General comments

The authors present the d18O water isotope record for the Holocene from the RECAP ice core from Greenland. Using spectral analysis techniques, the authors compare the 15-20-year variability with the Bond Cycle. Using the Community Firn Model climate influences on diffusion correction are explored and a simple energy balance model is introduced to explore whether insolation and not sea ice could drive changes in d18O. Analysis of the seasonal signal of the last 2.6 ka reveal changes in the trends for summer and winter d18O signal. The authors speculate that these differences correspond to changes in sea ice conditions.

The manuscript is well written, and the methods and analysis are overall clearly explained. However, several places in the text, statements are written without showing sufficient values/analysis of the data to support these statements.

In the current version of the manuscript a large focus of the full manuscript is put on explaining the effects of sea ice variability on d18O signal in the ice core. This reflects an imbalance between the current well documented findings that the paper presents and the ideas and hypotheses that the authors mention without sufficient scientific argumentation. There are reasons to suspect that the observed trends and correlations can be results of the post-processing of data and not directly an effect of sea ice. It is possible that sea ice is the driver of these effects but without clearly documenting (e.g. using a model or other proxy data) that sea ice is expected to influence the ice core site, the argumentation becomes a bit weak.

A clearer separation between method uncertainties and their resulting effects on one side and then a separate discussion on effects caused by climate/sea ice variability would strengthen the scientific argumentation significantly.

The presented d18O data from the Holocene part of the ice core is of great value to the scientific community, both on annual and seasonal values. Connecting the RECAP ice core signal to the regional sea ice signal is a shared interest among paleo climatologists from several disciplines. It is therefore highly relevant that the authors pursue this connection. However, the authors are encouraged to significantly strengthen the analysis on method weaknesses regarding diffusion correction and strengthen the argumentation regarding the hypothesized sea ice influence on the d18O signal.

Based on the above I suggest publication with major revision.

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.

Major comments:

Influence of diffusion correction on findings: In the paper by Vinther et al, 2010 (sec 4) the following statement is written "Looking at the 14 winter and summer season d18O series presented in Figs. 5 and 6 it can be seen that the time series from Renland and DYE-3 show least variability in the high-frequency domain. It should be noted immediately that this apparent lack of variability is a consequence of the particular diffusion correction applied to these series and should not be interpreted as a consequence of a different climatic forcing."

This highlights an important caveat of this paper. Diffusion correction can influence variability in the signals as a result of the method itself. The effects of this must be clearly and thoroughly demonstrated. In addition, it is relevant that the authors clearly state how the method applied in this study differs from the method in Vinther et al 2010 and thus that the diffusion correction is ok to apply for RECAP.

There are generally two strategies to account for diffusion in water isotope records of ice cores: 1) Back diffusion (estimated directly from the data) and 2) Forward diffusion (estimated from models). In case 1, as we have done in this paper, we correct for the effects of diffusion to recover an estimate of the isotopic signal as it would have existed at the surface of the ice sheet. In case 2, we can advance diffusion to be equal to the maximum amount of diffusion in the ice core; this will eliminate high-frequency information. Vinther et al. 2010 opted for case 2, but their record was from an older ice core which was measured using now outdated technology, with much lower resolution than the record we recovered recently. Thus, Vinther was dealing with a water isotope record that was already compromised in terms of preserved high-frequency variability, partly erased by the sampling procedure, and with high-frequency data further removed due to forward diffusion. With new technology, we have a record that preserves more high frequency information, thus we can use case 1 of back diffusion, which preserves the high-frequency data for climatic interpretations.

Another significant problem not stated by Vinther et al. 2010 is that diffusion models are highly uncertain. For example, both Johnsen et al. 2000 (in his seminal paper) and Jones et al. 2017 note that diffusion in excess of what models predict is evident in the GRIP and WAIS cores. At this time, it is accepted that firn diffusion models cannot always accurately reconstruct diffusion lengths, and should be approached carefully. This means that case 1 above is the preferred method, which estimates diffusion length directly from raw data, rather than relying on uncertain models. Since case 1 allowed for reliable Gaussian fits for the last 2.6 ka, we opted to use this method.

