Interactive comment on "AMOC-emulator M-AMOC1.0 for uncertainty assessment of future projections" *by* Pepijn Bakker and Andreas Schmittner

Anonymous Referee #3

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A review of "AMOC-emulator M-AMOC1.0 for uncertainty assessment of future projections"

I'm a physical oceanographer, not an expert in climate research, and not very familiar with the climate literature, especially in this line of statistical studies. I cannot, therefore, judge whether the authors adequately quote past studies or whether this study is novel. I just trust the authors in that respect.

1 Summary

This study develops a simple coupled atmosphere-ocean model ("AMOC-emulator") to predict the strength of AMOC. Some of the model parameters are tuned to fit GCM results. The emulator has some skill in predicting AMOC strength in GCM runs that were not used to tune the parameters.

I agree with the authors that such a simple model can be useful in exploring many climate scenarios, but I think there are two serious problems in this manuscript: 1) the model is dubious; 2) the tuning of parameters is careless (I think) and the validation of the result of the tuning isn't adequate.

In the following, I explain these two points as well as offer some general comments and numerous minor comments, some of which support my arguments on these major points and others, I hope, may be helpful in improving the manuscript.

2 The model formulation

2.1 On using a conceptual model

I don't understand why this kind of ad-hoc box model is preferred. One would resort to Stommel's box model if one knows almost nothing about AMOC. Stommel's model was just *conceptual*, whose sole purpose is to get very rough ideas on how AMOC might work, and is not designed for the kind of *quantitative* modeling that the present authors pursue.

Stommel's box model was presented in 1961, and we know a lot more today. I would think that a simple *dynamical* model like Gnanadesikan's (1999, Science vol. 283, pp. 2077–) is much better because even uncertain parameters are based on (that is,

constrained by) clearly-identified dynamics. In contrast, some of the present authors' "parameterizations" aren't adequately defendable; they are based on hand-waving arguments. For example, how can one defend the parameterization that the AMOC strength is proportional to the interspheric surface-density difference? We know a lot better than that.

Perhaps even better, the models of Schloesser et al. (2014, Prog. Oceanogr. vol. 120, pp. 154–) and McCreary et al. (2016, Prog. Oceanogr. vol. 123, pp. 46–) provide constraints among integral quantities, such as AMOC strength, thermocline depth, and meridional density difference, and hence can be utilized as a "box model". Those constrains are derived as solutions to dynamical equations rather than assumed on the basis of hand-waving arguments.

In short, I don't see any advantage today in utilizing an old conceptual model for quantitative prediction.

2.2 AMOC proportional to interhemispheric density difference?

The authors says that "the assumption that the meridional Atlantic density contrast between the North Atlantic and the South Atlantic is the first order driver of the AMOC" is debatable, but I think that's off the mark. The current wisdom is that the Southern-Ocean winds (and perhaps vertical diffusivity) are the first-order driver. They cite Butler et al. (2016) as the *other side* of the "debate" but Butler et al. do not argue that the surface meridional density gradient "drives" the AMOC. The just use density integrated twice in the vertical as a "diagnostic" of the AMOC.

It is clear from ocean GCM studies that the meridional density gradient is not the first-order driver of AMOC. When the sea-surface density is restored toward a prescribed profile in an ocean-only GCM and windstress is changed in the Southern Ocean, the AMOC strength changes roughly linearly to the windstress. See Toggweiler et

al. (1995, Dee-Sea Research vol. 42, pp.477–) and the series of studies that follow. This is evidence enough that the interhemispheric density difference does not drive AMOC.

Of course, this evidence is based on ocean-only models, and it is possible that the interhemispheric density difference is correlated with the AMOC strength through atmospheric feedbacks, but to use a one-to-one correspondence like (1) needs justification based on atmosphere-ocean coupled dynamics.

By the way, I found that Butler et al. (2016) still use the traditional hand-waving parameterization $\Delta p_x/L_x \propto \Delta p_y/L_y$. See Schloesser et al. (2012, Prog. Oceanogr. vol. 101, pp. 33–) for a better parameterization based a lot more on dynamics.

3 Tuning and validation

3.1 GCMs for tuning

Why aren't multiple coupled GCMs used to tune the parameters? Do the authors recommend that the AMOC-emulator be tuned differently for each model?

The emulator is based on equations that represent physical processes in the real world. Then, if at all possible, the parameters should be tuned on the basis of reality. Granted that there is not enough data for the deep ocean. Then the second best thing is the publicly-available collections of coupled GCM runs.

I think that studies have indicated that a multi-model ensemble is usually better than a single model to mimic reality. So, the tuned parameters would be more likely better if they are based on multiple models.

3.2 Variables for tuning

The variables (salinity, ocean temperature, etc.) of the emulator should be compared with those from the GCM. It is possible that the state of the emulator is very different from that of the GCM even when the AMOC strength m agrees.

