Review of The Temporal Phasing of Rapid Dansgaard–Oeschger Warming Events Cannot Be Reliably Determined

The manuscript studies the time lags between climate parameters during transition in rapid warming events using Bayesian ramp fitting, and the associated uncertainties that the methods imply. They create analogue simulations of such transitions as represented in General Circulation Models (GCM) and proxy records (namely the North Grip ice core), and evaluate biases that the ramp fitting can induce in estimating the lags between the climatic parameters.

The study relies on sound statistical approaches that provide the authors with estimate of such biases of the same order of magnitude than the expected lags, suggesting that at the present stage, it might be difficult to evaluate what parameters describing the past climatic conditions changed first during a given DO event.

The manuscript is relatively well written and though it is quite technical on a possibly niche topic, I believe it fits really well within the scope of Climate of the Past. Some improvement on the structure could be beneficial to highlight the strength of the manuscript, but beyond that, I recommend the manuscript be published after minor corrections.

General comments:

The approaches described here, i.e. studying the impact of the Bayesian ramp fitting on the uncertainty of the estimation of lags between climatic parameters simulated by GCM or reconstructed from ice core records, is usually a discussion topic for paleoclimate studies. I think it is a perfectly valid study, and do not question the relevance of the publication, but highlight this point because I would say that the *results* of your study correspond to what would be the *discussion* of a paleo reconstruction of a DO event lag study: indeed, classical paleo papers would estimate the lags and in the discussion attempt to provide uncertainties or biases of their estimates. Here, I found the structure of the result section a bit confusing and would suggest the authors to reorganise it a bit keeping in mind what the results are. Specifically, to me, the actual results come in section 3.3., while sections 3.1 and 3.2 detail sensitivity tests on the parameters that were used for the synthetic transitions and the resolution.

Overall, there are implicit equivalences that are made between the study of the outputs of CCSM4 and the parameters reconstructed from the NGRIP ice core which might be not accurate. In Section 2.2, a list of what climatic parameters are usually associated with the proxies retrieved from the ice core record is provided, but it is mentioned that the one to one equivalence is not totally correct, for instance, $\delta^{18}O$ is influenced by both temperature and sea ice extent changes (Sime et al., 2019). Then, the way that Table 3 and Figures 4 and 5 are represented suggests that some parameters are equivalent with the way that they are represented either on the same line or with the same colour.

Finally, I'm a bit confused with the choice of the reference parameter for the lag estimate for CCSM4 and the NGRIP ice core. Indeed, I don't understand why in CCSM4 the lag is calculated against temperature, while NGRIP ice core, it is calculated against the calcium, which is referenced to be a proxy of the NAO. This might be something classically done here, but since amongst the four parameters used from NGRIP ice core, $\delta^{18}O$ would be the one the most closely related to temperature, it appears that the lag as presented here would have different meanings for models and paleoclimate reconstructions.

Specific comments:

Introduction: the introduction overall feels too detailed, to the point that I struggled to see what was the topic of the manuscript when I first read it. Line 58 says "Subsequent to this work…", suggesting that the overall timeline of the study of lags of climatic parameters for DO events are detailed, while an introduction probably just needs the most up-to-date information that is needed to understand the interest of the manuscript.

Line 44: “On the first line of evidence, Adolphi et al…”

It is not clear what line of evidence this is. The similar formulation line 53 also puzzled me.

Line 89: “which Vettoretti et al. (2022) have kindly provided.”

“Kindly” is not an appropriate word to use, especially considering that most journals now require open access to the data of a published paper.

Lines 124 – 125: “For in-depth discussion of the physical interpretation of these proxies, see Section 2 of Erhardt et al. (2019) and references therein.”

This could be personal style, but I believe that a manuscript should be stand-alone. The detailed discussion of the physical interpretation of the proxies is beyond the scope of this manuscript, but overall idea should be provided in the introduction or the methods.

Lines 128 – 129: “The aerosol concentrations were measured using Continuous Flow Analysis (CFA) at temporal resolution ranging from 2 years for the most recent period to 3 years for the oldest period”

Was anything measured for this manuscript? Is the fact that the data are measured via CFA relevant here? This sentence was very confusing, making me think that you measured aerosol concentrations within this manuscript. Overall, only the effective resolution of the data is important here. Considering that all this information is already provided in the table, I would remove this paragraph.

Line 138: “The addition of an AR(1) noise process makes the model probabilistic”

Where is the AR(1) noise added? Why is it not explicitly given in an equation which shows exactly what is added where?

Line 143: “This leads to improved agreement of the transition model with the analysed data (Figure 1).”

This is not clear. Is the improved agreement shown in the histogram below each figure? What do they mean? they have no label, no caption, and no explanation.

Lines 143 – 144: “Additionally, our extension of the method reduces the sensitivity of the transition timing to the search window, which is otherwise one of the drawbacks of this method (Capron et al., 2021).”

Also unclear what is meant here.

Line 157: The detailed description of each hypothesis is one line of text each. They should be included here, and not in the appendix.

Lines 160 – 161: “We conduct our analysis of the NGRIP ice core in the same manner. In this instance, we calculate time lags for the other three proxies relative to Ca”

Why calcium which you say represent the NAO (line 120), and not d18O which is closer to temperature? This would be more coherent compared to what is done with models?

Lines 227 – 229: “However, we have established that there is no unbiased means by which to estimate these transition durations, and so we could not guarantee that this would be any more accurate.”

Where was this established? And shouldn't be this line in discussion rather than in Results?

Figure 5: The matching colours between Figures 4 and 5 suggest equivalency between precipitation and Sodium, sea ice and d18O, and AMOC and thickness. The colours should be different in both figures if they are not supposed to represent equivalent parameters.