Response to Referee #2

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.

We thank the reviewer for taking the time to provide a thorough analysis with constructive criticism. Below we individually address each comment, outlining changes we have made to improve the manuscript. All line numbers are in reference to the original manuscript.

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.

While we see the value in having a separate results and discussion section, we believe that this would likely make the structure of the paper more disconnected and difficult to follow - jumping between results and discussion of high-frequency variability over 8 ka and variability of seasonality over 2.6 ka may be more confusing and flow poorly. However, in an effort to better link the two sections, we have revised the conclusions section to first reintroduce the core and record, then briefly discuss conclusions from each section, and tying them together at the end:

"The RECAP ice core from coastal East-Central Greenland contains a high-resolution water isotope record of the Holocene, obtained using continuous flow analysis (Jones et al., 2017b). The coastal proximity of the Renland ice cap makes the isotope record subject to influence from sea surface conditions (Holme et al., 2019). The record preserves annual variability for the last 2.6 ka, interannual variability for the last 5.5 ka, and decadal variability for the last 8 ka. We perform a diffusion correction calculation on the annual signal, based upon the diffusion length fitting routine of (Jones et al., 2017a).

The diffusion correction calculation for annual variability requires testing by a firn model to determine if biases in seasonality of accumulation can alter the patterns observed in summer and winter extrema. Since diffusion corrections assume constant annual accumulation rates, diffusion correction can over- or under-estimate the summer and winter signals. We utilize the Community Firn Model (CFM) (Stevens et al., 2020) to test an extreme case of shifts in seasonality of accumulation, from constant accumulation to summer-fall weighted accumulation, as given by MAR (Fettweis et al., 2017). We cannot rule out that centennial variations in summer and winter isotope values could be the result of seasonality of accumulation and the associated diffusional effects. However, the millennial-scale trend is robust: The summer extrema are relatively steady, and the winter extrema are steadily declining with a greater magnitude of change than can be accounted for by changes in seasonality of accumulation. We suggest that increasing sea ice driven by decreasing total annual insolation over the last 3 ka could cause the winter decline; however, other regional climate variables such as atmospheric and oceanic circulation may also exert an influence on the seasonal isotope signal. Future isotope-enabled modeling studies are needed to constrain how these factors are reflected in the RECAP δ^{18}O signal..

The interannual and decadal signals are variable at millennial and centennial timescales, with a somewhat constant trend over the last 8 ka. This is to be expected, as the background climate state of the Holocene is also stable. We note that variability centered on 20 years has millennial-scale variations that appear similar to Bond Events recorded in a North Atlantic sediment core off the west coast of Iceland (VM 28-14) (Bond et al, 2001). The relationship is significant, but with low correlation for the full 8 ka record. However, VM 28-14 has large dating uncertainty, which could lessen the correlation. This potential relationship may be of interest for future isotope-enabled modeling, as this sort of millennial variability is pervasive in the North Atlantic (Bond et al, 2001).

Future ice core studies at locations with the annual signal preserved will provide additional constraints on spatial and temporal variability in the annual water isotope signal. At this time, Renland is the only available Greenland ice core that has high-resolution sampling and high accumulation rates. These factors are necessary to rule out seasonality of accumulation effects on diffusion, allowing for an interpretation of the summer and winter patterns for the last 2.6 ka. Whether Renland is unique in its downward trend in winter values remains to be seen. We suggest careful planning for future studies of the annual signal: 1) If site accumulation rate is low, impurity data will be needed to constrain the seasonality of accumulation; and 2) Isotope-enabled modeling should be used once new records are obtained, which will improve our understanding of regional climate dynamics."

Additionally, we do agree that results and hypotheses should be more clearly separated, and as such we have added statements to make it more clear when we are transitioning from discussing results to hypotheses. For example, sections 3.2.1–3.2.2 are primarily results directly derived from our data (with the exception of part of the discussion of melt layers, in which the data that comes from Taranczewski et al., 2019 is clearly stated), and section 3.2.3 is primarily a discussion based on other studies and our hypotheses. To clarify this, we have added the following statement near the beginning of section 3.2.3:

“While it is difficult to determine the cause of this trend without isotope-enabled modeling, we can hypothesize how possible mechanisms involving regional climate variability might influence seasonality of the isotope signal at Renland.”

In section 3.1, we clarify the transition from results to discussion with the following statement: “...considering the common geographic region, we can hypothesize how potential mechanisms could link the two records.”

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 Btikofer, 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?

To address this, we have changed phrases such as ‘oscillation’ and ‘cycle’ to more general ‘variability’ or ‘signal’, where appropriate. (ie. L9, L36, L45, L46, L47, L48). We have also changed phrasing regarding Bond events from ‘1500-year cycle’ to ‘millenial-scale variability’, and changed the title of the paper to “High-frequency climate variability in the Holocene from a coastal-dome ice core in East-Central Greenland”. We have also revised the introductory paragraphs LL35–42 (and LL 43–51) as suggested:

