General Comments (on the revised version)

van Westen *et al.* have generally well addressed the comments in my previous review. In particular, the methods section is significantly clearer (although it could be streamlined). Moreover, I appreciate the addition of section 4.2. However, I still think the manuscript would benefit from more clarity in the language used, since many descriptions and statements are still very convoluted and/or conflicting. Overall, I think these would be simplifications or reductions of what is currently written. Lastly, I think it is important that the authors take more care to distinguish the 'AMOC-collapse induced' and 'AMOC-induced' effects on DSL, since these are very different. This is particularly relevant in the conclusions. I would recommend publication once these have been addressed. All line comments are relative to those in the tracked change version.

Specific Comments (on the revised version)

L19: For broader framing in the introduction (or elsewhere), the authors may want to cite this recent study: K. Seeger, P. S. J. Minderhoud, Sea level much higher than assumed in most coastal hazard assessments. *Nature*, (2026). https://doi.org/10.1038/s41586-026-10196-1

L42: It would be good to cite: W. Liu, S.-P. Xie, Z. Liu, J. Zhu, Overlooked possibility of a collapsed Atlantic Meridional Overturning Circulation in warming climate. *Science Advances*, **3**, e1601666 (2017).

L61: I think the methods section is generally much better organised. It might be helpful to have the advantages and disadvantages and what is possible with each experiment put more succinctly in a table, rather than introducing and comparing each one in the text (which is currently very convoluted).

L88: By 'cancelled', do the authors mean to say the effects of F_H are equal?

L98 – L103: As they're currently written, these lines are quite vague and confuses the messaging. I'm not sure if they're necessary. I will leave it up to the editor/authors to keep or delete this.

L158 – L160: As it is written, this appears to conflict with the linear approximation in d(DSL)/d(AMOC). Does consistent just mean monotonic?

L203 – L210: I still don't follow the arguments here. Are the authors trying to say that, through the equation of state, the haline contribution to density varies approximately linearly with AMOC through the AMOC weakening/collapse phase once this has been integrated over a region? Is there a temperature contribution? (see comment for L158-L160).

L491: I think the authors need to take more care here, and in similar places through the manuscript – these high values only apply for when the AMOC is collapsing and are significantly less if the AMOC were to only weaken, as most CMIP models suggest under realistic forcing scenarios. The introduction presents somewhat conflicting expectations of the AMOC's future evolution (*e.g.*, compare L29-30 with L39-40), which, when taken with the conclusions, leaves the reader uncertain as to whether the 'AMOC-induced' changes are really that important for contemporary climate change (say up to 2100). Ultimately, these high values need to be properly framed as AMOC-collapse driven, following the manuscript's title, otherwise the results are overstated.

Technical Points (on the revised version)

L5-7: This should probably be two separate sentences.

L63: Maybe just “Fully-coupled climate model simulations”?

L104: Maybe just “Ocean-only simulations”?

L145: The authors switch between ‘zeta’ and ‘DSL’ – perhaps just stick to one?