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

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We would like to thank both reviewers for their time and effort in reviewing our manuscript. Their suggestions have helped to clarify several points and improve the description of FORTE2.0 in many areas. In the response below, we duplicate the reviewers comments and respond point by point in line.

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Response to RC1

We thank the reviewer for their time and effort in reviewing our 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 com-

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pared 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 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).

FORTE 2.0's performance, and the use of the term 'Fast' is in part to retain the connection to earlier studies which used FORTE, and in part due to the choice of the components, which when FORTE was originally coupled were indeed 'fast'. The IGCM (Forster et al., 2000, Joshi et al., 2015) is a spectral atmosphere, and spectral models are well-known to integrate much faster than grid-point models. At the time it was written, MOMA was shown to yield significantly faster performance on array processors than the GFDL MOM code upon which it was based (Webb 1995).

To avoid repeating the description of IGCM4 (Joshi et al 2015) we referred the reader to that publication for details of the stratospheric dynamics. We have expanded the description of the atmosphere and make it clear to the reader where they may find more information.

We have performed a second simulation using the L20 configuration, and have now integrated a comparison of the simulations with and without the stratosphere into the manuscript. This has resulted in many changes throughout the text and figures.

FORTE2.0 is readily configurable. We refer in our introduction to previous studies that have used FORTE in a variety of configurations to illustrate this point. It is difficult to quantify configurability, but we expand the text to give some more details on this element of FORTE2.0.

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.

Hardware and compiler differences aside, the code and configuration of FORTE that has been archived will reproduce the (now two) simulation(s) presented in this manuscript (using the L20 and L35 configurations of IGCM4). In that sense, this could be considered a 'standard configuration', and by archiving the code/configuration and publishing the resulting simulations it may be used as a future reference. As we state in the manuscript, we have not exhaustively calibrated the model, and 'better' climates almost certainly could be achieved. Furthermore, the potential applications of FORTE2.0 are wide-ranging, and we expect that many users will define their own 'standard configuration' adapted to suit their purposes. When considering the plots for the manuscript we tried to strike a balance between the number of figures/subpanels and what we think the readers would be interested to see. In some cases, such as precipitation, difference plots are harder to read/interpret than the quantity itself. In other cases, such as SAT, we agree that a difference plot could be informative. In the revised manuscript we will present difference plots between the SAT in both simulations and 20CR (Compo et al., 2011). In addition, all data and scripts for plotting the figures in the manuscript are provided as supplementary material, so it is possible to reproduce and compute differences from other models and observational datasets.

Compo et al., “The Twentieth Century Reanalysis Project” QJRMS, 37, 654, Part A, 2011, Pages 1-28

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.

We agree that, for some users, the lack of interactive sea ice may be a strong limitation. We have included in the abstract “At present FORTE 2.0 does not include interactive sea-ice, its effect is represented instead by a simple flux barrier.”.

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’.

We have moved ECBilt as you suggest.

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.

We use the Levitus and Boyer (1998) and Levitus et al. (1998) T and S climatologies for the initial state of the simulation. Their use is historical, and as the reviewer suggests their age is unlikely to make a big difference to the result. We now plot Gaussian distributions for both the L35 and L20 configurations, and for HadISST data extending from 1870-2019.

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

It is applied only over open ocean. We clarify this in the revised text. Land roughness is treated separately, and is based on land-cover (vegetation type).

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

This has been rephrased to say “...will not reduce until the temperature rises above freezing point and the flux barrier deactivates.”

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

The polar island is a single row of ocean cells, so the northern extent of the ocean is 88oN. This detail is now added to the text.

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

Thank you for spotting this. We had omitted the reference.

EN3 is a subsurface ocean T and S product from the UK Met Office which can be accessed here: https://www.metoffice.gov.uk/hadobs/en3/

Ingleby, B., and M. Huddleston, 2007: Quality control of ocean temperature and salinity profiles - historical and real-time data. Journal of Marine Systems, 65, 158-175 10.1016/j.jmarsys.2005.11.019

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.

Convection occurs in MOMA where d(rho)/dz < 0. We do not routinely output where this takes place, but the mixed layer depth is an indication of where deep convection occurs. Winter mixed layer depths in the southern Labrador Sea reach 2500 m in a few grid cells. Winter mixed layer depths south of the Denmark Strait, Iceland and the Faroe Bank Channel can reach 1000 m. Wintertime convection is too shallow in the Nordic Seas, with mixed layer depths reaching 125-150 m in the central and eastern

Nordic Seas. We have improved the text describing the regions of deep convection.

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.

