency band decays rapidly". I suggest to remove this result, since it is a direct consequence of firn diffusion and therefore a trivial and expected result for almost any ice core, which is not relevant for the abstract.
- P1 LL8-9: "Comparison to other North Atlantic proxy records suggests": As far as I follow the paper, the results concerning the Renland 15–20 yr variability are compared to only one other proxy record, which is the VM28-14 record of Bond et al. (2001), so this statement should be adjusted.
- P1 L17: "Greenland ice core records are valuable for determining a more comprehensive picture"; more comprehensive compared to what or which other records? This is not clear from the context provided.
- P1 L20: "[...] were used in analysis of the Renland ice core". This is a bit confusing since there is also the old Renland ice core which was already drilled in 1988. If I am not mistaken you present and refer here to the new RECAP ice core. Please clarify this throughout the paper.
- P2 Fig. 1: Please introduce the scope of the figure in a first sentence, e.g. "Map of the study region", and add to the caption that the red circle denotes the RECAP (?) drill site, blue circles other Greenlandic ice core sites, and the green circle the location of a marine sediment drift ice proxy record.
- P2 LL28-29: The Noone and Simmonds (2004) paper explicitly investigates the influence of sea-ice cover on western Antarctic ice cores; the statement here would suggest to a reader unfamiliar with the topic that it instead covers the link between coastal Greenland climate and ocean conditions, which is not the case. Please adjust the statement, stating that a similar influence of sea ice as observed in Antarctica could be expected for coastal Greenland cores, or provide a directly relevant reference. This similarly applies to the same reference in Sect. 3.2.3 (P17 L301).
- P1 L21 – P2 L34: The flow of information is rather incoherent in this paragraph. Please consider rewriting it such that you start to introduce the new RECAP core and then describe the peninsula, the ice cap and the local climate.
- P3 LL69-70: "due to a small amount of mixing introduced in the system". Please explain the CFA mixing effect shortly here and either state the amount or provide a reference.
- P4 L79 – P5 L86: As I understand it, the main reason for the loss in high-frequency variability is thus in both cases diffusion (as expected), but amplified by two distinct reasons. Please restructure the paragraph accordingly to highlight this and mention diffusion as the cause in the first place, and then elaborate the two reasons for the strong diffusion within the LGM and the basal ice.
- P5 LL90-91: "along concentration, temperature, and vapor-pressure gradients"; I would argue that concentration and vapor pressure are directly linked, so stating both is redundant. Just mention concentration, as it is more intuitive.
- P5 L104: Please provide a reference for the CFA system mixing effect, if not done before (please see respective comment above).
- P5 LL105-106: "with a 100-year time step"; at first reading, this can be misunderstood. I understand you mean that the step size between the overlapping windows is 100 years, but as it is written it might be confused with the temporal resolution; please clarify. Please also underline that you produce a spectrum for each window. Additionally, it is more common to refer to this quantity as the power spectral density (PSD); please change this throughout the manuscript.

– P5 L111 – P6 L118: Why is the linear regression in log space only used to assess the uncertainty of the diffusion length but not to estimate the diffusion length value itself? Should this not maybe provide a better fit than a nonlinear model? Or do you assume any other shape for $P_0(f)$ than white noise for the fit? What about the measurement noise – is it subtracted before? You do talk about this some point later in the manuscript, but it should be mentioned here already.

– P6 Eq. 3: I am not sure if I understand your uncertainty estimation correctly. From the slope m of the regression of $\ln(P)$ against f^2, one obtains the diffusion length as $\sigma = \sqrt{m}/(2\pi)$. If the slope estimate has some uncertainty Δm, then the uncertainty of the diffusion length should be $\Delta\sigma \sim \frac{\partial\sigma}{\partial m}\Delta m = \frac{1}{2\pi}\frac{1}{2\sqrt{m}}\Delta m$; so the exponent in Eq. (3) seems to be wrong ($-\frac{1}{2}$ instead of $\frac{1}{2}$), and isn't the slope uncertainty missing in there as well?

– P6 L124-130: Please clarify; it is clear that the diffusion length in depth should decrease due to thinning, but for the sake of clarity it could be worth to mention that, to a first approximation, the firn diffusion length should stay constant in the time domain after pore close-off, so an observed increase in diffusion length in time must have a different cause, i.e. ice diffusion etc.

– P6 L134: There is not really much information regarding the two fitting methods to be found in the appendix or the respective figure caption.

– P6 L139: What about the contribution of non-climatic noise (e.g. from stratigraphic noise; Münch and Laepple (2018)) to the spectra?