“The modern instrumental record documents a number of influences on climate variability in the North Atlantic and Arctic, which may exert an influence on the local climate at Renland. The Atlantic Meridional Overturning Circulation (AMOC) controls heat transport to the Arctic and can have a substantial effect on Arctic climate over long timescales. Heat is supplied to the Arctic via the Norwegian Current, carrying warm Atlantic water to the Arctic (Polyakov et al., 2004); changes in heat supply and northward advection will influence atmosphere-ocean heat exchange, regional Arctic climate, and sea ice cover (Muilwijk et al., 2018). Heat distribution through AMOC controls sea surface temperature and drives the Atlantic Multidecadal Oscillation (AMO), which is observed in prior Greenland ice cores and influences sea ice cover in the Arctic (Chylek et al., 2011)with approximately 20-year variability. Subdecadal climate signals are also observed in the Arctic, such as the North Atlantic Oscillation (NAO), which is currently expressed as shifting sea-level pressure differences between the Subtropical High and Subpolar Low. This multi-year variability influences temperature, precipitation, sea ice distribution, and ocean circulation across the entire North Atlantic (Hurrell et al., 2009), and it is recorded in multiple proxy records including tree-ring data and central and western Greenland ice cores (Barlow1993, Appenzeller1998).

On longer timescales, changes in sea surface conditions are also reflected in North Atlantic sediment cores. Sediment cores from the North Atlantic show that millennial-scale climate variability occurred throughout the Holocene (Bond et al., 1997, Bond et al., 2001), referred to as Bond events. The mechanism forcing this variability is still under debate, but it is potentially driven by solar forcing (Bond et al., 2001) or internal climate dynamics such as interactions between the ocean and atmosphere (Wanner et al., 2015). The effects are most prominent in the Arctic, likely transmitted to lower latitudes through AMOC (Bond et al., 1997, Bond et al., 2001, DeMonocal et al., 2000)."

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.

Our method of spectral analysis provides information about the amplitude of a signal at a given frequency for each 300-year PSD window, which is different from an analysis of variance. We have clarified this issue by expanding the methods section, L140–143 which is revised to the following:

"Further analysis of the PSD can be used to determine the amplitude of climate signals on an interannual to decadal scale (Jones et al., 2018). While the raw isotope data is comprised of a continuum of climate variability at all frequencies, it is possible to separate out the strength (ie. the amplitude) of varying frequency bands within that continuum. The power (P_i) of individual frequencies from 1–20 years are identified for each 300-year PSD window, as well as the frequency bands of 3–7, 7–15, 15–20, and 20–30 years. For frequency bands, the average power is calculated by integrating across the power spectrum within each frequency band:

$$P_i = \frac{\int_{f_a}^{f_b} P(f)\mathrm{d}f}{f_b - f_a} \tag{1}$$

where f_a and f_b represent the lower and upper frequency limits, respectively. Because the frequencies are not evenly spaced, this method ensures that the average is not biased towards higher frequencies. The amplitude (ie. the strength) is calculated as the square root of P_i. In our analysis, the strengths of individual frequencies are normalized to the strength of the annual signal in the most recent window, and the strength of each frequency band is normalized to its most recent value. This produces a time series of the relative strength of isotopic variability for several individual frequencies (1, 3, 5, 7, 10, 20 years) and interannual to decadal frequency bands (3–7, 7–15, 15–20, and 20–30 years) (Figs. 6, 7)."

Additionally, we have added to the appendix a figure comparing the strength of decadal variability to the raw isotope data, demonstrating the difference between variations in raw data and in spectral analysis. We have also changed the title of the paper to "High-frequency climate variability in the Holocene from a coastal-dome ice core in East-Central Greenland"

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.

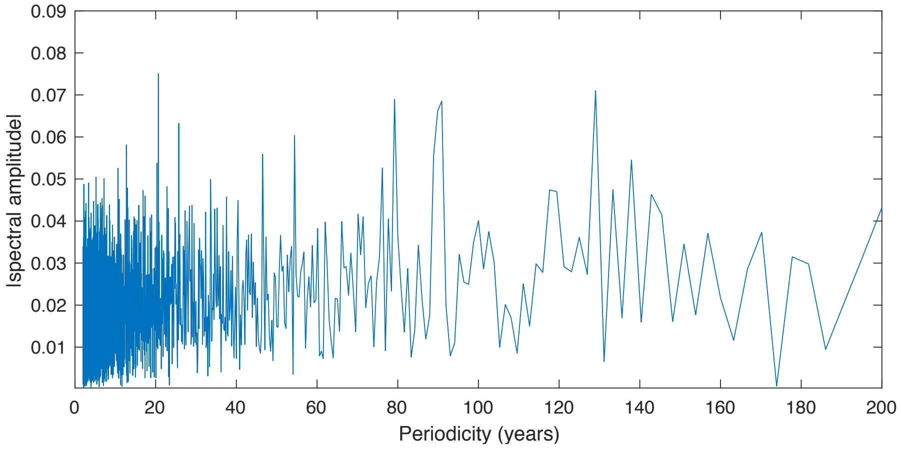
As shown in Fig. 7, the diffusion-correction has very little influence on the shape of the curve. We have added shading for the uncertainty on the diffusion correction in Figs. 7 and 8, showing that the error is very small. L188 is also revised to better emphasize this:

"The higher frequency bands are more diffused, and we find that the 3–7 and 7–15 year bands are not reliably corrected after 5.5 ka due to uncertainty in the diffusion length estimate (see Fig. 3). Fig.7 demonstrates that the 15–20 and 20–30 year bands are less influenced by diffusion, as there is a minimal difference between the strength of the raw and diffusion-corrected data, and the diffusion-correction does not significantly influence the shape of the curve over 8 ka."