As Vinther et al. 2010 state, melt layers can occur at Renland. But, this is true of inland ice core records, as seen in 2012 on the Greenland ice divide (and known to have occurred in the past, e.g. in the GRIP record) as well as the WAIS Divide ice core in West Antarctica (Jones et al. 2017). Vinther et al. states that melt layers will cause ”ringing” in the diffusion-corrected isotope data. Despite melt layers, neither GRIP nor WAIS exhibit ringing, and there is no evidence for ’ringing’ from melt layers in our diffusion corrected interval to 2.6 ka for the annual signal, summer, and winter (see later comment for further discussion on melt in the RECAP core).

Even if there is still concern with the aforementioned methods in our paper, there is one important point that cannot be ignored: the pattern we observe in diffusion corrected data is also evident in the raw data. Thus, the diffusion correction we apply, and the associated summer and winter patterns, is not an artifact of the diffusion process.

We further prove that the seasonality of accumulation - not treated in the Vinther et al paper - cannot account for the pattern of summer, winter, and annual amplitudes. The seasonality of accumulation can weight diffusion toward whichever season has less snowfall. This is problematic because diffusion corrections assume a constant snowfall rate throughout the year. The high accumulation rate at Renland is beneficial in this regard, as any seasonality effect on diffusion is sufficiently damped to preserve the summer and winter patterns. Interestingly, a WAIS paper in prep. requires additional impurity information to constrain the annual signal since WAIS has much less accumulation than Renland.

When the strengths and weaknesses of the applied method are introduced it is meaningful to first thereafter explore effects of climatic variability on the diffusion correction, as done with the CFM model, which is introduced to explore the effect of changes in accumulation seasonality. Please also discuss issues regarding the interplay between accumulation seasonality, insolation and sea ice changes. Can accumulation seasonality in reality considered to be constant or are the combined effects on diffusion correction larger?

The goals of our study are as follows: 1) Determine if the back diffusion method (rather than forward diffusion) was valid for the new high-resolution RECAP record. (We find that for the last 2.6 ka, back diffusion is possible with good Gaussian fits to the raw data). 2) Determine if seasonality of accumulation affected firn diffusion enough to alter the summer and winter signals. (We determine that the patterns in summer and winter on millennial timescales are robust). 3) Provide varying hypotheses for why these patterns might occur, leaving GCM and/or isotope-enabled modeling for future studies. There are some limitations to our methods. For goal 2) We cannot constrain variations in the seasonality of accumulation (i.e. we cannot claim this seasonality effect is a constant), thus centennial scale variability in summer and winter could be purely an artifact of firn diffusion, rather than a direct climate signal. For 3), we cannot conclusively determine a cause of the decreasing winter signal, for example. The modeling required to answer this question is outside the scope of our paper, but does provide opportunities for future papers to resolve this RECAP record, and hopefully other high-resolution records planned for the next decade.

To answer the reviewers question, we cannot directly treat the interplay between accumulation seasonality, insolation and sea ice changes without more advanced modeling, which will have to be reserved for future studies. Thus, we recognize the limitations of our study, and look forward to future results that validate the cause of Renland isotope seasonality. The seasonality effect cannot be considered constant, and we do not have sufficient impurity data or other proxies to constrain the seasonality effect beyond the work we have done using the CFM. It should be noted that the CFM results are an extreme case, meant to show that even a highly unlikely shift in seasonality cannot remotely come close to explaining the trends we see in the isotope data.

However, we will note that changes in seasonality of accumulation is the only climate variable that could likely introduce significant uncertainty to our diffusion correction results. While changes in temperature (driven by variability in insolation, sea ice, or other factors) can influence the diffusion length, these factors are included in our diffusion length estimation because it is calculated directly from the isotope data, and they would not introduce a seasonal bias. If, for example, we had estimated diffusion length by means of modeling using a thinning function and temperature estimate, climate variables could introduce uncertainty since they would be more difficult to constrain. L241 has been revised to clarify this:

"Climate variability such as changes in temperature and accumulation rate can influence the extent of diffusion that occurs in the firn column. While the method of estimating diffusion length directly from the water isotope record includes the effects of long-term changes in mean temperature and accumulation rate, changes in seasonality of accumulation creates uncertainty in the diffusion-correction calculation of the annual cycle."

