Response to Anonymous Referee #1

We would like to thank the referee for taking the time to review our manuscript and for their comments. We have included the reviewer comments below along with our responses.

Reviewer comment: Richter et al. present a temperature reconstruction for a season that is rarely captured by proxy archives. To do so, they apply an exciting emerging alkenone-based proxy (UK37) using an elaborate extraction and purification procedure. The authors build on a previously published robust chronological framework that allows them to confidently resolve shifts in multi-decadal climate conditions. I do, however, have a number of major concerns about the analysis and interpretation of this work:

The presented dataset suffers from species mixing, complicating its interpretation as a temperature record. Now, the authors use RIK37 cut-off values to exclude samples that are dominated by Group II haptophytes. As calibrations exist for this phylotype, a significant portion of their data points is excluded as a consequences of this rather crude solution. I would recommend the authors to calculate the RIK38E index to better differentiate phylogeny (mixing) and derive temperatures from Group II data.

Response: Developing a temperature calibration using the RIK_{38E} index would not resolve the issue of species mixing as both Group I and II Isochrysidales produce C_{38}Et alkenones (see Zheng et al., 2019). Further temperature calibrations for Group II vary for planktonic and benthic species (see D'Andrea et al., 2016). We decided to rely on C_{37}Me alkenones due to the low concentrations of C_{38}Et and C_{38}Me alkenones. Further, the U^{K}_{37} index was successfully applied to reconstruct temperature changes in other lakes containing Group I Isochrysidales (D'Andrea et al., 2011, 2012; van der Bilt et al., 2019; Harning et al., 2020; Longo et al., 2020).

Reviewer comment: I commend the authors for their efforts to better constrain the seasonality of haptophyte production (and temperature sensitivity), but have two concerns. First, ice-off dictates the timing of haptophyte blooms: with this in mind, I wonder why the authors did not rely on satellite data to validate the 30 yr control run outlined in section 2.4. High-res imagery is freely available for the entire period: if in agreement with model output, this would significantly strengthen the robustness of their approach. Secondly, the presented modelling results reveal that both late winter as well as spring season temperatures help determine ice-off dates: this does not justify presenting the record as a "cod season" reconstruction.

Response: Thank you for the suggestion, however, the purpose of performing the sensitivity studies with the lake model is to identify the main drivers that influence lake water temperatures during the spring season and, therefore, our proxy. Validating the exact date of spring ice-off would not alter the main conclusions of our study. Further, previous work has shown that the primary alkenone bloom likely begins prior to the exact ice-off date (Longo et al., 2018). We will clarify the points discussed above in the text of section 2.4.

We will change "cold season" to "winter and spring"/ "winter-spring" in our manuscript, where winter-spring is defined as December to May.

Reviewer comment: See line 215: I think the wording is far too strong here. The authors argue existing calibrations provide "unreasonable" estimates and back this up with unrealistically high temperature values. They do, however, not state that these values were calculated using site-specific intercepts provided for each of the used calibrations while the authors of the applied calibrations advise against doing so. To remedy this, I advise the authors to discuss the relative temperature fluctuations plotted in Fig. A2(b): indeed, the magnitude of these swings are of equal magnitude as those observed during the spring transitional season (Fig. 5b).

Response: Thank you for the suggestion, we will modify section 3.3 to discuss the relative temperature fluctuations in Fig. A2(b).

Relative temperature changes determined using only the slope of the calibration for Group I still provide unrealistic temperature changes (for U^K_{37} = 0.0219T the temperature range is 26.9°C). The slopes determined by D'Andrea et al. (2016) for Group III (U^K_{37} = 0.0447T) alkenone calibrations result in a smaller temperature range of 13.2°C and an estimated temperature change of 8°C from 250-350 CE to 1850-1950 CE.

Reviewer comment: Paragraph around line 230: here the authors try to relate their reconstructions to warming/cooling periods that are often referenced in the (North Atlantic) literature. I would stay clear from this and consider removing this section for a number of reasons. First, a string of recent studies has underlined just how spatio-temporally heterogeneous expression of these events is (see e.g. Werner et al. 2018 – COP, McKay et al. 2018 –GRL, and van der Bilt et al. 2019 – QSR). Secondly, most of these events are most clearly expressed in summer, while the authors argue that their record captures "cold season" conditions. Finally, and related to this, the perceived correspondence is tenuous at best as the authors also confirm by using wording like "roughly coincides" or "could be associated".

