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Comment on gmd-2020-248

Anonymous Referee #1

Referee comment on "The ENEA-REG system (v1.0), a multi-component regional Earth system model: sensitivity to different atmospheric components over the Med-CORDEX (Coordinated Regional Climate Downscaling Experiment) region" by Alessandro Anav et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-248-RC1>, 2021

The manuscript describes and evaluates a new regional Earth System Model (ESM), the ENEA-REG system. The system's main components are a regional ocean model, the MITgcm, a river routing scheme, the HD, and two choices of atmospheric models, the RegCM and the WRF. The description paper for ENEA-REG, version 1.0, is certainly needed, and the GMD is the proper journal for this. But, there are a few issues the authors should work on before the final publication.

It is an advantage to have the possibility to choose between two atmospheric models in the ESM. This possibility allows a, as it is tried in the manuscript, fair comparison and investigation of the impact of different atmospheres on the, e.g., ocean circulation. But, both atmospheric models itself have many options available. For example, WRF can choose from various land surface schemes, microphysics schemes etc. Therefore, there are already many different atmospheric forcings to be got from different WRF setups. Why using two different models, which will, e.g., increase the challenge of future support? This decision should be more strongly motivated in the introduction.

The discussed simulations were driven by ERA-Interim reanalysis. WRF was nudged, i.e. not only driven at the domain boundaries, to ERA-Interim. It has been shown before that nudging improves the simulation quality, but only if nudged to reanalysis data. It is probably not helpful to nudge against any global climate projection, which is far less good than any reanalysis, as nudging imprints the driving models deficits on the nested simulation (e.g. Leps et al. 2018). And, RegCM was not nudged. I would prefer to see a comparison with both atmospheric components not nudged. Perhaps, the simulation results would be more similar. On the other hand, the different atmospheric results force the ocean differently, which shows the impact of different atmospheres more clearly.

I additionally would like to get a bit of information about the computational costs. WRF is more expensive than RegCM? The ocean MITgcm cost is negligible?

The authors often used the reanalysis ERA5 as a reference, e.g., for 10-m wind over the sea. It should be made clear that ERA5 over the Mediterranean Sea might be off too and should be taken cautiously.

The statement on page 10, line 279, "no single combination of parameterizations yields optimal results" is a bit misleading. This statement is used as an explanation of a temperature bias larger than 4°C, which is quite substantial. It is true that no parameterization, and no combination of parameterizations, can be perfect, but still there are successful global climate simulations. I would avoid using this excuse here.

The language of the paper should be improved. A few examples are:

Titel: ".", perhaps a "-"?

Abstract: line 21: "remarkable biases are relevant for some variables" -> relevant for processes, seen in simulated values of different variables?

page 17, line 514: "estimation has been faced" -> "the challenge of estimation ... has been faced"

page 26, line 785: "climate constraint by coupling" -> the simulated climate is modified by the actively coupled Med. sea or similar. The coupling itself cannot change the climate, and the Med. sea cannot constrain but modify the European climate.

Leps, N., Brauch, J., & Ahrens, B. (2019). Sensitivity of Limited Area Atmospheric Simulations to Lateral Boundary Conditions in Idealized Experiments. *Journal of Advances in Modeling Earth Systems*, 11(8), 2694–2707. <https://doi.org/10.1029/2019MS001625>