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Interactive comment on "The Norwegian Earth System Model, NorESM1-M – Part 2: Climate response and scenario projections" *by* T. Iversen et al.

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We are grateful for this review, which include constructive remarks that significantly help to improve some weak points in the paper. We have addressed all of these minor issues, as detailed below.

1) Page 2, first paragraph of the introduction: the second sentence of this paragraph ("Explicit description [. . .] (Tjiputra et al., 2012)") sounds negative to me. I suggest to remove it here, and replace it with the following sentence at the end of the same paragraph: "Another version of NorESM1 called NorESM1-ME includes an interactive carbon cycle, and will be described in a coming paper (Tjiputra et al., 2012)".

We agree with this suggestion, and write:

Although carbon cycling is included in the ocean and land models of NorESM1-M, another version of NorESM1, called NorESM1-ME, is used to simulate the earth's climate with an interactive carbon cycle as described by Tjiputra et al. (2012).

2) On the introduction: from page 3 - line 6 to page 6 - line 2, the text is resembles more a model description or experimental setup than an introduction to what will be done in the paper. For clarity, only keep in your introduction its two first paragraphs, and the last one. The rest of the introduction (from page 3 - line 6 to page 6 - line 2) will consist in a "model and simulation description" section, following the introduction.

We have decided to follow this suggestion except for the name of the new section 2. Thus, the introduction is now considerably shorter, whilst the removed information is included in a new section 2 called The model and model simulations. The numbering of the following sections are changed accordingly.

3) Page 6, line 25: add "(following Andrews et al., 2012)" after "gross feedback factors", and remove the last sentence of this paragraph. Done

4) Replace "TRC" with "TCR" on: - page 8, line 27, 28 and 29 - page 9, line 27 - page 10, line 1

Done.

5) Page 10, line 24: add the reference to the volcanic sulfates dataset used here (or point to another companion paper or section).

This reference, (Ammann et al. 2003), was given in the introduction (now in Section 2), but is repeated here.

6) Page 12, line 29: give a reference for the non-linearity test, or give more details.

Since the issue of non-linearity is not the main point per se, the text may be confusing. Hence, we have replaced the part that begins with "In some cases random reinforcements. . .” and ends with “. . . A stronger proof requires multiple ensemble members.” with the following:

To some extent, random reinforcements or cancellations can be checked by adding the spatial response of each single forcing experiment, and compare this sum with the response of a single experiment that employs the sum of the two forcing components. In areas where the two fields differ considerably, chaotic internal variations may dominate over systematic mutual reinforcements or cancellations. Since random patterns in the two single-forcing experiments also may behave similarly by chance, however, a more confident conclusion requires multiple ensemble members.

7) Figure 4: adding the maps for the natural forcing only experiment would provide some very interesting comparison with the two other simulations. Also precise what are the white areas on the maps (name of the test and significance level). We have extended Figure 4 to include maps for the natural forcing experiment only. At the same time we have changed all the maps to be based on annual data rather than monthly. This makes the estimate of the 5% significance level more reliable. There are tiny changes in the figures only. The name of the significance test is two-sided Student t-test, and the significance level is 5%. This info is now included in the figure legend.

The discussion around Figure 4 takes into account the two added maps in a new paragraph as follows:

Fig. 4 also shows that there are only small and patchy regional changes in the run with only natural forcing included. Even though they many places are diagnosed as significant at 5% level compared to the unforced variance of annually averaged values, the trends appear unsystematic, and positive and negative values are approximately equally likely. This contrasts the systematic trends in the runs with GHG-forcing only and aerosol-forcing only which tend to have opposite signs.

8) Legend of Figure 5, line 9: replace “insignificant” with “not significant”; precise the name of the statistical test.

Done

9) Maps of Figure 5, linked with last paragraph of page 12: your point for this diagnostic is to show that a large part of the forced variability in the historical run can be explained by the sum of the responses to GHG and aerosols forcings. Then it has to be the first thing that we want to see on those maps. Hatch the areas where the differences between the GHG only + aerosols only and the all forcings experiment are identified as “considerable” (following the last paragraph of page 12). Otherwise looking at those maps consists in a tiresome game of “find the differences”. Say how you define the considerable differences (in a statistical sense).