We would expect that the visual similarities between the 15–20 and 20–30 year bands suggest that the link to climate is not strictly confined to the 15–20 year band, and rather is centered on this 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.

Here we have computed the FFT of the raw (we cannot diffusion-correct the full 8 ka record) RECAP δ^{18}O data through 8 ka, smoothed to a 1-year sampling frequency to remove high-frequency noise. We can see that there is a peak at a periodicity of approximately 20 years, similar to that observed in Chylek et al. (2011). This peak may be slightly weaker to that observed in Chylek et al. (2011), as the stack of ice cores used there would increase the signal-to-noise ratio. This figure has been added to the appendix.



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.

We have reframed section 3.2.3 as a look at regional climate variables, including sea ice. In this case, we separate the effects of sea ice on regional temperature and moisture source. We have replaced citations for Noone and Simmonds, 2004 with more appropriate citations for Greenland ice cores, such as Johnsen et al., 2001; Holme et al., 2019; and Klein and Welker, 2016 where appropriate. The following is the revised section 3.2.3:

“Indirect solar effects may influence other parts of the climate system, which then affect the local climate at Renland. A cooling trend in the North Atlantic is observed over the last 3 ka in records of glacial expansion, ice sheet growth, and increased drift ice (Miller et al., 2010), driven by a decrease in total annual insolation (Kaufman et al. 2009). In the RECAP core, a decrease in δ^{18}O is observed in winter, while the summer signal remains stable. While it is difficult to determine the cause of this trend without isotope-enabled modeling, we can hypothesize how possible mechanisms involving regional climate variability might influence seasonality of the isotope signal at Renland.

Since Renland is closer to the coast and open ocean than other inland ice cores, sea surface conditions could play a substantial role in Renland climatology (Holme et al., 2019). One possible factor is sea ice extent, which is both influenced by annual insolation (Muller et al. 2012) and influences total absorbed insolation at the surface due to albedo. A number of studies have documented an increase in sea ice cover in the North Atlantic and Fram Strait over the last 3 ka (Mller et al., 2012; Fisher et al., 2006; Jakobsson et al., 2010; Polyak et al., 2010). Sea ice extent has been previously studied through impurities in the RECAP core; iodine concentrations from the RECAP ice core suggest increasing sea ice over the last 3 ka (Corella et al., 2019; Saiz-Lopez et al., 2015), and bromine enrichment has been used to estimate sea ice conditions through 120 ka (Maffezzoli et al., 2019).

The formation of sea ice primarily occurs in winter, and increasing sea ice would be correlated with decreasing regional temperatures in winter and for portions of the shoulder seasons, depending on the timing of ice formation in fall and melt in late spring. A more open ocean regime at 2.6 ka, driven by higher total annual insolation, would keep winters warmer in coastal Greenland due to ocean heat contribution to the atmosphere (Screen and Simmonds, 2010). In recent centuries prior to the Industrial Revolution, lower total annual insolation and increased sea ice would dampen the moderating effect the open ocean has on coastal winter temperatures, resulting in colder winters. Increasing sea ice is therefore consistent with increasingly colder winters at Renland, whereas summers would largely be immune to sea ice response since nearby water bodies have little to no summer sea ice. This may explain the similarity between the winter δ^{18}O signal and total annual insolation at 71° N (Figs. 10c, 8c).

However, it is nearly certain that other regional climate variables have an influence on the δ^{18}O signal in the RECAP core. At this time we lack a comparison to seasonality at other locations in Greenland, which would help to determine the extent to which the trends observed in the RECAP record are due to local or regional influences. The RECAP core is unique in that it has both high sampling resolution (0.5 cm, whereas most other cores are over 2 cm), and high accumulation rate (45 cm yr^{-1} compared to 10–20 cm yr^{-1} inland), allowing for a much more accurate diffusion-correction of the seasonal isotope signal. At Renland, we may also observe the effects of atmospheric and oceanic circulation patterns and sea ice extent, which can control the influence of local oceanic moisture in comparison to long-range transport. This would alter the δ^{18}O signal through moisture source instead of a direct influence on local temperature (Johnsen et al, 2001; Klein and Welker, 2016). As we do not have records of isotope seasonality from inland Greenland, it is difficult to identify whether an effect such as this uniquely influences the RECAP isotope signal. These factors could be instead be further explored through additional modeling studies. "

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)?

After careful consideration, we have decided to remove the comparison to GRIP from the manuscript. Originally, we thought it was an interesting comparison, but the uncertainty on the GRIP record is too high for any meaningful comparison and it will only introduce confusion when interpreting the RECAP core as a unique coastal ice core record. Here, we outline the reasoning for the uncertainty on the GRIP record, and how we have modified our conclusions on the RECAP record after removing this comparison.

First, the GRIP isotope record has much lower sampling resolution (2.5 cm) than the RECAP record (0.5 cm). This is because the GRIP record was measured using discrete ice samples, prior to the advancement in technology that allowed for the continuous flow method used in analysis of the RECAP core. Additionally, GRIP has a much lower accumulation rate (23 cm yr^{-1} compared to 45 cm yr^{-1} at Renland), resulting in lower resolution sampling with time.