Response: Thank you for the suggestion, we will modify the text in section 3.3 to only discuss changes in our record. However, part of our goal is to compare our record with existing warm season reconstructions. Although there is considerable spatio-temporal variability in major warm and cold events, it is still useful to highlight anomalous time periods defined by previous warm season reconstructions in Iceland and other regions in the Northern Hemisphere. Therefore, we think it is important to keep the comparisons with warm season reconstructions in section 4.2.

Reviewer comment: Section 4.1: please restructure and tighten this paragraph. As the presented record only covers the past 2millennia, I think the current full Holocene focus is not the right way to frame things. Also, the authors allude to the so-called "Holocene temperature Conundrum" but don's state so (or explain it clearly). The way I see things, the main message here is that spring temperatures are (not entirely surprisingly) not driven by changes in summer insolation. I would contextualize/strengthen this by discussing other non-summer temperature reconstructions (which the authors already do to some extent), and argue why one would expect to see this "cold season" imprint in a maritime Arctic setting like Iceland, where it is known that many feedbacks may overprint any radiative signature, notably surface ocean currents, but also sea-ice feedbacks – in this respect, I recommend the authors to check Park et al. 2019 – Science Advances.

Response: Thank you for the suggestion, we will modify the text to explain the Holocene temperature conundrum and discuss the forcings that are relevant for the last 2,000 years. As mentioned by the reviewer, radiative forcings may be overprinted by sea-ice feedbacks and circulation changes, however as we will fully discuss in the modified manuscript, this is not necessarily the case.

With regards to sea-ice feedbacks (Park et al., 2019), sea ice normally only occurs off the coast of northern Iceland, and therefore has a stronger influence on climate in northern

Iceland relative to southern Iceland (Ogilvie, 1984; Ogilvie & Jónsson, 2001; Hanna et al., 2004). This observation is consistent with results from the study on mid-Holocene temperature changes in response to sea ice loss by Park et al. (2019): note the significantly lower SST response to Arctic sea ice loss (Fig. 3d) along the southern coast of Iceland (0.2 K) relative to northern Iceland (0.8 K) in the results from the mid-Holocene simulation. We will modify section 4.1 to discuss these points in more detail.

As mentioned by the reviewer, the close proximity of VGHV to the coast means that air temperatures at VGHV are also influenced by changes in sea surface temperatures (SSTs; Hanna et al., 2006). In particular, the Irminger Current, a branch of the northward moving warm waters of the North Atlantic, is advected clockwise along the southern and western coast of Iceland (e.g. Daniault et al., 2016). However, reconstructions of subpolar North Atlantic Current SSTs show diverging trends over the last 2,000 years with varying degrees of centennial to millennial variability, most likely reflecting differences in proxy seasonality (see Moffa-Sánchez et al., 2019). This makes it difficult to assess how SSTs have contributed to changes in winter and spring temperatures at VGHV, but as rightly stated by the reviewer, should not be ruled out. We will modify discussion section 4.1 to highlight the points we just discussed.

Reviewer comment: Finally, as the authors point out in section 2.4, Iceland receives little sunlight during winter: I therefore recommend them to plot early spring insolation in Fig. 6a instead of winter + spring insolation.

Response: We will modify Fig. 6a to plot spring and winter insolation separately.

Reviewer comment: Section 4.2: the authors (partly) attribute higher-frequency changes to shifts in regional climate dynamics, notably the NAO. When doing so, it would be most helpful to provide a contextual understanding of this complex system on Iceland – what happens to the different components of the regional climate system during (shifts between) positive/ negative NAO phases. Now, it oft feels as if this discussion is shoehorned into an NAO mould using a hotchpotch of sources. Also, respect the sampling and chronological resolution of this dataset: I don‘t think it warrants attribution to multi-annual forcing mechanisms.

Response: Thank you for the suggestion. We will update the text in section 4.2 to discuss forcings that are important on multi-decadal timescales and how they influence the regional climate of Iceland, particularly during the winter and spring season.

As discussed below and as we will explain in the modified text, it is hypothesized that low frequency changes in instrumental and paleoclimate archives from the North Atlantic region are driven by variability in the NAO (e.g. Hurrell, 1995; Pinto & Raible, 2012; Ortega et al., 2015). A recent study demonstrated that NAO variability on interannual to decadal timescales is most likely dominated by meridional shifts in the jet stream and storms tracks, whereas on multi-decadal timescales NAO variability is associated with changes in the speed and strength of the storm tracks (Woolings et al., 2015). On multi-decadal timescales studies have also linked variability in the NAO to changes in sea ice (Delworth et al., 2016), the Atlantic meridional overturning circulation (Delworth et al., 2016), and the Atlantic Multi-decadal Variability (Omrani et al., 2014, 2016; Peings & Magnusdottir, 2014).

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