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Interactive comment on "Cold season warming in the North Atlantic during the last 2000 years: Evidence from Southwest Iceland" *by* Nora Richter et al.

Anonymous Referee #1

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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.

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.

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).

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 ten-

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uous at best as the authors also confirm by using wording like “roughly coincides” or “could be associated”.

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. 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.

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.

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