Sea ice signals in the RECAP core: A change in sea ice does not always directly change into a similar change in d18O. See e.g. Holme et al., 2019, Faber et al. 2017, Sime et al. 2013, Divine et al., 2011. And for paleoclimate signals on Merz et al., 2015 and Li et al 2010. The authors are currently not demonstrating the processes in which a regional sea ice change near RECAP translates into a changed d18O signal. Existing literature is used to argue that a link is plausible through d180, sea ice and AMOC, but the demonstration that this is actually the case for RECAP is missing. Maffezzoli et al 2018 explored sea ice in the RECAP using impurities. The findings from this paper is extremely relevant to include here in order to argue for how sea ice variability is "seen" from the RECAP core using impurities. In the current approach the authors introduce a simple energy balance model to only because the variability in surface temperature (and therefore d180??) is not caused by insolation and thus indirectly argue that sea ice is the driver of the variability. This is not convincing. Effects on d18O and atmospheric circulation are not considered in this approach. I strongly suggest that the authors to include the use of (isotope) model simulations, moisture source tracking or similar to strengthen the argument on how the connection between sea ice and RECAP d18O variability must be used in order to demonstrate that the effects of Holocene sea ice variability is reflected in the d18O of RECAP.

As stated above, a limitation of our study is a lack of GCM and/or isotope-enabled modeling. We have made this more clear in the paper: This is already mentioned in L229, and we have added in section 3.2.3 and in conclusions that modeling would be beneficial as a future study. However, we do want to offer potential hypotheses for the pattern that we observe in summer and winter, and we have included identified text in the manuscript to alert the reader to any hypotheses we are making. We have reframed section 3.2.3 as a more general look at regional climate and sea surface conditions, and consider hypotheses for how these factors could influence the seasonal δ^{18}O record over the last 2.6 ka. We include a citation for Maffezzoli et al. 2019, which does demonstrate how the bromine enrichment proxy records sea ice in the RECAP core. However, the discussion in Maffezzoli et al. 2019 is limited to the last glacial period and deglaciation, so the impurities record cannot be used as a direct comparison to our results through 2.6 ka. As additionally mentioned in L309, Corella et al., 2019 does provide a record of impurities at RECAP in the Holocene. The following is the revised Section 3.2.3 "Regional climate variables":

"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. ”

RECAP and GRIP comparison (appendix A): This is interesting and important. The study argues that RECAP record signals of sea ice variability. Thus, it is important to demonstrate that RECAP is unique in this sense and that the same variability patterns are not found in the same cores. Differences in terms of accumulation and measurements resolution exists among the cores which creates issues, but the authors must present stronger arguments for why they find that RECAP variability is unique and driven by sea ice. This deserves to be treated thoroughly in the manuscript instead of the appendix.

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.

