Additional comments to the revised manuscript and the authors' response to the referees' comments to the original manuscript "The Temporal Phasing of Rapid Dansgaard–Oeschger Warming Events Cannot Be Reliably Determined" by John Slattery et al.,

The suggested revisions represent improvements that cover most of my concerns. I provide a few additional comments below (in normal typeface following the authors' responses in bold), and in particular, I think the title should be revised to something closer connected to the conclusions. It is, for example, quite unclear what "reliably" means without the context provided in the text (which will hardly fit into the title): as discussed in the reviews and in the updated manuscript, the biases will often be too small to be of any practical importance unless data from many events are stacked or otherwise combined. Also, it's not clear what the mentioned phasing refers to (between events, between proxies …), and "Rapid" is not needed as Dansgaard–Oeschger events always are rapid.

Some suggestions to illustrate my point:
"Methodological biases hamper the detection of climatic leads and lags across Dansgaard–Oeschger events"
"Estimating biases during detection of leads and lags between climate elements/mechanisms across Dansgaard–Oeschger events"

Furthermore, for the NGRIP ice core proxies, our extended method finds slopes of comparable magnitude in the pre-ramp stadial and post-ramp interstadial. This will be made clear in our revised manuscript as all of the relevant parameters used to create the "analogous" synthetic parameters for the purpose of bias estimation will be listed in a table in the appendix. We most often find that the slope during the pre-ramp stadial is positive - that is to say in the same direction as the ramp itself. See also the mean parameters for each proxy (including slopes) shown on Additional Figure 2 (page 10). The proxies all show significant slopes, in the sense that these slopes lead to a significant bias, however we have not tested the statistical significance of these slopes in isolation.

Why not?
The observation that allowing a pre-ramp slope changes the ramp location is not surprising in itself. If the observed pre-ramp slopes are not significant, it's not clear that (or how often) the updated model is an improvement. In particular, for non-model data, it remains to be demonstrated that there are significant slopes in the stadials and that the updated model is indeed an improvement and not 'just' another model.

Capron et al. test 20 realisations of a synthetic ramp, and find no significant evidence of a bias in either the transition midpoint or duration. Neither of these are directly comparable to the transition onset time, which we focus on as we feel it is more physically meaningful when trying to understand the progression of DO events. Nonetheless, if there were a bias in the onset time then Capron et al. would surely have seen this reflected in either the midpoint or duration, and they do not in fact see this. As the referee notes, the uncertainties shown in Supplementary Figure 2 of Capron et al. are large, and so could include a bias of a few years. Even so, the test conducted by Capron et al. would seem to rule out decadal-scale bias of the kind that we find in our study, at least for this particular combination of transition shape and noise. We would therefore suggest that the transitions tested by Capron et al. happen to lie in a region of parameter space for which

the bias is small, even at high levels of noise. When using the original Erhardt et al. implementation we find very little bias for synthetic transitions with no pre- or post-ramp slopes (see Additional Figure 2 on page 10 of this response). …

I think this is a likely explanation. I guess it also means that the bias problem is expected to be relatively small for most of the data sets of transitions derived from ice cores.

… Although the shape of the deterministic ramps used by Capron et al. is not made clear, it is likely that they are flat before and after the ramp. …

Indeed, this is the case, and this should be clear from Capron et al.: "i.e., a linear change in the raw or logarithmically-transformed data between two stable states"

…Instead, we demonstrate that their finding depends strongly on the unstated assumption that there are no pre- or post-ramp slopes. …

Citing Capron et al.: "The assumption that the transitions are adequately described by a linear change from one stable state to another is not trivial and has been challenged previously, but neither our observations nor the current understanding of the nature of the transitions justifies employing a model with more degrees of freedom."

The authors are very welcome to challenge this assumption, but it is hardly unstated.

Line-by-line comments:

- Line 1 and throughout: Hyphens should be used in "sea-ice extent", "ice-core records" and other similar compound adjectives, but not when "ice core", "sea ice" etc. appear as nouns.
- Line 68. Revise grammar.
- Line 74-76 seems inconsistent with line 12.
- Line 84: Models provide a lot more direct insights into the dynamics than ice-core proxies, but "they provide complete information" seems like an overstatement.
- Line 109: "appears to depends" .. no s or just "depends" … the paper details the event duration's dependence on CO2 quite explicitly.
- Line 109: The "chosen range" range does not make sense. The model oscillates with a range of CO2 values (which is not chosen), but the range of CO2 investigated is broader than this,
- Line 174: Missing "the"
- Caption fig. 1: The last sentence does not apply to a) and d)
- Line 191: in terms in terms
- Line 243: Rather "synthetic data series"?
- Line 279-280: yes, but especially for the Greenland stadial slope, the range of values used in the tests is much larger than the values observed in data, so the effect may look more dramatic than what is realistic.
- Line 307: Challenging
- Line 424: Suggestion: One important caveat is that the bias we have identified is generally fairly small relative to the timing uncertainty of individual proxies across single DO events, …
- Line 427: Suggestion: may involve
- Line 433: It seems unlikely that 20 synthetic data series with different noise realizations would not show any bias if it indeed was a problem. A more likely explanation is given in the authors' comments: "We would therefore suggest that the transitions tested by Capron et al. happen to lie in a region of parameter space for which the bias is small, even at high levels of noise."

- Line 473-477: It would be fair to mention around here – or elsewhere in the conclusion - that (significant) slopes are not always present in the data and that the original model outperforms the extended model in the absence of slopes in the data.

Sune Olander Rasmussen, June 19th, 2024.

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