We now compare the distributions of the Nino 3.4 SST index from both FORTE 2.0 simulations and an up-to-date time series from the gridded observation-based product HadISST. The distributions simulated by FORTE 2.0 are too narrow compared with observations, and extreme values are approximately 0.5C too small.

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.

We mention this because a visual inspection of Figures 13 b,d suggests that there is more year-to-year variability in the time series of the first principal component. It may be that the longer time series of the 20th Century Reanalysis (140 years vs the 100 years of FORTE2.0 that we present), and/or the higher spatial resolution (2 degrees for the 20th Century Reanalysis vs 2.8 degrees for FORTE2.0) are sufficient to explain this difference. We will investigate this and revise the text in the manuscript accordingly.

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Response to RC2

We thank Dmitry for his time and effort in reviewing our manuscript.

A general question before downloading the code: in which programming language is FORTE written?

FORTE is written in FORTRAN. This is an important point, and we have added a statement to the abstract to make this clear to the reader.

Page 3, lines 2 and 3: It sounds like two different grids are being used in the atmosphere. I would therefore rephrase to something like "A longitudinally regular and Gaussian in latitude grid with a grid spacing of _2.8_ is used for advection and diabatic processes."

This sentence has been reworded as suggested.

Page 3, line 31 and below: "FORTE 2.0 does not include dynamic sea-ice representation. Instead, sea ice is represented by a barrier: : :" Do I understand this correctly that there is no dynamic sea ice nor the sea ice itself but the flux barrier? I am curious about the process of flux computation in this place. How the values for temperatures and albedo to parameterize the presence of sea ice were chosen? At the end of the page the authors say "...until the ice has melted". Considering what is said above, is it the same as ... until the atmospheric temperature becomes > 271_K? I assume the restoring of SST below the sea ice a part of the ocean component? Maybe it is worth mentioning this since this chapter describes the atmospheric component.

Yes. There is no dynamic sea ice in FORTE2.0 at the moment. This is arguably the largest shortcoming in the current configuration, and top of the list of things we would like to address in the future. The albedo for sea ice typically ranges from 0.5 to 0.7. It is a tunable parameter in FORTE2.0. The simulations presented use a value of 0.6. The flux barrier becomes active once the sea surface temperature reaches 271K, and from this point IGCM4 computes the surface temperature in 'ice covered' regions. If the surface temperature continues to fall the albedo can increase further to mimic the effect of snow cover on sea ice. In reality, accumulation of snow cover on sea ice acts to increase the surface albedo to values as high as $\sim$0.9. Given the size of the grid cells in FORTE2.0 we opted for setting an upper bound of 0.8.

Page 4, line 13: How the topography was interpolated? Was it smoothed in between or not? conservatively?

The topography used for the simulations present and supplied in the repository is a

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bilinear interpolation of the HadCM3 bathymetry (Gordon et al., 2000). Due to the coarse grid this was followed by a manual process to adjust the widths of narrow channels. Note that with the Arakawa ‘B’ grid a single grid point channel does not permit flow, but diffusion of tracer quantities will occur.

Page 4, lines 18 to 20: Considering the model “biases” which are shown below in the paper I wonder whether the geometrical scaling of GM could improve the solution? Was there a run made without GM? Does it improve the SPG in the NA?

We have not implemented or tested a geometric scaling of GM, or run FORTE2.0 without GM. These are interesting suggestions, and we would be interested to explore these in future.

Page 5, lines 2 to 3: in the Table 1 the background vertical diffusivity is defined as a constant value but it is stability dependent here. Which mixing scheme is used above the background?

This was an error in Table 1. We have removed the vertical diffusivity quoted in the table. In correcting this we noted that two of the other quantities (isopycnal tracer diffusivity and the steep slope horizontal diffusivity) in the table did not reflect the values used in the simulations presented. We have corrected these also.

The background vertical diffusion is stability dependent following Gargett (1984), as we described in preceding paragraph.

Page 5, section 3: it is worth repeating that the pre-industrial atmospheric concentrations of CO2 were used.

This is now done in the revised manuscript.

Page 5, line 16: should there be the minus sign? The ocean warms initially but cools towards the end of the control run. Actually most of models simulate higher than observed ocean temperatures even under pre-industrial forcing (e.g. Griffies et al. 2011, doi:10.1175/2011JCLI3964.1; Lucarini and Ragone 2011).

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We meant to say +/- 0.2 W m-2. From around 600 years onwards the long-time mean is slightly negative, but here we intended to say that the even on short timescales the surface heat flux imbalance in a control simulation remains within +/- 0.2 W m-2.

Page 5, line 17: is the salinity trend caused by the use of the linear free surface (W_surf * SSS)?