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

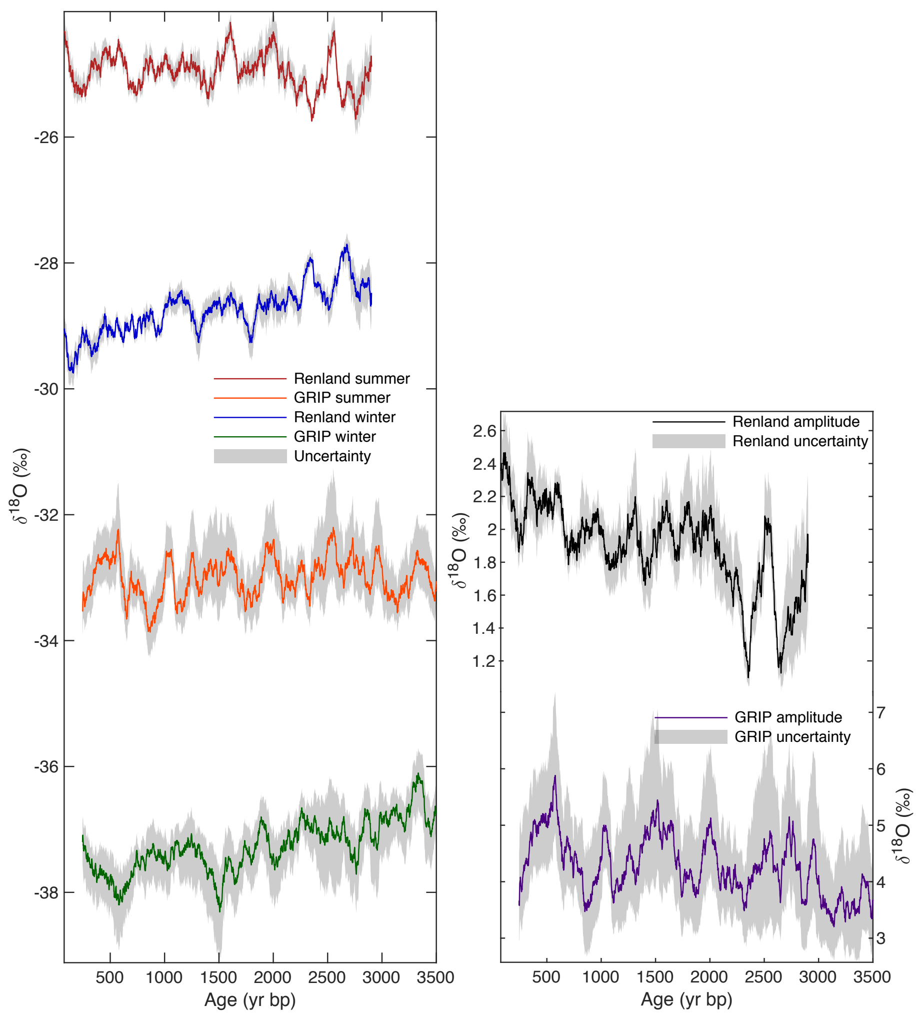

FIGURE 1. Comparison of RECAP and GRIP seasonal data. (a) The summer and winter $\delta^{18}O$ signal for both RECAP (red and blue) and GRIP (orange and green). (b) The annual amplitude for RECAP (black) and GRIP (purple) shown as both $\delta^{18}O$ (right axis) and normalized to the maximum $\delta^{18}O$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

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."

Melt: The authors address the effect of melt in L264ff. For the Holocene, frequent summer melt must be expected at RECAP. It is unclear how melt is influenced the d180 seasonality trough vertical mixing of summer and winter layers. Given the importance of reconstructing a correct summer/winter d18O signal for the conclusions of this paper, the role of melt on seasonality is relevant to address further. The authors are free to find the best approach to address this challenging issue.

Vinther et al. 2010 state that the shape of the diffusion-corrected water isotope data will be altered by melt layers, resulting in 'ringing' effects of spurious high-frequency oscillations. In the last 2.6 ka - the interval of time for which we interpret the annual signal - we do not observe ringing in diffusion-corrected data. This strongly suggests that melt layers have had a minimal impact. As mentioned by Johnsen et al. 2000, diffusion in a firn column without melt layers is expected to produce isotope data in which the fit to the diffused portion of the PSD is a Gaussian (Johnsen et al., 2000). Indeed, this expectation holds for the last 2.6 ka in the RECAP isotope record.

This section has been expanded to better clarify the potential effects of melt on the seasonal layers, including a better description of the vertical mixing of seasonal layers, discussion of the 'ringing' effect in the spectrum, and further comparison between data in Taranczewski et al. (2019) and our seasonal data:

"Since the Renland site is subject to warm summer temperatures, we must also consider the possible effects of melt layers on the seasonal signal. A summer melt event could cause surface snow with a relatively high δ^{18}O signal (ie. near the seasonal peak) to percolate vertically through the firn column, mixing with the underlying winter layer which has a lower δ^{18}O value. This mixing would cause the preserved δ^{18}O value of the winter layer to increase, resulting in a decrease in the annual amplitude. Alternatively, melt water which refreezes in the firn as an ice lens can produce a local barrier to further diffusion. In either case, it is important to consider the extent to which melt layers could influence the recorded water isotope signal, which we do so by examining both the isotope data and the melt layer density in the RECAP core.

Diffusion in a firn column without melt layers is expected to produce isotope data in which the fit to the diffused portion of the PSD is a Gaussian (Johnsen et al., 2000). Substantial alteration of firn processes due to melt, either through liquid water mixing or an ice lens barrier to diffusion, would likely influence the shape of the spectrum. Over the time period in which we reconstruct the annual signal, we do not observe degradation of the Gaussian fit in the 300-year windows of PSD, indicating that melt has not significantly influenced the isotope data. Additionally, it has been noted that melt layers can cause a 'ringing' effect in the diffusion-corrected data, resulting in spurious high-frequency oscillations (Vinther et al., 2010). We do not observe this effect in the diffusion-corrected isotope data at RECAP over the last 2.6 ka.