Because GRIP has a much lower sample resolution, this introduces significant uncertainty to the analysis and diffusion correction. The lower accumulation rate also makes the diffusion correction more susceptible to seasonal bias in accumulation. We could rule out this issue at Renland by using the CFM test, but preliminary CFM models of WAIS Divide (which has similar temperature and accumulation rate to GRIP) have indicated a much stronger influence on the isotope signal. For WAIS Divide, additional proxy records such as chemistry data can be used to help constrain any changes in seasonality of accumulation (manuscript by Jones et al., will be submitted soon), but we do not have the needed additional data for GRIP. Therefore, we cannot rule out the possibility that any trends observed at GRIP are not simply due to changes in seasonality of accumulation, making a comparison to RECAP less meaningful.

For example, we have calculated the mean rate of change over the period from 2.6 ka–present for the normalized amplitude. While the rate of change is larger at RECAP (0.075 ka^{-1}) than at GRIP (0.034 ka^{-1}), the uncertainty on the amplitude for GRIP effectively overwhelms the signal. This is demonstrated by the gray shading in the figure shown here, which is substantially larger for GRIP than for RECAP.

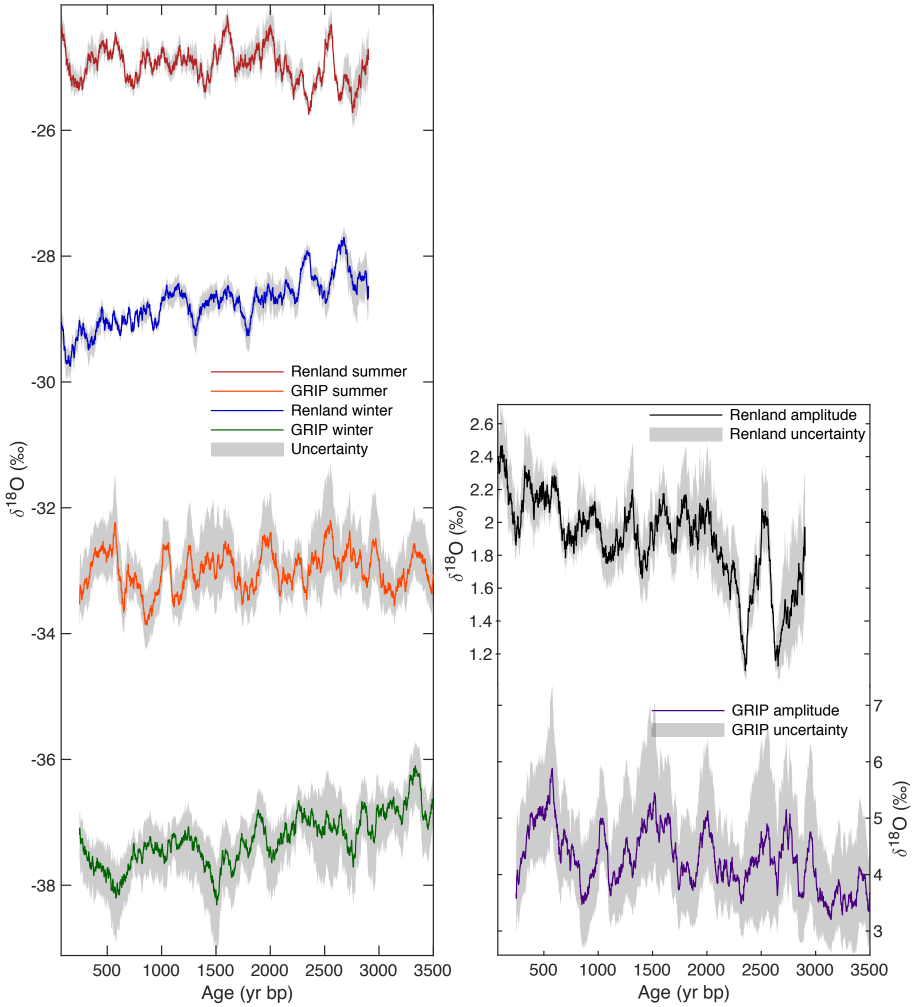


FIGURE 1. Comparison of RECAP and GRIP seasonal data. (a) The summer and winter δ^{18}O signal for both RECAP (red and blue) and GRIP (orange and green). (b) The annual amplitude for RECAP (black) and GRIP (purple) shown as both δ^{18}O (right axis) and normalized to the maximum δ^{18}O for each record (left axis).

Because we do not have an inland ice core with high enough sampling resolution and accumulation to accurately reconstruct the seasonal signal, we cannot claim that the trends observed in the RECAP isotope signal are unique. However, the RECAP record is unique in that it is the only existing Greenland core which we are able to use for this type of analysis, making it a very valuable record. We have revised part of Section 3.2.3 and the Conclusions to reflect this:

Section 3.2.3: “However, it is nearly certain that other regional climate variables have an influence on the δ^{18}O signal in the RECAP core. At this time we lack a comparison to seasonality at other locations in Greenland, which would help to determine the extent to which the trends observed in the RECAP record are due to local or regional influences. The RECAP core is unique in that it has both high sampling resolution (0.5 cm, whereas most other cores are over 2 cm), and high accumulation rate (45 cm yr^{-1} compared to 10–20 cm yr^{-1} inland), allowing for a much more accurate diffusion-correction of the seasonal isotope signal. At Renland, we may also observe the effects of atmospheric and oceanic circulation patterns and sea ice extent, which can control the influence of local oceanic moisture in comparison to long-range transport. This would alter the δ^{18}O signal through moisture source instead of a direct influence on local temperature (Johnsen et al, 2001; Klein and Welker, 2016). As we do not have records of isotope seasonality from inland Greenland, it is difficult to identify whether an effect such as this uniquely influences the RECAP isotope signal. These factors could be instead be further explored through additional modeling studies."

