

## ***Interactive comment on “Seasonal reconstructions coupling ice core data and an isotope enabled climate model – implications of seasonality, climate modes and selection of proxy data” by Jesper Sjolte et al.***

**Anonymous Referee #2**

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The manuscript by Sjolte et al. investigates a new innovation in the rapidly developing field of paleoclimate data assimilation. Specifically, they investigate the potential of reconstructing seasonal fields using subannually resolved ice-core (and to a lesser extent, tree ring) data from the North Atlantic. The manuscript is well-written, well-illustrated and generally well-organized, and the results are interesting, and suited to Climate of the Past. I do however, have a few concerns and suggested additions to the manuscript that I'd like to see addressed.

### **Major Issues**

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In general, in my opinion, the primary weakness of the manuscript is that the exploration of the reanalyses is rather limited. For example, in the authors subdivide the ice cores into a group of 8 that extends from 1241-1970 and a larger group that is shorter (1777-1970). However the reconstructions are only analyzed in the context of instrumental data. No results from prior to 1850 are shown in the manuscript or supplement, except for figure S3, which is specifically focused on the tree ring sites. In evaluating this technical approach, it is important for readers to be able to see how the longer term variability compares to other reconstructions from the region, and to consider and discuss how the seasonal assimilations affect long-term variability, and the potential climatic implications of that. Given that this approach creates a field reconstruction; these results could be compared to regional temperature reconstructions, NAO reconstructions, and more, and give the readers a better sense of how this approach compares with previous efforts.

At present, the evaluation of the results is restricted to spatial comparison of the first three PCs with instrumental data, temporal comparison of the same thing. I was glad to see SSTs averaged and compared to instrumental data, but feel like the comparison was ultimately very limited.

The other major weakness of the manuscript, that I believe should be able to address, was the representation of uncertainty. The methodological approach to uncertainty quantification; an ensemble based approach, is reasonable. I was disappointed however that the results were not presented in the manuscript. Every figure in the manuscript, except for the first two, could, and should, have uncertainty ranges (like 95

### **Additional issues/notes**

I'm a little confused about how the analog matching is working, based on figure 1. Specifically, are any adjustments made to the model-output before calculating the EOFs of  $\delta^{18}\text{O}$ ? If not, I'm confused about how there is such fine spatial structure in the model  $\delta^{18}\text{O}$ , given that it has 3.5 degree grid cells. In fact, I think it would be helpful

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to see the outlines of the gridcells on the lower half of Figure 1. Maybe there's enough resolution there, but I found it confusing. I'm also pretty surprised about how comparable the modeled and observed  $\delta^{18}\text{O}$  EOFs are, they're nearly identical. I'm not particularly familiar with this region and proxy, but model-proxy EOF comparisons this similar are exceptionally rare, unless one was forced/derived from the other, and I'd be interested to learn more about this.

Here's a suggestion that might be beyond the scope of this manuscript, but that I think is interesting: have you considered trying to assimilate different proxies for different seasons, but for the same assimilation? It would be really interesting to see what an annual reconstruction looks like where tree rings were assimilated for summer, while ice cores were simultaneously assimilated for winter – i.e., do the analog matching differently for each season but find the years that match both optimally.

**Minor issues:**

Line 7. “Reconstructs” should be “Reconstructions” 32: 18-O should have the standard superscript formatting 85: “extend” should be “extent” 328: “depended” should be “dependent” 385: “particularly” should be “particular”

Figure 7: Add some additional labels to the panels to help differentiate. It took me awhile to figure out why c and d were separated.

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-136>, 2019.