The density of melt layers at RECAP was measured by Taranczewski et al., (2019), determining a high-resolution record for the last 2.1 ka. The ratio of snow water equivalent of a melt layer to the respective annual layer is characterized as the annual melt ratio (AMR), which over the last 2.1 ka has an average value of less than 2% and is therefore a very small fraction of the total annual ice volume (<1 cm for 45 cm ice per year). The AMR exhibited some centennial variability with a few distinct periods of increased melt, but did not demonstrate a long-term trend over the last 2.1 ka (Taranczewski et al., 2019). Furthermore, during brief periods in which there is a $\sim$1% increase in AMR (ie. 1850–1700 yr b2k, 200 yr b2k–present), increases in melt layer occurrence would likely serve to decrease the annual amplitude, which we do not observe. Based on this evidence, it is likely that the presence of melt layers is not significantly influencing the seasonality trends observed in the δ^{18}O signal. "

Sec 3 "Results and discussion". I encourage the authors to separate the results from discussion to not mix up results from this study with hypotheses entirely based on other studies.

We find that the flow of the paper is better with a combined 'Results and discussion' section, so as to not repeatedly jump between analysis of interannual climate variability and seasonality. However, 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."

Comparison with VM28-18: It is relevant and meaningful to compare the data to this core and the authors argues for this in a good way. It is however symptomatic for this analysis that this hypothesis is entirely driven by other studies and the data and analysis in this study does not convincingly support this hypothesis (full span of the records r=0.34). In the current format of the paper this link with the Bond cycles provide an argument for connecting d18O records to sea ice. I suggest the authors to reconsider whether this approach is optimal. If yes, then the analysis of the correlation between d18O and Bond cycles must be statistically stronger to ensure that this correlation is not just noise and coincidence. If no, then please explore d18O and sea ice in alternative ways as explained later in this review.

We agree that there is not enough evidence to draw any conclusions about a correlation between our records and the VM28-14. As stated, it could arise purely by chance due to noise in the system, and the correlation is much too low for significance. While it is possible that a better timescale for VM28-14 could result in an increased correlation (as discussed in a later comment), we cannot completely discount the possibility that this relationship is just coincidence. Yet, discussion of Bond Cycle in both the Holocene and glacial has persisted for decades, and this is yet another breadcrumb in that debate. There is a chance that the decadal variability at Renland is related to these Bond Cycles, and we provide Figure 8 as evidence of a potential link. In this paper, we cannot make a definitive conclusion about any such relation, and we are very cautious in how we present the relationship (ie. L192 "...we cannot rule out the possibility that this similarity arises due to random noise in the climate system..."; L226 "While the correlation ... is not conclusive evidence of this relationship, there is a plausible physical connection..."). However, the information is important for future modeling that could explore links between ocean circulation and decadal variability expressed through the hydrologic cycle at Renland.

Sec 2.5 Community Firn Model (CFM): This section has several issues. The motivation for introducing the CFM, is only mentioned in the end of the section and is unclear for the reader. Please expand the text to explain accordingly.

We have moved the description of the purpose of the CFM test from L241 (results section) to L162 (methods sections, where CFM is introduced):

"Climate variability such as changes in temperature and accumulation rate can influence the extent of diffusion that occurs in the firn column. While the method of estimating diffusion length directly from the water isotope record includes the effects of long-term changes in mean temperature and accumulation rate, changes in seasonality of accumulation creates uncertainty in the diffusion-correction calculation of the annual cycle. If there is a seasonal accumulation bias (i.e. more snow in summer than winter), the drier season will be subject to greater isotopic attenuation due to firn diffusion. Because we cannot selectively diffusion-correct the seasonal isotope signal for accumulation bias, we must assume constant seasonality of accumulation. Therefore, the drier season will be under-corrected for diffusion, and the wetter season will be over-corrected to a lesser extent, resulting in potential inaccuracies in the amplitude of the seasonal signal. While there is no current method for reconstructing past seasonality of accumulation, we can utilize a series of tests to determine the extent to which the diffusion-correction calculation for δ^{18}O could be affected by seasonally-biased accumulation.

We use the Community Firn Model (CFM) (Stevens et al., 2020) to test the effects of seasonally-biased accumulation on firn diffusion in the ice core. We test five 490-year scenarios for isotope evolution..."

