

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-3

Anonymous Referee #1

Referee comment on "Performance of the Adriatic Sea and Coast (AdriSC) climate component – a COAWST V3.3-based coupled atmosphere–ocean modelling suite: atmospheric dataset" by Cléa Denamiel et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-3-RC1>, 2021

Review of the paper "Performance of the Adriatic Sea and Coast (AdriSC) climate component – a COAWST V3.3-based coupled atmosphere-ocean modelling suite: atmospheric part" by Denamiel et al., 2021

General comment

In the paper entitled "Performance of the Adriatic Sea and Coast (AdriSC) climate component – a COAWST V3.3-based coupled atmosphere-ocean modelling suite: atmospheric part" the Authors introduce and analyze the performances of the atmospheric compartment of the AdriSC coupled model. The paper is interesting and opens the door to some new important development in the Mediterranean modeling community. However, I'm little bit puzzled about some points that I summarize here:

- The Authors do a very robust validation of the atmospheric variables using several datasets. But in the text it looks like that some of them are not very reliable. And so I asked: why do not You use another dataset? I never saw papers using EOBS for the Sea Level pressure. Why do not you use ERA-interim which assimilates Sea level pressure and thus it can be considered more robust for the validation?
- The text is full of numbers and thus for a reader sometimes it is difficult to follow the discussion. I think the Authors should avoid such an inflation of numbers and try to establish some take-home messages focusing on the most important biases.
- Information about the coupled system are missed: for example, if you consider a spin-procedure or not for the ocean part? Any information about the river (which are important in the thermohaline circulation of the northern part of the basin)?
- The most important point: the Authors list very carefully the biases with respect to the observations. But as far as I see the discussions of the source of these biases are missed. I think that a possible user of the modeling tool should be aware of the existence of the biases and the respective sources. This should also help the

improvement of the performances in the future. The biases observed are related to convective scheme? Boundary layer? Boundary conditions? Land surface scheme

To summarize I think that the paper deserves to be published in GMD but only after some major revisions which address all these points.

Specific comments

Line 24-34: The Authors correctly list a series of RCMs developed in the framework of Cordex-initiative. However, they do not report the Med-Cordex initiative that focus specifically on the Mediterranean region (which also includes the Adriatic basin which is the focus of their work). I think the Authors in this introduction should focus more on the Med-Cordex initiative eventually discussing the development of regional coupled system in this framework (see for example Sevault et al., 2014; Ruti et al., 2016; Somot et al., 2018; Reale et al., 2020; Sein et al., 2020) than on the global Cordex initiative. Moreover, I see the message in the sentence "RCMs...land-sea contrast" but the sentence, I think, is misleading as RCMs are specifically developed to better resolve topography etc.. Additionally, the coupling between ocean and atmosphere works well (sometimes) also in the open ocean areas not only in the coastal areas. To summarize I would reformulate the paragraph

Fig.1 : I would show in first panel also the bathymetry of the Adriatic Sea (which you discussed in the text and it is quite interesting) because the coupled model has an ocean compartment

Line 40-44 As far as I remember the Adriatic Ionian Sea interaction (BioS) is driven by dense water formation in the Southern Adriatic and not by the bora wind. Deep water formation in the Northern Adriatic becomes important only in case of extreme events (see winter 2012, Gacic et al., 2012). I would rephrase the sentence.

Line 53 "to" proper not "for" proper..

Line 56 Why do you choose 1987-2017? (Because of the ocean MEDSEA reanalysis?)

Line 57-77 I think this part should go in data and methods. In the Introduction the reader is more interested in learning about the scope of the work and its structure

Section 2.1.3: this section should go before section 2.1.2 that describes the portal

Line 140 Which resolution ERA-interim has? 1.5/0.75°? The citation for ERA-interim I think should be Dee et al. 2011

Line 145-146 You should report that the reanalysis is a CMEMS product

Line 158-159: I would cite some works using E-OBS

Line 240-246 I do not see the Authors' point here. You are comparing hourly data with hourly data and then you have a good sampling and the correlation is good. If you compare hourly data with 6-hourly or daily data maybe you are suffering of some oversampling/undersampling due to the frequency considered. I would remove this sentence otherwise the Authors should explain better with some examples their findings.

Fig.4 Why do not you show the first, 25, 75, 99 percentile in this order?

Section 3.2 As I said in the general comments the Authors should describe the possible sources of the biases observed. In this way a reader or a possible future user of the data can appreciate the possible limits of the tool available. For example 8 C in 99th percentile is quite high. Do you have an explanation for that? Boundary layer scheme or short wave radiation overestimation related to ERA-interim?

Fig.5 I would revert the color bar showing in blue the peak of the precipitation or the overestimation of precipitation and the red the drier conditions or the underestimation. Around the zero the color should be white not gray.

Line 325-326 as in the general comment if you think that the dataset CCMP is not very reliable as reference dataset (also EOBS in the case of Mean SLP) why do not use another dataset just for the comparison (ERA-interim reanalysis could be a good reference as for MSLP)?

Line 375 1 C bias is not slightly. please remove "slightly"

Fig.12 -Fig.15 I would color both land and sea in white and leaves in black only the coastlines: the marker line of each dot can be also done in black

Line 558-560 (and after) I would be a little bit careful with that..As far as I remember Theocharis et al., 2014, Reale et al., 2016 and Reale et al., 2017 discussed the necessity of the inclusion of the Aegean Sea in the Adriatic Ionian to explain the BIOS variability...I should discuss that in the sentence..Moreover, does it mean that you plan to include also the Aegean Sea in your modeling domain?Do you expect that the MEDSEA (which provide the BCs in the ocean model) reanalysis include the BIOS signal?Did you check that?

Line 570-573 I found interesting the idea to apply your model to simulate future scenarios. But as I've seen from the text to run your model for 31 years (without spin-up) you need at least 18 months. For a longer run how would you deal with a simulated period of 100years? What about the spin-up of the ocean part?