Conclusions: "At this time, Renland is the only available Greenland ice core that has high-resolution sampling and high accumulation rates. These factors are necessary to rule out seasonality of accumulation effects on diffusion, allowing for an interpretation of the summer and winter patterns for the last 2.6 ka. Whether Renland is unique in its downward trend in winter values remains to be seen."

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.

Done

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.

This statement refers not only to VM28-14, but also the discussion including other Greenland ice cores from Holme et al. (2019) and Chylek et al. (2011).

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.

"more" has been removed from this sentence.

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.

This is clarified throughout the paper, such that "Renland" refers to the peninsula and its local climate, and "RECAP" refers to the ice core and water isotope record.

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.

The caption has been revised:

"A map of the study region shows the RECAP drill site (red) is located on the east coast of Greenland. The Renland ice cap is approximately 80 km wide, and is isolated from the Greenland ice sheet. The drill site is near the summit of the ice cap, at a location of -26.75, 71.2333. The locations of several other Greenland ice cores are also shown for reference (blue), as well as North Atlantic sediment core VM 28-14 (green) (Pawlowicz, 2020)."

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).

We have replaced citations for Noone and Simmonds 2004 with Holme et al., 2019; Johnsen et al., 2001; and Klein and Welker, 2016, which more appropriately discusses Greenland climate.

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.

This paragraph has been revised:

"The RECAP ice core extends 584 m to bedrock, with the oldest ice dating 120 ka; here we present the Holocene water isotope record (i.e. δ^{18}O) (Fig. 2). Polar ice core water isotope records are correlated to condensation temperature at the time of precipitation (Dansgaard, 1964; Dansgaard et al., 1973; Craig and Gordon, 1965; Merlivat and Jouzel, 1979; Jouzel and Merlivat, 1984; Jouzel et al., 1997), and integrate across regional ocean and atmospheric circulation patterns and sea surface conditions along the moisture transport pathway (Johnsen et al., 2001; Holme et al., 2019).

The Renland Peninsula is located on the eastern coast of Greenland in the Scoresbysund Fjord (Fig. 1). The ice cap is unique in that it is isolated from the Greenland Ice Sheet by steep fjords and is only 80 km wide; as a result, the thickness of the ice cap is constrained and did not experience significant change during most of the Holocene (Vinther et al., 2009; Johnsen et al., 1992). The Renland Peninsula experienced post-glacial uplift in the early Holocene due to ice sheet retreat, but the rate of uplift has been minimal over the last 7 ka (Vinther et al., 2009). These factors imply that the climate record is not influenced by long-term changes in elevation through the mid to late Holocene. The modern accumulation rate at Renland is approximately 45 cm ice equivalent accumulation per year, resulting in clearly defined annual layers. In comparison to inland ice core records, the local climate at the coastal Renland site is more likely linked to sea surface conditions and North Atlantic climatology (Holme et al., 2019; Johnsen et al., 2001)."

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.

This sentence has been revised:

"Water isotope data has sub-mm nominal resolution, as there is a small amount of mixing introduced in the system due to liquid water mixing in tubing or vapor mixing (Gkinis et al. 2011, Jones et al. 2017)."

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.

This paragraph has been revised:

"The RECAP record exhibits a much higher effective resolution in the Holocene than during the glacial period. High-frequency variability is lost rapidly with depth due to an increase in the effect of water isotope diffusion, which occurs for two reasons: 1) At the last glacial maximum, the Laurentide ice sheet extended to approximately 40° N, and the associated temperature decrease and sea ice increase led to significantly drier conditions with minimal precipitation in Greenland. As a result, water isotope diffusion has a much greater effect in thinner glacial ice layers, acting to eliminate high-frequency signals. 2) Extreme basal thinning (see Fig. B1) effectively deforms annual layers into very thin intervals of ice, allowing solid-phase water isotope diffusion to have a disproportionately strong effect on the oldest ice. The glacial period is condensed into approximately 30 m, and no high-frequency climate signals (i.e. annual, interannual, decadal) are preserved. As a result, we focus here on the Holocene record through 10 ka, which has retained greater high-frequency climate information."

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.

'vapor pressure' has been removed.

P5 L104: Please provide a reference for the CFA system mixing effect, if not done before (please see respective comment above).

Citations have been added for Gkinis et al. 2011 and Jones et al. 2017 in L70.

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.

This section has been rewritten, and other instances have been corrected to PSD:

"...overlapping windows corresponding to 300-year time periods with a step size of 100 years between windows. This produces the power spectral density (PSD) ($‰^2$m) vs. frequency (f) (m^{-1}) for each 300-year window."

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.

The linear regression in log space and the Gaussian produce the same estimate of diffusion length, but it is easier to tune the fit of the Gaussian to find the most accurate estimate. The measurement noise does not influence the fit of the Gaussian, since only the diffused section (at lower frequencies than the noise) is used for the fit. This is shown in Fig. 3, by the blue line indicating the section used for fitting the Gaussian. LL113-114 has been edited to clarify this:

"Eq. 2 is fit to the diffused portion of the PSD to estimate σ_z (Fig. 3a, diffused section indicated by blue line)."