On a more basic note, have the authors made sure that all the variables of the emulator take reasonable values? I don't think it would be okay if, say, salinity takes a value of -100 psu even if the value of m is reasonable!

If the atmosphere and ocean states aren't realistic, how can we trust the emulator?

One approach to cope with this problem would be to include other variables than AMOC strength in the cost function. Another approach would be to compare various variables between the runs of the tuned model and those of the GCMs (part of validation). I think both are necessary.

3.3 Models for validation

Moreover, I think the tuned emulator should be validated against another set of *different* models. Otherwise the validation isn't robust.

I also wonder if an ensemble of runs are necessary for the GCM (UVic) for tuning and validation. For example, there is only one run for each case in Figure 8, but doesn't the AMOC strength differ from realization to realization? I don't know how chaotic the GCM is (because it uses a low-degree-of-freedom atmospheric model), but isn't the reality more or less chaotic?

4 Forcing

I may be missing something, but it's not clear to me what forces the emulator. The solar flux S seems constant in time (Table 1), but then how is the increase in green-house gas represented?

If I understand it correctly, ϵ forces the model (equation 19) toward one particular GCM solution, but wouldn't it damp the emulator's variability? especially when the emulator is to simulate a state that is very different from the GCM state used for ϵ? Doesn't this amount to building the solution into the simulation?

5 Minor points: math notations

5.1 Arrays

The authors define boldface math symbols to mean "arrays", but I recommend avoiding this unconventional convention. For example, a multiplication of two "arrays" can mean several different things in conventional mathematics. Equation (13) includes the multiplication of the arrays S and α_p, which is meant to represent $(S_1\alpha_{p1}, S_2\alpha_{p2}, \ldots)$, which is hardly conventional. K and T_a have the same problem in (14). Also, equation (11) includes $1/z$, by which the authors mean $(1/z_1, 1/z_2, \ldots)$, but which is not widely used in math.

All these problems are usually solved by using indices: for example, (14) can be written as

$$H_{aj} = -CK_j\frac{\delta T_{aj}}{\delta y}, \quad j = 0, \ldots, 4.$$

14'(1)If too many subscripts become nuisance, we could use both super- and subscripts: H_j^{a2o}, for example.

5.2 Subscripts

I recommend using an upright font for multi-character math symbols such as "start" and "gcm"; or avoiding them. In particular, the subscript "it" looks as if it represented two subscripts i and t. I recommend using a single-character subscript, such as "ψ_{i-1}" or if you insist on multi-character subscript, you may want "$\psi_{\mathrm{it}-1}$" using an upright font.

6 Point by point comments

Some of the following comments support my arguments above, some raise other concerns, and others point out minor, mostly editorial, problems. I wrote many of them as I read the manuscript for the first time, and as a result, they include some redundancy. I leave them as they are, because they often reflect difficulties or problems the reader may encounter as she reads the text.

6.0.1   p. 1, l. 19:

"due to climate sensitivity, polar amplification, GIS melt and model dependent sensitivity of the AMOC . . . "—I'm confused. Doesn't "climate sensitivity" include all the remaining items in the list? Why is it listed in parallel with the rest?

6.0.2   p. 2, l. 5:

"(Rahmstorf and Willebrand, 1981)"—As the reference list indicates, this should probably be Rahmstorf and Willebrand (1995).

6.0.3   p. 2, ll. 5 & 31:

"the so-called Bjerknes feedback"—Probably this is because I'm not much versed in climate research, but isn't the "so-called Bjerknes feedback" restricted along the equator? A direct overturning circulation occurs connecting cooling in the eastern Pacific, say, and warming in the western Pacific only along the equator, where the Coriolis force vanishes, and the surface windstress associated with this zonal overturning circulation enhances the upwelling of sea water, which further lowers the sea-surface temperature in the eastern Pacific—a positive feedback, which is "the so-called Bjerknes feedback".

The authors cite Rahmstorf and Willebrand (1995) for "the so-called Bjerknes feedback", but Rahmstorf and Willebrand proposed a negative feedback due to heat transport within the atmosphere, I think.

6.0.4   p. 2, l. 7:

"tuning a numbrer of free parameters"—They aren't "free". They represent specific physical processes and hence must be ultimately determined by physics, even though it's in practice difficult to derive their values purely from physical principles.

6.0.5   p. 3, l. 12:

Why is F prescribed? I would expect it to change according to the state of the climate system. What do IPCC-class coupled GCMs say about the change in F under global warming, for example?

. . . but, later in the text, the authors say that F is related to the global atmospheric temperature (equation 12). So, it's not prescribed after all.

6.0.6 Equations (3)–(10):

What does this "δ" mean? Is it a typo for "∂"?

6.0.7 Equation (11):

State whether z's are fixed, and if so, give their values here or refer the reader to a table or something.

6.0.8 Equation (12):

ΔT_{glob} should be defined. (How is it computed from T_a?)