This is a good suggestion. However, it is not the case. Taking into account the linear free surface the volume average salinity is of the order 1e-5 PSU higher after 2000 years. There is a small positive trend in the linear free surface, such that after 2000 years the global mean sea surface is 2.4e-7 m higher.

Page 6 line 8: the drop in AMOC happens abruptly. Do you have any idea of what has happened?

We believe the sudden drop is linked to the establishment of a fresh surface signature across the GIN and Irminger Seas (Figure 7f). The abrupt drop in AMOC has been a 'feature' of FORTE simulations, and it continues with FORTE2.0. As you note later, the common "cold bias" around Newfoundland is actually a warm anomaly in FORTE, and this extends into the Labrador Sea. This limits the dense water formation in FORTE2.0 to the Irminger and GIN seas. Hence the fresh anomaly that develops over these regions has a pronounced effect on the AMOC strength. We have added some text to explain this in the revised manuscript.

Page 8 line 5: Is there a link between low ACC and GM (or is it because of winds)?

There is a link between GM and ACC strength. Kuhlbrodt et al (2012) examine coupled models submitted to IPCC AR4, and show that there is greater sensitivity of the ACC strength to the value of k (GM thickness diffusion) than there is to the zonal wind stress. Our simulations use a value of k = 2000. Looking at Fig. 1b of Kuhlbrodt et al (2012) FORTE2.0 has a stronger ACC than other models that use k = 2000. It is closer in strength to the models using values of k = 700-1000.

We have expanded the text discussing the ACC to include discussion of the link between GM and ACC strength.

T. Kuhlbrodt, R.S. Smith, Z. Wang, J.M. Gregory "The influence of eddy parameterizations on the transport of the Antarctic Circumpolar Current in coupled climate models" Ocean Modelling 52-53 (2012) 1–8

Page 8 line 9: I wonder why only 25 years? Do you expect any change in results if a longer period is considered?

Analysis of the climate state, either from models or observations is typically done for periods of a few decades. The choice of 25 years was arbitrary, and we would not expect any substantial change if a different duration or period was chosen.

Page 10: A pre-industrial run is compared with the present day climatology. Bias seems to be not the proper word then. Maybe it is also worth mentioning that the Levitus climatology is being used as a metric only.

I see your point about the use of the word 'bias' in this case. We have reworded accordingly. Whilst addressing this point, we realized that 'Levitus climatology' should actually read 'EN3 climatology', as this is what is presented in Figure 7. We have rectified this.

Page 11, line 13: the commonly observed "cold bias" around Newfoundland is replaced by a warm anomaly instead. Something very different from most of the climate models is happening there. Same in SSH (page 13, line 5). Is it because of the winds? Does FORTE depict any MLD in the Labrador Sea or it is fully shifted towards high latitudes? Which role you expect GM to play in improving the NA SPG?

The extent of the subpolar gyre in this simulation is confined very much to the west. Correspondingly you can see the 10C and 14C contours in Fig. 7a extend further northwest than in EN3. The winds may play a factor in the structure seen here. FORTE2.0 does show some deep MLD in the Labrador Sea. We have monthly output for the final

25 years of the simulation, and have now added a figure of MLD for both the L35 and L20 configurations to the manuscript. In the period of monthly output we have, the L20 configuration shows stronger convection in the Labrador Sea and more generally across the high latitude N. Atlantic, whilst the L35 configuration depicts less extreme MLD that also occur further north into the Labrador Sea. As you alluded to earlier, a spatial/geometric scaling of GM would enable the user to choose GM that is more appropriate at both high and low latitudes, rather than seeking a compromise. This could improve the representation of the N. Atlantic subpolar gyre. Numerous studies, e.g. Decau and Myers (2005), Beismann and Redler (2003), have investigated the role of GM on N. Atlantic circulation and found positive results.

Deacu, D. and Myers, P.G. “Effect of a Variable Eddy Transfer Coefficient in an Eddy-Permitting Model of the Subpolar North Atlantic Ocean”, Journal of Physical Oceanography (2005) 35 (3): 289-307.

Beismann, J-O. and Redler, R. “Model simulations of CFC uptake in the North Atlantic Deep Water: Effects of parameterizations and grid resolution”, Journal of Geophysical Research, Vol. 108, C5, 3159

Page 13, line 17: Was the OHT computed through the meridional velocities or the atmospheric heat flux?

It was computed through the meridional velocities.

Section5: were the EOFs computed for global fields or only for shown areas?

We followed the method of Hurrell (1995) for the AO and NAO analysis. We now state in the revised text over which regions we compute our EOFs.

Page 17, line 19 (Summary): naming is incorrect: control simulation is only 25 years long and 2000 years simulation is attributed to as spin up throughout the text.

Thank you, this is now corrected.

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