Using CFM with input from MAR forced by ERA-Interim creates a long chain of uncertainties and known model biases. Especially for the Renland Ice Cap which is not represented well in most model grids. Nearby coastal and AWS weather stations exists, please demonstrate that the model results are in line with nearby observations. The outcome/results of this method are not clear from the text (but clearer when fig 10 is shown)

Nearby weather stations have only been recently installed and do not have as long records (ie. the closest PROMICE station was installed in 2008), and coastal stations with longer records are at significantly lower elevation and different latitudes. The output using the ERA forcing data is validated using existing weather stations and satellite data (Fettweis et al. 2017), and is expected to be the best available estimate of local climate.

Furthermore, a key point here is that our analysis with the CFM uses the MAR data only as an estimate of modern-day accumulation, in order to select a highly unlikely end-member possibility of changing accumulation seasonality; therefore the analysis is not dependent on getting present-day climatology exactly correct. Any biases in the MAR data would not substantially change the modeled isotope values as we are already using extreme scenarios, and existing climate records do not indicate a change in seasonality of accumulation as large as what we model. To clarify this in the text, we have added the following statement to L261:

"While this analysis may include uncertainties in the MAR reanalysis data, it provides an end-member possibility for highly unlikely shifts in seasonality of accumulation, demonstrating that the effect on δ^{18}O seasonality is minimal in comparison to observed trends."

Methods: Please add a short separate section for information regarding the location and drilling of the RECAP core (see similar in Holme et al, 2019 and Maffezzoli et al 2018)

The following text has been added to the beginning of the Methods section:

"The RECAP core was drilled near the summit of the Renland ice cap (-26.75, 71.2333) from May-June 2015. The drilling location is approximately 2 km away from the location of a core previously drilled in 1988 (Johnsen et al, 1992). The 584 m RECAP core, drilled to bedrock, contains a continuous record extending through 120 ka."

Naming RECAP vs Renland: The manuscript refers to the ice core as Renland (except for the Figure caption of fig 2) The Renland core was drilled in 1988 and RECAP (REnland ice CAP) ice core in 2015 nearby, both on the Renland Ice Cap. I suggest that the text is corrected in a suitable way to avoid misunderstandings and reflect the correct naming of the core.

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.

Comparison with existing record (Renland 1988): How does the former Renland core compare to the newer RECAP core? Do we see the same signal for the Holocene period or are there any deviations? Other than differences in instruments and precision the cores are not expected to be very different, but it would be good to demonstrate the agreement with the previous core. This can for instance be done in one of the very first figures.

The former Renland 1988 record was measured at much lower resolution and potentially has dating inconsistencies, resulting in a vastly different isotope signal. For example, the 8.2 ka event, clearly visible in the new core, is not resolved in the 1988 record. Lack of tie points likely contributed to dating issues in the 1988 record, making a direct comparison between the two cores difficult. For this reason, we have chosen not to include a comparison as it may only introduce confusion. However we have added citations for prior publications including the Renland 1988 data (Johnsen et al.

1992) to the beginning of the Methods section, if the reader wishes to make their own comparison.

Figures: The manuscript is relatively long and have several figures of less relevance. It would strengthen the quality and readability of the manuscript if some of these less relevant figures would be placed in the supplementary instead. Examples are Fig 5, Fig 6,7

Figure 5 has been added to the supplementary material, as a panel of Figure B3, and the caption for Figure B3 has been revised accordingly. However, we maintain that Figs 6 and 7 are relevant enough to remain in the main text of the paper. Fig. 6 and 7 both show the effect of diffusion on higher frequency bands, supporting our decision to focus on analysis of 0–8 ka bp, and 0–2.6 ka bp for the annual signal. Fig. 7 shows that the diffusion correction does not influence the shape of the frequency band relative amplitudes, which is important to consider when comparing the 15-20 year band to core VM 28-14.

Minor comments

Figure 1: Please add the description of the location markers and the reasons for the different colors (ice, marine etc) to the figure caption

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)."

L29: This reference "Noone and Simmonds 2004" concerns conditions in Antarctica and is not suitable here. Please use a better reference, e.g. evidence from other analysis on the same core or similar (e.g. Holme 2018)

The reference to Noone and Simmonds 2004 has been changed to Holme et al. 2019.

L29-30: "These climate parameters are recorded in the ice core water isotope (i.e. δ 18 30 O) record through changes in condensation temperature at the time of precipitation". This statement does not agree with the last 10-15 years of isotope and ice core research. Please add a few lines with supporting newer references that explain how the ice core isotopes is an integrated signal of several processes from source to site.

This statement has been revised:

"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)."