– P6 L140: I would not call it amplitude at all, since this term refers to truly oscillatory behaviour. Just use "strength" or "strength of the variations". Also it is not clear to me which exact quantity you investigate: do you use the square root of the power at the respective frequencies or averaged over the frequency band? Or do you integrate (for the frequency bands) the power spectrum to obtain the average variance (and then standard deviation from the square root of it) within each band? Please clarify here and also when discussing the results in Sec. 3.1.

– P6 LL141-142: "individual frequencies are normalized", "each frequency band is normalized"; better write that not the frequencies are normalized but the PSD/variance at the frequency/within the frequency band.

– P7 Fig. 3: Please introduce the scope of the figure in a first sentence, e.g. "Isotope power spectra and diffusion length estimation." Also, can you comment on the rather unusual shape (sharp increase, then flat) of the estimated spectra in panel (a) for large periods (> 3 m); which part of it can be trusted and which might be an artifact of the estimation method? In addition, it might be also worth to mention that the strong increase in PSD of the diffusion-corrected spectrum for large frequencies (small periods; violet curve) arises from blowing up of the measurement/CFA noise by the diffusion correction.

– P8 L154-156 and Fig. 4: I wonder, given the sub-seasonal variability of the raw (diffused) signal, if the smoothness of the deconvolved signal is an artifact of the inversion method and not real? In other words, would we really expect the original (pre-diffusion) seasonal δ^{18}O signal to be so smooth? Have you tried to diffuse again your deconvolved signal and compare it to the original?

– P8 L164: "and a constant amplitude (4‰)"; why do you choose the amplitude about twice as large as observed from the seasonal isotope data (Fig. 9e)?

– P8 L165: One cannot really see the precipitation bias clearly on Fig. B3; better refer to Fig. 5 here and reference Fig. B3 in the first sentence. Also please consider to list the input information in a more coherent way: Start with the MAR temperature data, then mention precipitation bias in MAR and then your assumed accumulation scenarios.

- P10 Fig. 6: Please introduce the scope of the figure in a first sentence. Additionally, dissipation in a physical sense refers to the energy loss by friction; please rephrase, e.g. “the annual signal strength decreases”/“the annual signal diffuses”.
- P10 L185 and Fig. 6 and 7: Why does the normalized amplitude has a unit (‰)?
- P10 LL186-187: Remove the sentence “Each frequency band is...”, since this information is already given in the Methods section and the figure caption.
- P12 L219: What kind of “similar 20-year signal”? Between the time series (correlation of e.g. 20-yr averages) or in spectral properties?
- P13 L227: “of this relationship”; this is unclear: of which relationship? How does this sentence relate to the statement from the Knight et al. paper directly before?
- P13 L236: Please clarify what you mean with non-stationarity here. You just presented that the winter time series shows a trend, so is non-stationary in the the sense that its mean value is decreasing, but you say that the summer value does not show any clear trend, so it would be stationary. Or do you refer in both cases to other statistical quantities which show time dependence, e.g. the time series variance?
- P13 Sect. 3.2.1: This is a well-written section that convincingly argues that accumulation seasonality and changes in melt layers very likely are not the cause for the observed trends in isotope seasonality. The only thing which comes to my mind is, however, the possibility of changes in the seasonality in firn temperature to maybe affect the isotope seasonality via seasonally varying diffusion lengths (Simonsen et al., 2011). Could you provide arguments to constrain this possibility?
- P13 LL253-254: How can I see the pre-diffusion amplitude in Fig. 10 to assess the under-correction? Is it identical to the constant accumulation case diffusion-corrected value? Or does this case also lead to an amplitude under-correction?
- P14 Fig. 9: Please introduce the scope of the figure, e.g. “RECAP annual and seasonal δ^{18}O data.”
- P15 Fig. 10: Please clarify: “Comparison of diffusion-corrected seasonality...”.
- P15 L255: I cannot see an 18% difference in Fig. 10; when I compare the green dots to the black line, there is at most a difference of maybe 7%?
- P16 LL274-275: “and the snow layers are so thick at Renland...”; this is repitition from above, you might consider removing this part.
- P17 LL291-292: It might be worth to elaborate a bit more on this conclusion. Do you mean that the insolation cannot explain the seasonal isotope changes since the summer insolation change is too weak or because there is no winter change, or both? It could also be useful to underline the importance of having seasonal information available due to the accumulation bias in the annual data. How do your results connect to the millennial-scale trends seen over the Holocene in other Greenland ice cores (Vinther et al., 2009)?
- P17 L307: Someone not familiar with the sea-ice edge distribution might wonder whether the area is actually ice free in summer at present?
- P17 L308: The first part of this paragraph seems to be a repetition from the previous one; please consider to restructure the two paragraphs.
- P18 L322 and L324: Given the speculative nature of your conclusions, I would rather nuance this and instead write “...and diffusion correction reveals a decreasing trend...” as well as “We instead suggest that the winter trend is likely...”.

