Review of Boot et al.: “Potential effect of the marine carbon cycle on the multiple equilibria window of the Atlantic Meridional Overturning Circulation”

This paper studies the width of the range of existence of multiple stable equilibria of the AMOC, as a function of different coupling mechanisms between a box-model for ocean circulation and carbon-cycle model studying the relevant ODEs with the continuation software AUTO. Both models have been previously studied and validated in a range of publications. The methods and the results are innovative and relevant for understanding AMOC stability and tipping in a range of climate states, they allow to identify and discuss relevant mechanisms and they certainly demonstrate that, in principle, a feedback between AMOC and carbon cycle is possible. Still, additional work is required, in my opinion, to organize in a clearer and more logical way the presentation of both the methods and the results.

In particular:

- The AMOC box model is presented in section 2.1 with 5 boxes but these are then extended to 7 (with addition of two boxes for the Indo- Pacific) in section 2.3.

- The coupled model is further modified when discussing the solution method in section 2.4, dropping the deep Atlantic box and substituting it with a global conservation constraint. This further change to the model structure occurs after having already discussed various additional coupling mechanisms in section 2.3 (which instead are fundamental for the further discussion of the results in section 3). An additional observation is that while the individual components (AMOC box model and CC model) have been previously applied in the literature, with these modifications we are now talking about quite different models, to what extent are these now comparable to the ‘full’ version of the components ?

- The different couplings (identified with BIO, E_s, FCA, CS_{LO}, CS_{HI} by the authors) are mostly introduced in section 2, but FCA is described later, in the results section 3.1 instead. In general it would be good to introduce these labels close to the equations or maybe add equation numbers in table 1.

- The couplings presented in the main text (additional ones are introduced in the appendix) appear a bit like mixed bag of random choices. Some additional discussion on why these should be considered important and relevant couplings (also with reference to the literature) would be recommended. Which feedbacks could be expected associated with these couplings?

- When the SST dependence on atmospheric CO2 concentrations is introduced, with a simple model of climate sensitivity, it would be good to remind the reader what role SST plays in the model equations, which processes it does control.

- Actually a similar observation is valid also for other couplings: the rain ratio coupling in eq. 4) could be accompanied by a short reminder on its role in the biogeochemical cycle (or at least just repeat the description from line 83)

- L217-210: It is said that when the BIO coupling is not used, then PO_4 concentrations become negative in the surface ocean under a collapsed AMOC regime. This is not shown in any plot (not even in the appendix) and in general this sounds quite ominous: negative concentrations? Is this a numerical issue? Which mechanism leads to the drop in concentrations if a fixed biological export production in the surface boxes is used. If this is so, why is the BIO coupling actually considered an option and not integrated permanently in the model?
- Table 1 is introduced in line 237, after already on the previous page the impact of different couplings has been discussed.

Other questions are:

- To what extent are these results sensitive to particular modelling choices (such as for example the depth of the boxes?)

- The relationship between E_s and atmospheric CO2 concentrations derived from CMIP6 models and described in eq. 8 could also be interpreted as a function of temperature, so should its use not be linked also to the activation of the climate sensitivity feedback?

- L228 and onwards: the total carbon content in the ocean+atmosphere system is kept fixed. I believe that some additional explanation on this hypothesis is due. What about carbon in terrestrial vegetation and soil carbon ?

- L237 onwards: there is no reference to the effect of introducing the E_s coupling compared to the BIO case alone.

- Why were only these 'incremental' combinations of couplings explored? Is there a reason why only certain combinations should be used? I understand that considering all combinations might be confusing but maybe a short comment would be good.

- L239: I might have missed it, but is there an explanation why the FCA coupling increases atmospheric CO2 ?

- The fact mentioned on line 255 that the on-branch becomes unstable before reaching the saddle-node bifurcation (due to a Hopf bifurcation):
  1) is this an hypothesis or was it confirmed by AUTO?
  2) Please clarify somewhere if the MEW is defined as the range between the saddle-node bifurcations or between the left saddle-node of the off-branch and the hopf bifurcation on the on-branch.

- The top axis of Figure 4 reports CO2 values between about 50 and 750 with E_s varying between 0.25 and 0.50. Compared with fig 2c this does not look like the same fit (in that figure CO2 between 400 and 1200 has E_s between 0.4 and 0.5).

- The fact that changes in SST (as modelled by eq. 3) do not affect ocean circulation in the model is discussed in the conclusions but indeed this might be a major drawback. Particularly through arctic amplification feedbacks, changes in the mean state can be associated with important changes in the meridional gradient of temperatures. Maybe the discussion on this point in the conclusions could be expanded.

- I realize that this is outside the scope of this study, but processes linked to sea-ice represent a major element affecting the strength of the AMOC, a comment might be in order. This could also be linked with the missing dependence of AMOC on model temperature which the authors recognize in the discussion.

- In the conclusions it could be beneficial to add a short comparison of these results (in particular the identification of the most important mechanisms) with other studies, maybe based on proxy data or on modelling with more complex climate models.

Minor issues:

- Line 177: for reproducibility it would be better to list somewhere which 28 CMIP6 models were used, which ensemble members
- Line 301: “These clear and plausible mechanisms….” → Which mechanisms? The previous sentence is about the CMIP6 fit ….. probably this paragraph (or the previous one) is not in the right place.