L60-65: This should be well known to most readers, and is not relevant in the method section

This section has been shortened and instead amended to L69:

"...This technique produces δ^{18}O, δD, and δ^{17}O water isotopes, where delta notation refers to a ratio of heavy to light isotopes measured with respect to a standard (Vienna Standard Mean Ocean Water) and is expressed in parts per thousand (per mille or ‰) (Dansgaard, 1964). Water isotope data has sub-mm nominal resolution..."

L88-98: This text is mainly "textbook material" and does not belong in a method section. Please shorten this.

LL88-96 has been kept as a brief explanation of diffusion, but is shortened:

"In the firn column, vapor diffuses along concentration and temperature gradients. Exchange of water molecules takes place between unconsolidated snow grains and vapor, attenuating the seasonal water isotope signal and acting as a smoothing function (Whillans and Grootes, 1985; Cuffey and Steig, 1998; Johnsen et al., 2000; Jones et al., 2017a). Solid-phase water isotope diffusion in ice below the firn column occurs at a much slower rate, with diffusivities increasing for warmer ice near bedrock. Over thousands of years, solid-phase diffusion can have a substantial impact on the attenuation and smoothing of high-frequency signals (Itagaki, 1967; Robin, 1983; Johnsen et al., 2000; Gkinis et al., 2014; Jones et al., 2017a)."

L164. Please argue for the choice and robustness of assuming a 4 permill sine wave for the amplitude which is larger than shown later.

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 the δ^{18}O signal."

L163: The authors chose to force the MAR model with re-analysis data in a time period pre-satelite era. Biases in this time period is expected to be large in the Artic region given the very limited data. Please discuss, maybe based on Fettweiss et al 2017, whether this choice is expected to introduce new biases to the results.

The MAR model is validated with several other datasets, including ice core records and surface mass balance measurements which span the period used here. It is also validated with satellite data starting in 1979, and PROMICE data weather station data starting in 2007, improving model output for earlier time periods (Fettweis et al., 2017).

We do not expect that this choice would introduce biases in the results. In our model, we show that even with large bias and variability in seasonality of accumulation, the diffusion correction of the water isotope signal is not significantly influenced. The input data would have to be substantially different to change this result, and we believe this is unlikely. See previous comment for changes made to text to clarify this.

L165: exchange the word predict with simulate. (The model simulates temperatures in the past)

Done.

L165: "...July-September receive the most precipitation on average". How much more precipitation comes during summer than winter? Please add the relevant numbers to support this statement including relevant statistics as precipitation is highly variable on Greenland, especially on the coast.

This statement is supported by Fig. 5 and Fig. B3, which have been combined in the appendix. Seasonal accumulation bias is shown in [previously] Fig. 5, and standard deviation has been added to show variability. A reference to the figure has been added to the text for clarity.

L167-172. Please be clear how and if the sum of annual accumulation varies from year to year in each of the scenarios.

The sum of annual accumulation is the same for all scenarios, with just the monthly distribution changing; L166–167 is revised to clarify:

"...The mean annual accumulation from the 20-year MAR period is used for all model scenarios, with the following variations on the seasonality of accumulation applied..."

Fig 5,6,7: I suggest that these figures are added to the supplementary material instead.

Figure 5 has been added to the supplementary material, as a panel of Figure B3, and the caption for Figure B3 has been revised accordingly. However, we maintain that Figs 6 and 7 are relevant enough to remain in the main text of the paper. Fig. 6 and 7 both show the effect of diffusion on higher frequency bands, supporting our decision to focus on analysis of 0–8 ka bp, and 0–2.6 ka bp for the annual signal. Fig. 7 shows that the diffusion correction does not influence the shape of the frequency band relative amplitudes, which is important to consider when comparing the 15-20 year band to core VM 28-14.

Figure 5: Please add the standard deviations to the figure. The figure B3 demonstrate large variability.

The standard deviations have been added to accumulation and temperature values.

L191: Could the variability in correlation strength be due to methodology/age model? Are there reasons to believe that age model differences in the Bond core can explain this mismatch? Please discuss this.

L192 has been expanded to introduce this possibility:

"...that they are statistically significant. Additionally, it is possible that inaccuracies in the dating model for VM28-14 (on the order of $\pm$ 200–500 years) (Bond et al., 2001) could account for some of the reduced strength in correlation outside of the period from 2.5–6.4 ka. While we cannot rule out the possibility that this similarity..."

