Review

High-frequency climate oscillations in the Holocene from a coastal-dome ice core in east central Greenland

Hughes et al, CPD, 2020

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.

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 $d^{18}O$ 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.

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?

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.

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

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.

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.

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.

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.

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)

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)

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.

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.

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

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

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)

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.

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

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

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

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.

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

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.

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

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

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

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

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

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

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: 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.

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

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.

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