6.0.9 Equation (12):

Give F_0's their values here or refer the reader to a table or something.

6.0.10 Equation (13):

I may be mistaken, but it seems that the $T_a{}^4$ is the only nonlinear term. Doesn't it make sense if this term is linearized around a mean state?

6.0.11 Equation (13):

The solar flux S is a confusing notation. By the authors' own convention, $S = (S_0, S_1, S_2, S_3, S_4)$, which uses the same symbols as salinity.

6.0.12 Equation (13):

The solar flux S should be discussed right below equation (13). Does it depend on time? Table 1 suggests that it's constant in time but that should be stated explicitly. So, does the emulator solves only for annual averages?

6.0.13 Equation (13):

Define this ∇ precisely. (But I don't recommend this notation because a gradient of an array is a strange mathematical entity.)

6.0.14 Equation (14):

What does this "δ" mean? Is it a typo for "∂"?

6.0.15 Equation (14):

State that H_a is defined to vanish at the northern and southern ends of the northernmost and southernmost boxes. (I guess they are so defined, right?)

6.0.16 p. 5, l. 15:

I guess we need some discussion on other possible sets of tuning parameters. We have a vast range of possibilities. Then, how have we settled on these seven parameters? Have the authors tried other combinations of parameters?

6.0.17   p. 5, l. 15:

F and h are related by equation (12), and so cannot be determined independently. Moreover, if you tune F, you can forget about equation (12) and don't need to consider h.

6.0.18   Equation (15):

Why try to optimize m alone? It's conceivable that widely different states have similar m values. Because we have other variables like salinity, we could choose better sets of parameter values, if we include other variables in the cost function, couldn't we?

6.0.19   Equation (15):

I may well be mistaken, but it seems that the differential equations are linear in the tuning parameters and if so, the optimization problem on the new cost function

$$C' \equiv \sum \sum \left(\frac{\mathrm{d}m_{emu}}{\mathrm{d}t} - \frac{\mathrm{d}m_{gcm}}{\mathrm{d}t} \right)^2$$

is a quadratic function of the parameters and can be solved analytically, I think.

6.0.20   Equation (16):

The notation "$p_{start}(1-z)$" is confusing because it looks as if $p_{start}(z)$ were a function of z. Vectors customarily come after scalars, as in "$(1-z)p_{start}$"

6.0.21   Step 1:

I don't understand why we have to repeat this step. Why not choose values that are within the ranges in Table 2 in the first place? We can use a random variable whose PDF is uniform over the specified range for each parameter, can't we? I mean, if $(1-z)p_1$ is below the range, we can just use p_{1min} for the lower bound; that is, we can use $U(\max((1-z)p_1, p_{1min}), \min((1+z)p_1, p_{1max}))$ without repetition. The same argument holds for the last part of Step 3.

6.0.22   Equation (17):

I may be missing something, but shouldn't (16) and (17) be written in parallel forms? If we write $(1-z)p$ for (16), then we should write $(1-\psi_{it})p$ for (17). If we write $p - \psi_{it}p$ for (17), we should write $p - zp$ for (16). For a moment, I was confused with (17).

6.0.23   p. 7, l. 16:

I think that efforts should be made to narrow the range of the parameter values. If parameters are widely different even though the cost function is similar, doesn't that suggest that the parameters aren't well tuned?

What about comparing variables other than m between the emulator and the GCM? Wouldn't that tell which parameter values are bad?

It seems that the authors have forgotten that there is only one reality.

6.0.24 p. 7, l. 22:

What is "RCP"? (I may have missed its definition given in the text.) Because how ϵ is determined is important, it may be helpful to give a bit more information here.

6.0.25 Equation (19):

Does the model really use the full time-series of T_{gcm}? Or is that a long-term mean? State clearly how is T_{gcm} defined.

If the emulator uses the full time-series, it may not be appropriate for other models or for other scenarios.

6.0.26 p. 8, l. 2:

"Note that the temperature forcing files need to be interpolated onto the temporal resolution used in the atmospheric component of the AMOC-emulator"—Awkward in several counts.

1. The interpolation draws the attention of the reader as if it were something noteworthy. *Perhaps the results are sensitive to the method of interpolation?* the reader would wonder.

2. Is the fact that the GCM data are saved in files noteworthy? (I mean, why mention the files at all?)

3. Despite this cautious tone, the interval at which the GCM data is saved is not indicated.

If the result is sensitive to the interpolation, give more details. If not, what about just saying, "The GCM variables are saved at an interval of XXX hours and interpolated on to the time steps of the AMOC-emulator", something along the lines.

6.0.27 p. 8, ll. 5–6:

A similar problem. If interpolation is so noteworthy, give more details. If it's not so big a deal, just say, "the GIS melt forcing is interpolated. . . ." instead of "Note that the GIS melt forcing needs to be interpolated. . . ."