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?

Eq. 3 is adapted from Jones et al. (2017), which has a more in-depth derivation of the diffusion equations (we decided this would be an unnecessary repetition here, as it is cited). From Jones et al. (2017):
Eq. 3: $\sigma_z = \frac{1}{2\pi\sqrt{2}} \cdot \frac{1}{\sigma_g}$
Eq. 7: $\sigma_g = \sqrt{\frac{1}{2 \cdot \mathrm{abs}(m_{lr})}}$
Therefore combining these two equations yields our Eq. 3. The slope uncertainty is noted in L116 as the maximum and minimum slope (m_{lr}).

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.

LL127-128 has been revised:

"...However, the diffusion length in the time domain (Fig. 3d) increases with age. Because the effects of firn diffusion are locked in after the pore close-off depth, an increase in diffusion length with time could be potentially due to effects of solid-phase diffusion. Alternatively, changes in surface conditions..."

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

Fig. B2 caption has been revised to include more information about the fitting methods:

"Gaussian fits (green line) can be found for the diffused part of the spectrum (green points) for most 300-year PSD windows, but there are two problematic sections from 5.6–6.7 and 7.8–8 ka. Two fitting schemes are used for these sections to ensure bias is not introduced, both shown here for a window from 6363–6663 yr b2k which exhibits a double-Gaussian shape. Fit 1 (left) utilizes a similar fitting range as surrounding windows to determine the diffusion length. The fitting range for each window is interpolated from the reliable ranges applied to the spectrum on either side of the problematic section. This results in a poor Gaussian fit, but a better estimate of the fitting range. Alternatively, the fitting range for Fit 2 (right) is selected so that the second Gaussian is used to determine the diffusion length. This results in a better Guassian fit, but erratic changes in the fitting ranges applied to different windows, which is not observed in the reliable spectra.”

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

It is possible that there is some stratigraphic noise in the record observed in high frequencies; however, Renland has a much higher accumulation rate than the Antarctic ice core sites studied in Munch and Laepple (2018), and would be expected to have a higher signal-to-noise ratio. This is confirmed in Holme et al (2019), with a comparison of three cores drilled at Renland (1988 core, 1988 shallow core, and RECAP). Therefore we do not think that stratigraphic noise is significantly contributing to our spectral analysis of high frequencies.

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.

We feel it is appropriate to use the term amplitude, as it is referring to specific frequencies and is also defined here as the strength of the signal. For the calculation of relative amplitude/strength of frequency bands, we integrate the power spectrum across the frequencies, and then take the square root to find the amplitude. This is better clarified in L140:

“While the raw isotope data is comprised of a continuum of climate variability at all frequencies, it is possible to separate out the strength (ie. the amplitude) of varying frequency bands within that continuum. The power (P_i) of individual frequencies from 1–20 years are identified for each 300-year PSD window, as well as the frequency bands of 3–7, 7–15, 15–20, and 20–30 years. For frequency bands, the average power is calculated by integrating across the power spectrum within each frequency band:

$$P_i = \frac{\int_{f_a}^{f_b} P(f)\mathrm{d}f}{f_b - f_a} \qquad (2)$$

where f_a and f_b represent the lower and upper frequency limits, respectively. Because the frequencies are not evenly spaced, this method ensures that the average is not biased towards higher frequencies. The amplitude (ie. the strength) is calculated as the square root of P_i.”

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.

All instances of this have been corrected.

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.

The flat section of the spectra is likely an artifact, but is at much lower frequencies than we analyze here and so would not influence our results. The x-limits have been adjusted to cut out this portion of the spectrum, since it is not relevant and could introduce confusion. The figure caption has been revised:

"Isotope power spectra and diffusion length estimation. (a) The PSD for a 300-year window from 2263–2563 yr b2k. The diffused section of the spectrum is fit with a Gaussian (Eq. 1), used to estimate diffusion length for each window. The signal is cut off at a frequency of 1.1 yr^{-1} (indicated by vertical black line), after which point it is primarily noise. At periodicities below the cut-off frequency, diffusion correction of noise results in an unstable signal (as shown by the purple PSD to the right of the cut-off frequency). (b) Natural log of the same PSD..."

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?

The smoothness of the diffusion-corrected signal is a result of cutting off high frequencies, which blow up due to measurement noise. If we were to artificially diffuse the diffusion-corrected signal, we would not be able to add the high frequencies back in and it would result in a smoother signal that the original raw signal.

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)?

The 4‰ sine wave is chosen based on the mean annual signal in the upper 10 years of the core, for which we can assume there is little to no effect from diffusion. Because it is not possible to diffusion-correct the firn column for the last 76 years, this is the closest estimate we can make for the MAR period 1958-1978. LL164-165 has been revised to clarify:

"A constant amplitude (4‰) sine wave is used to represent the annual isotopic variability (Fig. B4c), based on the mean amplitude of the relatively un-diffused most recent 10 years of δ^{18}O signal."

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.

This section has been rewritten (note that Fig. 5 has been moved to the appendix and combined with Fig. B3; (a–b) refer to what was Fig. B3, and (c–e) refer to what was Fig. 5).

