Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2022-181

Anonymous Referee #2

Referee comment on "The Euro-Mediterranean Center on Climate Change (CMCC) decadal prediction system" by Dario Nicolì et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-181-RC2, 2022

Review of "The CMCC Decadal Prediction System"
by Nicoli et al.
GMD-2022-181, submitted on 12 Jul 2022

General comments

In this manuscript the authors present an overview of the CMCC decadal prediction system (CMCC DPS), a contribution to CMIP6 and DCPP, with a focus on the evaluation of CMCC DPS's decadal retrospective forecasts (hindcasts) of surface air temperature. After an introduction to the topic, the authors present a basic description of the DPS setup, go into the details of their verification metrics, and then present their results mainly in terms of surface parameters and the evaluation of some climate indices. The manuscripts closes with a summarizing discussion.

I think that CMCC DPS and its skill assessment should definitely being published in GMD. However, with the current manuscript the authors fall a bit short of getting my recommendation. (1) In my opinion the skill assessment is just too much focussed on surface parameters, a short detour into the ocean would make the picture perfectly round, especially when considering that the system is working on decadal time scales. (2) Also, the authors' approach toward their full-field initialization scheme could be better explained and motivated. (3) And last not least I am missing some major conclusions in the abstract or at the end of the manuscript. Is the DPS' skill good enough to warrant the DPS use as an operational system? What is it that CMCC DPS can do very well?

Major comments

(1) On the focus on surface parameters: I would like to ask the authors to include an assessment of upper ocean heat content prediction skill and an assessment of the state of the AMOC. I am aware that reliable AMOC observations only exist since 2004 (at 26°N), so a figure of the mean AMOC cell and a timeseries at 26°N may suffice.
(2) On the initialization approach: I did not fully understand, how the initialization approach in CMCC DPS works. I am fine with the listing of all the products used in the process of initialization. But still: is there any "classical" data assimilation involved? Are initial fields directly derived from reanalyses or other reference datasets? How does the initialization compare to other initialization approaches within DCPP?
(3) On the conclusions: I appreciate the author's state to "only" report on the prediction system and its skill. However, some sharper conclusions (at the end of the manuscript and in the abstract) could improve the manuscript considerably. They could go along the line of these questions: Is the system good enough to be used for decadal predictions? What is it that the system can do exceptionally well?

(4) On the 10 historical members for verification: I understand that the authors would rather use 15 prediction members than only 10. Nevertheless the more consistent approach would be to use only 10 members in the prediction ensemble when it is going to be verified by a 10 member historical ensemble. Please elaborate more on that you can actually use the 15 members.

Minor comments

l. 10 "Abstract"
The abstract is missing any conclusion.
Please include some of your main conclusions, they are also still missing in "Summary and conclusions"

l. 15 "full-field initialization"
Please be precise in terminology. In the summary it is named "full-value". From what I understood the term "full-field and full-value" might actually be describing the setup best. But "full-field" would be accurate enough, I guess.

l. 27 "Model systematic errors"
Since these errors may not be present in an uninitialized historical or control simulation, I would suggest to call them not "model systematic" but only "systematic" errors.

l. 31 "For a long time"
Please be more specific. What do you mean with "long"? 10 years, 50 years?

l. 94ff "All members are initialized on November 1st, starting from full-field estimates of the observed state of the ocean, sea-ice, land surface and atmosphere."

I am puzzled how the 15 ensemble members are initialized. Just by simply taking reanalyses fields as the starting point? In the following, the authors name different initial conditions for atmosphere, ocean etc. Could you please summarize this in a table so that the 15 different initial conditions are shown together?
It remains unclear, how much of data assimilation has been used for initialization. Please include a statement on that topic.
If you have not been using data assimilation, could you please elaborate more on why not?
If you have been data assimilation, how many of the reanalyses used for this DPS have been done with the same or similar model? This question goes along the line of trying to use the very same coupled ESM for data assimilation, ensemble generation, and prediction, see e.g. Brune and Baehr 2020.

l. 113 "We use a 10-member ensemble of historical simulations initialized"
Unfortunately only 10 historical members are used for verification. I strongly suggest to use 15 historical members as well, or scale down the hindcasts ensemble to 10 members. If that's impossible, please elaborate more on the discrepancy between the two ensembles and the possible downsides in terms of skill assessment.

l. 290 "Summary and conclusions"
This is rather a summary discussion. A proper discussion is missing. For a summary it is quite long.
Please include at least a paragraph at the very end with the conclusions, incorporating your two or three main findings.

l. 292 "full-value initialization"
See above, please use consistent terminology.

l. 295 "different variables"
Please be more specific here: are you referring to "surface parameters", or "surface temperature", actually the results are about "surface parameters" and "surface climate indices".

l. 297, l. 302, l. 314 "NoInit"
I suggest using "non-initialized historical simulation" or "historical simulation" here to not overfreight the paragraph with abbreviations.

l. 297 "The ROC score index in Init highlights the DPS ability"
Please use "initialized prediction" or "prediction" instead of "Init" here. Also, the sentence sounds a bit awkward. Please rephrase, e.g. "The DPS correctly discriminates (...) intervals, with ROC scores of <value> (Fig.?)."

l. 308f "significant skill is only found over Sahelian Africa ..."
Perhaps it is worthwhile here to include skill values or the range of them.

l. 317 "AMV signal is skillfully predicted"
Same as above, a skill value would help.

l. 325 "Model systematic errors"
see above comment for the abstract, I would suggest to name this only "systematic errors".

l. 327 "Admittedly, AMOC is particularly affected by the initialization strategy,..."
I suggest including a figure illustrating the initial AMOC state and the resulting long-term drift (at 26°N). With the direct full-field/full-value initialization, strange things could happen to AMOC and come back later on the time scale of 10 to 30 years, which is characteristic for AMOC disturbances. This figure could go into the supplement.

Technical comments

l. 41 "(CMIP5)(Smith et al. ...)
Please use correct citation for CMIP5 AND please avoid brackets)(. Here I would suggest: "(CMIP5, Taylor et al. 2012; Smith et al. ...)"

l. 44, l.64 Same as above, please avoid brackets)(.

l. 332 "course"
Should read "coarse".

References

S. Brune and J. Baehr, “Preserving the coupled atmosphere-ocean feedback in initializations of decadal climate predictions,” WIREs Clim. Change, vol. 11, no. 3, Art. no. 3, 2020, doi: 10.1002/wcc.637.