This is a valid point, and we have changed Figure 5 to include a map of the difference between the sum of the responses of GHG-only and aerosol-only and the total historic response, whilst the map of the historic response is removed (as it is nevertheless discussed in the companion paper by Bentsen et al (2012)).

Furthermore, instead of hatching, we have included similar maps of the sum of the responses to GHG-only, aerosol-only, and natural-only as well as the difference between the latter and the total historic response. The remaining differences in the latter figures are due to the sum of pure unforced internal variability and the effects of land cover induced albedo changes. The figure 5 now clearly shows that while the aerosol and GHG forcing dominates and tend to cancel each other, the total precipitation response in some areas have the same sign. To the extent that single realizations permit, we discuss where vanishing and non-vanishing trends are potentially systematic and not the result of internal, chaotic variability.

We have considerably re-worded the discussion in line with the new Figures 4 and 5, hopefully making the reasoning to appear more evident. For example we avoid the term “considerable” unless it is clear from the context what is meant.

10) Page 15, line 26: replace “to” with “too”.

Done

11) Page 24, 1st paragraph: the EOF analysis reveals that the NAO in NorESM1-M tend to be projected on the 4th EOF, which explains around 7% of the variance. In NCEP, the EOF 2, exhibiting the NAO pattern, explains around 15% of the variance. This should imply that the NAO explains less variance in NorESM1-M than in NCEP (there should also be a link with the storminess, as mentioned in this paragraph). As well, the fact that the NAO seems to be projected on two EOFs is likely to arise some questions on the NAO in the NorESM1-M. In the contrary, the PNA in NorESM1-M is close to the PNA pattern in NCEP, so this result is already satisfying. This would thus be interesting to have a more precise comment on the NAO here. An additional figure with the two first EOF of the North Atlantic domain (NAO and East Atlantic Pattern, -80/40_E;20/80_N) with the amount of variance explained, on the same data (NorESM1-M and NCEP, as pre-processed for this section), will give a clearer view on the NAO in the model.

We do not fully agree with all the points made by the reviewer on this point. It is true that NorESM1-M does not replicate NCEP-reanalyzed data for the northern hemispheric EOFs of the de-trended monthly 500 hPa geopotential heights with the seasonal cycle removed. Whilst EOF1 compare quite well, this is not the case for EOF2, and a pattern comparable to NCEP's EOF2 is mainly found in EOF4 for NorESM1-M. NAO, as a part of AO (the Arctic Oscillation), is actually a part of the pattern of EOF1 for the NCEP-data as well as for NorESM1-M, and – although weaker and displaced eastwards compared to NCEP - NAO is also a part of EOF2 for NorESM1-M.

The pattern which is not readily seen in EOF2, but turns up in EOF4, is the cold-ocean-warm-land, COWL. As for NCEP's EOF2-pattern, this EOF4-pattern also includes a signature of NAO, and this signature is geographically more correctly placed over the North-Atlantic Ocean than the patterns in EOF1 and EOF2 of NorESM1-M.

Of course, we agree that the model is far from perfect in replicating the observed NAO-

behaviour. However, we also think that PNA is too dominant while COWL (and NAO in its correct position) is under-represented in the model. In our opinion we can also relate the phase error of NAO to some of the weaknesses diagnosed in the storminess analysis, where we find that the North-Atlantic storminess is too zonal and too frequently enters over Europe.

As the sectorial EOF analysis does not contribute any significant new information, we have not included those figures. We have, however, changed the discussion in section 7.3 to be in line with the features mentioned above. In particular, to sum up the EOF analysis, we state the following text:

To summarize the EOF analysis, the model's Arctic Oscillation is slightly too strong with an associated NAO-like pattern displaced towards the east over Europe, and it has too much variability. Furthermore, relative to COWL and the geographically correct NAO-pattern, the model produces too strong variability associated with the correctly placed PNA pattern. A sectorial EOF-analysis gives similar results for the leading EOF as the hemispheric analysis with respect to the displaced NAO-like pattern, probably because COWL is a truly hemispheric pattern.

12) Page 24, line 7: replace "NorEMS1-M" with "NorESM1-M". Done.

Interactive comment on Geosci. Model Dev. Discuss., 5, 2933, 2012.