"...provided by the Modèle Atmosphérique Régional (MAR; version 3.9 with monthly ERA forcing) (Fettweis et al. 2017 (Fig. B4a–b) A constant amplitude (4‰) sine wave is used to represent the annual isotopic variability (Fig. B4c), based on the mean amplitude of the relatively un-diffused most recent 10 years of δ^{18}O signal. The mean monthly 1958–1978 temperature cycle (Fig. B4e) from MAR is used for all model scenarios. For the 20-year period, MAR simulates that the summer months July?September receive the most accumulation on average (Fig. B4d). The mean annual accumulation from the 175 20-year MAR period is used for all model scenarios, with the following variations on the seasonality of accumulation applied:...”

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”.

Fig. 6 caption has been revised:

“The relative amplitude of selected individual frequency bands from 1–20 years is shown for the RECAP δ^{18}O signal, with all bands normalized to the strength of the annual frequency in the most recent window. This demonstrates how rapidly the strength of the annual signal decreases, while lower frequency signals are preserved for a greater period of time.”

P10 L185 and Fig. 6 and 7: Why does the normalized amplitude has a unit (‰)?

The ‰ unit has been removed from the y-axis for normalized amplitudes in Figs. 6, 7, 8.

P10 LL186-187: Remove the sentence “Each frequency band is...”, since this information is already given in the Methods section and the figure caption.

Done

P12 L219: What kind of “similar 20-year signal”? Between the time series (correlation of e.g. 20-yr averages) or in spectral properties?

This section has been rephrased to clarify the 20-year variability observed in spectral properties:

...“Over the period from 1303–1961, spectral analysis shows 20-year variability in the ice core stack, also observed in prior model simulations of AMO (Chylek et al., 2011; Knight et al., 2005). The correlation between prominent multidecadal variability in both Greenland ice core records and climate model simulations is attributed to...”

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?

This is referring to the relationship between the RECAP δ^{18}O 15–20 year variability and AMO; this sentence has been revised to clarify:

“...Additionally, Knight et al. (2005) shows that the AMO signal observed in the HadCM3 model is non-stationary on millennial time scales, potentially explaining the variability in the strength of the 15–20 year signal at Renland. While the correlation between the marine HSG record and the RECAP ice core water isotope record is not conclusive evidence of the relationship between the RECAP 15–20 year δ^{18}O signal and AMO, there is a plausible physical connection...”

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?

Non-stationary here refers to the time series variance; L236 is revised to remove 'non-stationary' to avoid confusion:

"Both summer and winter records also exhibit some variability on centennial timescales over the last 2.6 ka."

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?

Seasonal changes in firn temperature are certainly a valid concern with respect to diffusion length, but we believe that this does not significantly influence our results for a few reasons. First, the CFM does use an annual temperature cycle in the diffusion model. While it is possible that the timing and value of the minimum and maximum annual temperature may vary slightly year-to-year, we would not expect this to impose significant changes in diffusion over a time scale of months. Simonsen (2011) discusses the temperature effect on diffusion with regards to major climate events such as the Bolling-Allerod and deglacation, which would experience much larger changes in mean annual temperature. Additionally, if there is a long-term effect in diffusion length due to changes in mean annual temperature (ie. slightly colder mean temperature at present than at 2.6 ka), this would be reflected in our diffusion length calculations.

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?

The pre-diffusion amplitude is identical to the diffusion-corrected constant accumulation case. A statement has been added to L254 has been revised to clarify this:

"Seasonally-biased accumulation scenarios can be compared to the constant accumulation scenario, in which each month receives the same amount of accumulation and the diffusion-corrected amplitude matches the pre-diffusion signal."

P14 Fig. 9: Please introduce the scope of the figure, e.g. "RECAP annual and seasonal δ^{18}O data."

Done.

P15 Fig. 10: Please clarify: "Comparison of diffusion-corrected seasonality...".

Done.

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%?

The percent difference is calculated for the annual amplitude, and has been revised to use the constant accumulation scenario as the reference. Previously the difference was calculated as (max amplitude-min amplitude)/(min amplitude) = 18%, and is now calculated as (max amplitude-min amplitude)/(max amplitude) = 15.7%. To make this more clear, two changes have been made.

First, Fig. 10 has been revised to have three panels: summer δ^{18}O, winter δ^{18}O, and annual amplitude δ^{18}O; this way the differences are easier to see. Second, LL255-256 has been revised to clarify that the change is in reference to the constant accumulation scenario annual amplitude:

"Seasonally-biased accumulation scenarios can be compared to the constant accumulation scenario, in which each month receives the same amount of accumulation and the diffusion-corrected amplitude matches the pre-diffusion signal. In comparison to the constant accumulation scenario, there is a maximum 15.7% decrease in the annual amplitude of diffusion-corrected isotope values for varied accumulation scenarios, which is a direct result of the bias in the diffusion-correction."

P16 LL274-275: "and the snow layers are so thick at Renland..."; this is repitition from above, you might consider removing this part.

Done.

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)?

