Second round of review of Willeit and Ganopolski's "Generalized stability landscape of the Atlantic Meridional Overturning Circulation" by Yvan Romé, University of Leeds

General comments

Firstly, I would like to thank the authors for their fast and detailed response to the previous review. The authors addressed the comments in detail, and the manuscript was modified in depth to answer them.

The most recent version of the manuscript improves on the previous one and satisfactorily answers the main concerns raised during the previous round of reviews. In particular, the text is clearer, more precise, and more nuanced.

Regarding the previous comments:

- The authors' comment about the scope of the abstract is convincing, and the new version of the abstract is a good summary of the work conducted in this study.
- The introduction was significantly improved. It answered thoroughly my previous comments, and I find the new version succinct and impactful. I understand the authors' response regarding the freshwater forcing and millennial-scale literature. Shifting the narrative to the limitations of previous hysteresis studies is an elegant way to avoid going through the extensive references on freshwater hosing simulations, which is, I agree, not entirely relevant to the paper.
- The new version of the paper does an excellent job of highlighting the novelty of the FWF *x* CO_2 AMOC stability diagram. The distinction between the previous studies and this method is also more transparent.
- I appreciate that the authors discussed the potential occurrence of millennial-scale variability in Figure 1 around 0.05 Sv. I have additional comments about the interpretation of this hysteresis cycle, but they can be addressed as specific comments.
- The definition of modes is better defined in the manuscript and is rigorous. I believe, however, that this should be introduced at the same time as the modes in the manuscript. In Sections 2 and 3, and Figures 1 and 2, it sounds like the AMOC modes are only defined by the strength of the AMOC, which makes the identification of the modes vague when it is actually done precisely. This is particularly relevant in Figure 2 where the transition between the modern and strong AMOC modes is not evident on the AMOC index alone. As argued in my previous review, an additional Figure/panel with MLD time series in the different regions for

the hysteresis simulations could be a good way to illustrate this method. This could be in Supplementary information.

- The additional sentences about the discussion of the freshwater fluxes make the argument more precise and nuanced.
- The stages of the construction of the stability landscape construction are more apparent. However, I think this may still be missing a short justification for the chosen design. For instance, why map the OFF mode only by going from high FWF to low FWF, when both directions matter in a hysteresis cycle? This makes sense when referring back to Figures 1 and 2, but a couple of sentences about this would improve the fluidity of the reading.

Although I do not have any major modifications to suggest, there are three more general revision points that I would like to raise:

- I think the manuscript is missing a clear definition of what the authors mean by stability. This is particularly important in the interpretation of the hysteresis figures. For instance, line 74 ("*The AMOC in the model is monostable under pre-industrial conditions")*: One could argue that this is not the case because both the overshoot and the modern AMOC branch exist at this point. Stability becomes more transparent in the slow FWF variation plot (0.005Sv/kyr), and it is not clear why the authors did not choose this experiment.
- I would also avoid the use of more or less stable, as it is not clear what this means (more or less potential modes accessible for a point in the phase space? Less likely transitions between multiple modes? Or even more or less stochastic variability"). For example in line 241 ("a CO2 increase drives the AMOC towards a stronger and more stable AMOC"): visually, this is true in Figure 2, but Figure 5 indicates that a CO2 increase from PI conditions means travelling from a monostable mode to two bistable modes, which, one could argue, is less stable. This confusion is also apparent in L219 ("Our AMOC stability landscape demonstrates why warm climates are generally stable and cold are unstable"): There again, different interpretations can arise from the reading of Figure 2 and Figure 5.
- Figure 5 is a great tool to map the theoretical states accessible at a point of the phase space. However, because these modes also depend on the system's history, they can contradict the result of the hysteresis experiments from Figures 1 and 2 and create confusion in the text. In addition to the two examples introduced in the previous point (L241 and L219), the argument in L182 ("an Off AMOC state can not be achieved by varying CO2 alone, but only through a large enough FWF") is in contradiction with Figure 5. While these assertions are not wrong, they should be

presented with more caution and nuance to capture the complexity of the tools presented here.

The suggested revisions remain secondary, and I recommend publishing the manuscript after minor revisions without a new round of review.

Specific comments

Abstract
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Introduction
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Results

- Figure 1 – In the same way as the dip around 0.05 Sv was discussed, I believe the overshoot should also be mentioned in the text.
- L100 – I understand the authors' point in my previous review, but I am still unsure if *wrong* is the right word to use here. Otherwise, one could argue that the 0.02 Sv/kyr experiment also gives a wrong illustration of the hysteresis cycle, as it produces different transitions from the slow experiment (0.005 Sv/kyr).
- L115 ("The warmer the climate the further north do the sites of deepwater formation shift, following the northward retreat of sea ice") - I find this sentence in contradiction with some of the following points of this paper, which is not what this figure shows. "A stronger AMOC is generally associated with a northward shift of the sites of deepwater formation shift, following the northward retreat of sea ice"?

Discussion

- L214 ("for CO_2 below ∼250 ppm or FWF in the latitudinal belt 50–70∘N above ∼0.05 Sv") and L215 ("for CO_2 above ∼350 ppm or FWF in the latitudinal belt 50–70∘N below ∼-0.2 Sv") - I think this is oversimplifying the complex landscape presented in Figure 5. I would be more precise or remove these two assertions.
- L219 (". The fact that the AMOC is monostable under pre-industrial-like conditions explains why the AMOC always recovered at the end of glacial terminations, after

temporary shutdowns induced by the freshwater input (~0.1 Sv) from rapidly melting ice sheets") - I think this sentence needs more nuance: what do the authors mean by pre-industrial-like conditions, there needs to be a reference about CO2 concentrations during the previous interglacial, and I do not think that this explains the recovery of the AMOC during glacial termination given the transient nature of this phenomenon. Nonetheless, this is indeed a convincing argument in favour of it, and the discussion about the Pliocene is a good addition to the paper.

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Technical corrections

The technical corrections provided in the new version are satisfactory. I only have three minor points to raise:

- L51-53 – I am not sure I understand this sentence. Does this mean changes in CO2 and changes in surface ocean freshwater balance due to modifications in climate and land ice volume? Please ignore if this is not what was implied.
- L103 (and Appendix A2) - It may be clearer to write 20ppm/kyr instead of 2%/kyr (0.002%/yr)? Or do is it increasing the CO2 concentration by 0.002 % every year (CO2(n+1) = CO2(n)*1.002) ?
- L200 ("with a cooling of ~15◦C") - cooling by up to …