

General comments

We thank the authors for their response and for incorporating most of the feedback. We believe the manuscript has been substantially improved. However, we still have some comments on the structure of the results section and the general writing. With regards to the writing, the authors are advised to carefully check the grammar and for missing verbs in sentences, and to rephrase when necessary. Please see detailed comments about the content below. Line numbers refer to the manuscript version (not to the tracked changes version).

Main comments

The structure of the results section could be improved by placing the section about the warming thresholds (CDFs) in a more suitable place. The methodology is already used in Figure 3 and Figure 10, and should therefore be described before being used. As suggested before, the CDF methodology should be placed in the Methods section, but at least before the CDFs are used for the first time in the Results section. Since the warming thresholds for each system are already displayed in Figure 3, it could be an option to integrate the results of section 3.10 in this first part of the results. This would place the CDFs in the appropriate context and a clearer connection between figures 3 and 17 & 18 can be made.

In the response to the reviewers, the authors responded that they made changes to the conclusion. However, this change was not implemented in the conclusion. The proposed changes by the authors were only placed earlier in the discussion. Please rephrase the final conclusion to appropriately reflect the conclusion of the assessment.

Specific comments

Line 136-139. The last sentence of this paragraph does not add much to the methodology and could be removed; the computational complexity argument is not very relevant here. If the authors want to keep this argument, please rephrase it because the sentence is somewhat unclear.

Lines 180 – 184. The specific discussion about the monsoons is not necessary here, since it is already stated that this catalogue does not focus on atmosphere variables. The first part and the last sentence of this paragraph are however relevant.

Figure 10 is only referenced once in the text, in the last sentence of section 3.4, and once in the caption of figure 11. Could the authors reference this figure earlier in the section and put the figure into the context of the section?

With Figure 11, the authors added an explicit comparison with McKay, which is a valuable addition. However, we suggest that the temperature thresholds should be displayed differently. By taking the 0 and 100 percentiles of the CDFs of maximal change, you obtain a very large spread in expected thresholds. This comparison raises two issues that should be addressed:

- First, in McKay, the minimum and maximum are estimated through literature/expert assessment, meaning they have an error and should likely be taken as an estimated interval where a collapse is somewhat likely. By taking the 0 and 100 percentiles of the CDF, the authors effectively disregard this error on the McKay estimation. To address this, we recommend using e.g. a boxplot, violinplot or scatterplot for more nuanced comparison.

- Second, the authors analyze the full CDF of maximal change, while McKay looks at the likelihood range of a threshold. These are fundamentally different statistics: the uncertainty of the temperature threshold location within each simulation versus the uncertainty of an AMOC collapse occurring at a given temperature. Thus, it would maybe be more appropriate to use e.g. the median/mean/maximum slope of the CDFs as the basis for Figure 11. In any case, it requires an improved argumentation.
- Since the authors added a valuable comparison between this paper and McKay for the AMOC, the authors are encouraged to extend Table 1 by adding this comparison for all the systems (for those that were also assessed by McKay). The authors should choose a suitable error on their estimate depending on how they address the above two points.

Paragraph at line 395. What do the authors mean with the first sentence (“... urging for adjusting the burning embers in these reviews.”)? The authors claim here that there is a much earlier onset of collapse based on the analysis in figure 11. For this, see the comments above. Because of taking the full range of the CDF, this methodology will tend to estimate wider ranges than the other reviews. This is not because the collapse actually has to occur at much lower temperatures.

The causality mentioned at the end of the paragraph (lines 397-398) is not fully supported. In the mixed layer section (section 3.2 & 3.3), the order of events was reversed, where it is argued that freshening of the subpolar gyre is brought about by declining AMOC (line 334). In this paragraph (lines 397-398) it is said that a SPG collapse triggers an AMOC collapse which cannot be proven through the results shown. It is not given that a SPG collapse automatically leads to an AMOC collapse (models can show a stop of convection in the labrador sea while still having an active AMOC). This point should also be addressed more carefully in the discussion at line 584.

In the discussion, it is stated that the change in method is unlikely to explain the increased number of SNS (line 562). Could the authors add what the alternative/more likely explanation is (CMIP5/CMIP6 model differences/updated scenarios?)?

The mechanisms stated in the paragraph at lines 605 to 612 need references.

The reference to Portmann (... Line 905) is inaccessible since no preprint is available. Please remove this reference unless it is available at the time of publishing.

Technical corrections

- Line 261: missing verb, what happens to the mixed layer depth in this model?
- Line 343: reference to figure 7 incorrectly formatted
- Line 544: substitute “appears” with “is” since the statement is a fact.
- Last two sentences of section 2.1 should be swapped.
- The colours of the contourlines in Figure A2 are too similar, making it hard to distinguish them.