Vinther et al. (2009) does not address seasonal isotope changes, making a comparison to our results here difficult. However it is relevant to note that Vinther et al. (2009) shows that no uplift correction is needed for at least the last 7 ka (and minimal prior to that), supporting that our results are not influenced by uplift. This is already noted in LL24-26. LL291-292 has been expanded to clarify the comparison between isotope changes and the insolation model:

"The RECAP seasonal δ^{18}O signal shows a stable summer signal and decreasing winter signal; therefore, expected seasonal temperature trends due solely to changes in insolation are not able to account for the observed changes in seasonal δ^{18}O at the Renland site."

P17 L307: Someone not familiar with the sea-ice edge distribution might wonder whether the area is actually ice free in summer at present?

Revised sentence to clarify:

"...whereas summers would largely be immune to sea ice response since nearby water bodies have little to no summer sea ice."

P17 L308: The first part of this paragraph seems to be a repetition from the previous one; please consider to restructure the two paragraphs.

This section has been extensively restructured; please see previous response to General comments for revised version.

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...".

Done.

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.

Fig. B2 caption has been revised to include more information about the fitting methods:

"Gaussian fits (green line) can be found for the diffused part of the spectrum (green points) for most 300-year PSD windows, but there are two problematic sections from 5.6–6.7 and 7.8–8 ka. Two fitting schemes are used for these sections to ensure bias is not introduced, both shown here for a window from 6363–6663 yr b2k which exhibits a double-Gaussian shape. Fit 1 (left) utilizes a similar fitting range as surrounding windows to determine the diffusion length. The fitting range for each window is interpolated from the reliable ranges applied to the spectrum on either side of the problematic section. This results in a poor Gaussian fit, but a better estimate of the fitting range. Alternatively, the fitting range for Fit 2 (right) is selected so that the second Gaussian is used to determine the diffusion length. This results in a better Guassian fit, but erratic changes in the fitting ranges applied to different windows, which is not observed in the reliable spectra."

Technical comments

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

Done.

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

Done.

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).

Done.

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

We prefer not to hyphenate "ice-core records". Changes have been made to hyphenate other phrases (see following instances listed).

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

Done.

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

Done.

P1 L21: Please change to "Renland Peninsula".

Done (also changed on L24).

P2 Fig. 1: Please change to “east coast”.

Done.

P3 L41: Please change to “northward”.

Done.

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

Done.

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.

The phrase ‘water isotope’ is commonly used in ice core literature, and is our preferred phrase for general references to the water isotope record. However we have reduced the number of uses by changing a number of phrases from ‘water isotope’ to ‘isotope’ or ‘δ^{18}O’ where appropriate.

P3 L62 and Eq. (1): Change “SMOW” to “VSMOW”.

Eq. 1 has been removed, and as such the need for abbreviation VSMOW has been removed. VSMOW is now only referenced to once as ’Vienna Standard Mean Ocean Water’.

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)).

Done.

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

Done.

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)”.

Done.

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”.

We have changed bp to b2k, and defined as ‘years before CE 2000’

P4 L79: Please change to “exibits a much higher effective resolution”.

Done.

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}”.

Done.

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).

Extra references are removed, keeping only Johnsen et al., 2000.

P5 L105: Change to “in the depth domain”.

Done.

P5 L106: Change “cycles/m” to “m^{-1}”.

Done.

P5 L108: Change to “To estimate the diffusion length” or “To estimate diffusion lengths”.

Done.

P5 Eq. (2) and P6 L115: Use upright font for mathematical functions such as “exp” and “ln”.

Done.

P6 L115: Change to “of the data”.

The clarification “of the diffused section of data” is necessary as the linear regression is fit to only a section of the spectrum, as indicated in Fig. 3b.

P6 L124: Change to “m yr^{-1}”.

Done.

P8 LL159-160: For the sake of clarity, please insert “to the resulting summer and winter time series” after “is applied”.

Done.

P9 L177: Change to “diffusion-correct”.

Done.

P10 L181: For the sake of clarity, please insert “with time” after “decays rapidly”.

Done.

P10 L182: Change to “diffusion-corrected”.

Done.

P10 L186: For what is the Jones et al. reference needed here?

This method of analysis was used in Jones et al., 2018; the reference has been moved to L140 in the methods section.

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.

The purple dashed line has been changed to light purple dashed, and it has been clarified in the caption that the lines mostly overlap as the two fitting scenarios result in nearly identical diffusion-corrected signals.

P13 L243: Change to “diffusion-correct”.

Done (changed all instances of ‘diffusion-correct’).

P13 L249 and P15 L256: Change to “diffusion-corrected”.

Done (changed all instances of ‘diffusion-corrected’).

P15 L259 “in which we are aware”: I am not familiar with this phrase; do you mean “which we are aware of”?

Changed to “of which we are aware”.

P16 Fig. 11 caption: Please clarify: “Changes in Holocene insolation...”

Changed to “Changes in late Holocene insolation...”.

P16 L279: Change “of top of atmosphere” to “of the top-of-the-atmosphere”.

Done.

P17 L305: Change Fall->fall and Spring->spring.

Done.

P17 L316 “The Renland ice core”: Please clarify “The RECAP ice core”.

Done.

P19 Figure A1 caption: Please rephrase: “Comparison of Renland and GRIP seasonal data. GRIP experiences...”.

Done.

P20 Figure B1 caption: Please clarify: “Mean annual layer thickness in the RECAP core...”.

Done.

P20 Figure B2 caption, second line: “to determine a diffusion”: Do you mean “to determine the diffusion length”?

Changed to “to determine the diffusion length”.