

Interactive comment on “FORTE 2.0: a fast, parallel and flexible coupled climate model” by Adam T. Blaker et al.

Anonymous Referee #1

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The authors present an intermediate resolution coupled Ocean Atmosphere General Circulation Model that has been used previously but for which no proper model description is available. It is thus certainly worth to briefly describe the model and its mean state here. However, the authors fail in highlighting the strengths of the tool compared to similar intermediate resolution models and the description of the model and of its results are too superficial to be really useful. I thus consider that major modifications are required before the final publication of the manuscript.

1/ As it is presented, FORTE2.0 is an intermediate resolution model like many others. The fact that the model is relatively fast does not appear clearly as an advantage compared to similar models that should be more or less as fast if they are run at the same resolution. If I understand well, the tool seems to have two main advantages. First, the

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model can include or not the stratosphere. However, the stratospheric dynamics is not at all discussed. The minimum for me would be to present simulations with and without resolved stratosphere and see the impact of this choice on model result. 2/ It seems that the model is readily configurable, providing maybe more flexibility than other tools. This interesting aspect is mentioned but not developed enough to see if this is a real strength of the tool. The manuscript should thus insist much more on those potential strengths (and maybe on others).

2/ The model evaluation is very general. Many times it is mentioned that the results of the model are satisfactory or within the range of other models but the model performance itself is not really quantified. The observations are shown for some quantities for comparison with model results (some figures in section 4.2, figures 12-14) but not for many others (section 4.1, figure 14). This makes the evaluation harder to follow. Furthermore, it is not clear from the text if the simulation presented is from a 'standard configuration' of the model that may be used as a future reference or just an illustrative version that is not supposed to last and will not be used later. If the configuration presented is not a standard one, this strongly diminishes the interest of any diagnostic performed with this configuration and I would recommend that such a standard version is obtained before presenting it.

3/ The model has no interactive sea ice. This is a strong limitation compared to similar tools and this should be mentioned earlier (in the abstract for instance) as this may be an important element for potential users.

Specific comments

Page 1, lines 16- 21. The distinction between 'coarse resolution simplified models', 'intermediate resolution models' and the 'Earth Models of Intermediate Complexity (EMICs)' (introduced page 2, line 30) is to very clear to me. For instance, I would personally put ECBilt in the list of EMICs, and thus among the 'coarse resolution simplified models' rather than within the 'intermediate resolution models'.

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Even if it is always better to use the most up-to date datasets, I do not think that using relatively old ones like the climatology of Levitus and Boyer (1998) - Levitus et al. (1998) (page 2, line 15) or the heat transport of Trenberth and Caron (2001) (page 13, line 18) makes a big difference but stopping the analyses in 1997 for ENSO (page 16, line 11) may seem a bit too early to have a good evaluation.

Page 3, line 28. It is not clear if the variable drag coefficient is applied both over land and ocean.

Page 3, line 34. Not clear to me what is meant here by 'ice has melted' if there is no representation of sea ice

Page 4, line 9. What is the size of the 'polar island' ?

Page 11, Figure 3. What is EN3? Not sure it is defined.

Page 13, Lines 9-14. Please specify where convection occurs in the model. The wording 'not uncharacteristic of coarse resolution ocean models' is another example of a general sentence where more substantial, quantified information would be required.

Page 16, line 11. The evaluation of ENSO characteristics is based on a figure from a paper published in 1997 while it would be very easy to evaluate precisely the simulated Nino3.4 index compared to observed one.

Page 17, line 6. It is mentioned that 'Comparison of the corresponding principal component time-series (Fig. 12) suggests the presence of some higher frequency variability in observations that is not captured by FORTE 2.0'. This should be quantified both for the AO and the NAO.

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