L192: Please clarify that the p-values are calculated on time series without significant autocorrelation

L192 has been revised:

"Both relationships have a p-value <0.01, indicating that they are statistically significant, and the time series are not significantly autocorrelated."

L214: If this is a finding from Holme et al 2019, please connect this statement with a repeated reference to this paper.

The repeated reference to Holme et al. 2019 has been added.

Fig 9: is panel (a) necessary to plot? Removing this would make this figure a little less chaotic.

We believe that panel (a) is useful to show the comparison between raw and diffusion-corrected data for the full annual record. However, it is not critical to this figure, so it has been moved to the appendix to make Figure 9 easier to interpret.

L250: "The model results show that the Renland diffusion correction is minimally influenced by seasonality of accumulation (Fig. 10)". How is this clear? Please argue with numbers and a changed design of fig 10 (see below)

Fig. 10 has been revised (see below) and LL255-256 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. 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. Thus, in the unlikely case that accumulation shifted from a constant scenario to a seasonal-bias over the last 2.6 ka at Renland, we could expect up to a 15.7% offset from the true value of the annual amplitude... Furthermore, the annual amplitude in the observed RECAP water isotope record increases by approximately 50%...."

Fig 10: This plot is difficult to read and interpret. Either plot these on two different plots for summer and winter, or consider plotting the anomaly from the mean instead. In the current version of the figure it is unclear what the authors wants to demonstrate.

This figure has been updated to have three panels: 1) summer; 2) winter; 3) annual amplitude:

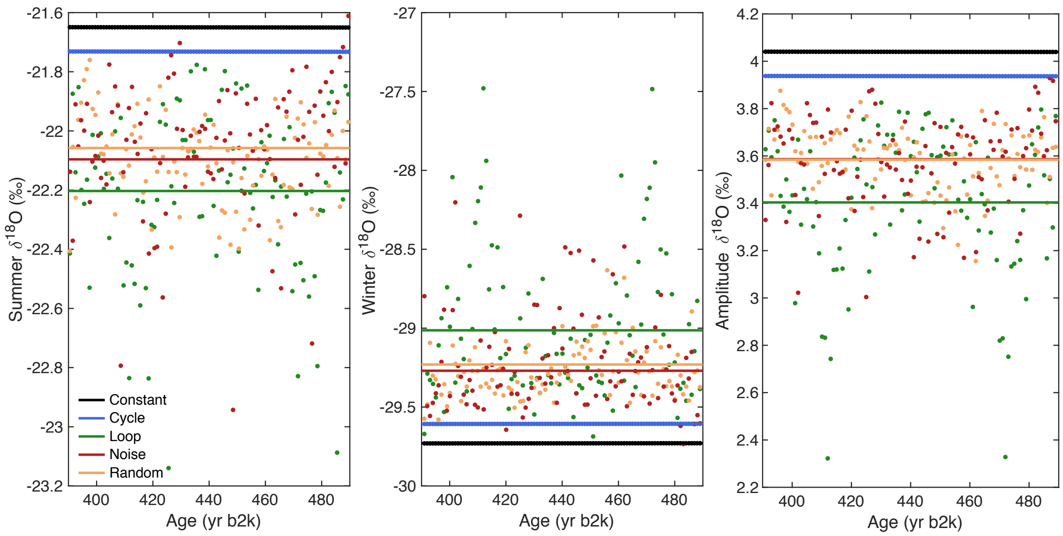

Figure A1: Please improve the figure caption to separately describe the left and right panel

This figure has been removed from the paper; see previous comment about the RECAP/GRIP comparison for details.

Appendix A1: It is argued that the effect of sea ice on GRIP is muted, but Fig A1 right panel show that both experience an increase in amplitude, but GRIP looks muted as the range of d180 is larger for GRIP. Please plot these on comparable scales. And argue sufficiently that RECAP is unique compared to GRIP. The conclusions about the comparisons in this section are currently not fully supported by values from analysis of the used models and data. E.g. L342: "A caveat in analyzing this data is that accumulation may have a greater seasonal bias at GRIP". Please support all statements in the appendix with values e.g. from MAR, CFM and ice cores.

This figure has been removed from the paper; see previous comment about the RECAP/GRIP comparison for details.

L335: Following open access style please provide all data types (raw and formatted) online before publication.

The data is currently in review for availability on Pangea (availability statement has been updated to reflect location).