– P20 Fig. B2: I must admit that I do not really understand the difference in the fitting approaches from the figure caption or the figure itself. Please explain this in more detail.

Technical comments

– P1 L1 and title: Please capitalize and hyphenate consistently "East-Central Greenland" throughout the manuscript.

– P1 L4: replace "and the annual..." with "while the annual..."

– P1 L9: Please follow the house standard and use a long (em-) dash for range of numbers, so 15–20 instead of 15-20 (here and throughout the manuscript).

– P1 L16: Please hyphenate phrases such as "Ice-core records" throughout the manuscript; I will mention only some additional instances in the following.

– P1 18-19: Please change to "Cavity Ring-Down Spectroscopy".

– P1 L19: Please hyphenate "high-frequency signals" (throughout the text); see previous comment.

– P1 L21: Please change to "Renland Peninsula".

– P2 Fig. 1: Please change to "east coast".

– P3 L41: Please change to "northward".

– P3 LL49-50: Please change to "tree-ring data" and "central and western Greenland ice cores".

– P3 LL59-60 and throughout the manuscript: I very much appreciate that you introduce the correct terminology "water isotopologues" here. However, then please be consistent and avoid phrases involving "water isotope"; instead, simply use "isotope" (e.g. isotope signal, isotope record) or use "water isotopologue" or "isotopic composition", where appropriate.

– P3 L62 and Eq. (1): Change "SMOW" to "VSMOW".

– P3 L65: Equations are considered to be a part of the sentence, so please start this sentence with lower case "such" (also for Eqs. (2–4)).

– P3 L67: Change cm/min to cm min^{-1}.

– P3 L68: For the sake of clarity, please rephrase the sentence to "using two Cavity Ring-Down Spectrometers running in parallel (L2140-i and L2130-i)".

– P4 L74 "0–4035 yr bp": I guess you mean years before present (1950) here; please define this at the first instance and use "yr BP".

– P4 L79: Please change to "exibits a much higher effective resolution".

– P5 L97 "804.3 kg/m^3": there is no need to use such a precise number here (since it is an estimated value); please write "$\sim$ 804 kg m^{-3}".

– P5 L102-103: I think one most relevant reference to introduce the diffusion length would be sufficient (e.g. the first paper which introduced the concept in the context of firn diffusion).

– P5 L105: Change to "in the depth domain".

– P5 L106: Change "cycles/m" to "m^{-1}".

– P5 L108: Change to "To estimate the diffusion length" or "To estimate diffusion lengths".

- P5 Eq. (2) and P6 L115: Use upright font for mathematical functions such as “exp” and “ln”.
- P6 L115: Change to “of the data”.
- P6 L124: Change to “m yr^{-1}”.
- P8 LL159-160: For the sake of clarity, please insert “to the resulting summer and winter time series” after “is applied”.
- P9 L177: Change to “diffusion-correct”.
- P10 L181: For the sake of clarity, please insert “with time” after “decays rapidly”.
- P10 L182: Change to “diffusion-corrected”.
- P10 L186: For what is the Jones et al. reference needed here?
- P11 Fig. 7 and P12 Fig. 8: The lines of the two fits (solid purple and solid dashed) are indistinguishable; please use another color for highlighting.
- P13 L243: Change to “diffusion-correct”.
- P13 L249 and P15 L256: Change to “diffusion-corrected”.
- P15 L259 “in which we are aware”: I am not familiar with this phrase; do you mean “which we are aware of”?
- P16 Fig. 11 caption: Please clarify: “Changes in Holocene insolation...”
- P16 L279: Change “of top of atmosphere” to “of the top-of-the-atmosphere”.
- P17 L305: Change Fall->fall and Spring->spring.
- P17 L316 “The Renland ice core”: Please clarify “The RECAP ice core”.
- P19 Figure A1 caption: Please rephrase: “Comparison of Renland and GRIP seasonal data. GRIP experiences...”.
- P20 Figure B1 caption: Please clarify: “Mean annual layer thickness in the RECAP core...”.
- P20 Figure B2 caption, second line: “to determine a diffusion”: Do you mean “to determine the diffusion length”?

References

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