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Geoscientific
Model Development
Discussions

Interactive comment on "Modeling the Caspian Sea and its catchment area using a coupled regional atmosphere-ocean model (RegCM-ROMS): model design and preliminary results" *by* U. U. Turuncoglu et al.

Anonymous Referee #1

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"Modeling the Caspian Sea and its catchment area using a coupled regional atmosphere-ocean model (RegCM-ROMS): model design and preliminary results" by Turuncoglu et al.

General comments ————————- This is an interesting paper that describes the devleopment and first tests with a coupled regional climate model fro the Caspian Sea. The Caspian Sea region is interesting because its high susceptibility to climate change and the high socio-econmic impact from such a change. Thus, a coupled climate model for the region is a valuable tool for impatc and adapation studies.

The most important feature still missing in the model - and the authors clearly mention it - is a good routing scheme for the major inflows. Without a routing scheme the Caspian Sea cannot respond properly to changes in the precipitation in teh surrounding draiange basins. I'd strobgly encourage the authors to pursue this work as announced in the discussion section.

The paper is generally well written and the language appropriate (to the extent as I as a non-native English speaker can judge it). The only confusion arises from SST and LST that are used synonomously, sometimes LST is used, in other places SST. I suggest the authours make this consistent and either talk about sea surface temperature or lake temperature.

Specific comments ————————

Abstract: "The distribution of sea ice and its seasonal evolution are well captured." I don't really agree with that statement, the sea-ice extent is well captured, but definitely not the sea ice concentration (fraction)

Model description (sections 2.1-2.2): These sections list all possible options fro teh different components, although only one of teh options is selected for the model. For example all possible convection schemes are listed, but this information is not relevant in the context of this paper. Why not shorten these sections and only list the relevant options? Possibly this could be done in a table.

Coupling (section 2.3): Figs 1 and 2 didn't contribute to my understanding, rather confused me. Take Fig 1: does the process marked with a 1 (the black dot with a 1) run first, then stops and process 2 is done? Or do they go in parallel? I think this section needs some revision to better explain the workings of the coupling in this model.

Atmospheric model (section 3.1): I am not an expert on lake models, but a vertical resolution of 1 m in combination with 50 km horizontal resolution seems to be odd. Please comment.

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Ocean model (section 3.2): "The ratio of internal to external mode time step is defiend as 20s." It's a ratio and should thus be unitless.

In this section it says that the wetting and drying scheme of ROMS are activated. Is that consistent with the statement on l.12 p.3912?

The last paragraph of this section is unclear. Does your model need relaxation to maintain a stable climate after the spin-up? Or do you relax during the spin-up?

Observational datasets (section 3.3): I miss a description of Ibrayev's climatology that is used later. In particular, this dataset could warrant a discussion of the fact that these data d not cover the same time as the model simulations. Given the large fluctuations of CSL this could be a rather strong caveat.

Results: l.21 p.3919: why do you think the excessive vertical heat transport is inherited from the driving ERA interim analysis at the boundaries? The Caspian Sea shouldn't be influenced by teh boundary conditions, or?

Figure 6 and discussion: I would't plot precipitation bias in an absolute scale, rather the ratio model/observation - 1 (relative bias). The reason is that precipitation varies a lot, and small differences can be significant in dry regions but mean nothing in areas were precipitation is abundant. The precipitation differences you show in Fig.6 are of similar order of magnitude than the observations, and therefore I wouldn't call the biases small in the text.

l.15ff p.3924: you mention a few caveats for the OAFlux dataset. Do you have a reference for these statements?

l.10 p.3925: You conclude that a 3-d ocean model is essential for the simulation of the evaporation. But, where is evaportation computed? Isn't it done in the atmosphere model? If so, how would the 3-d ocena model improve the representation of the evaporation?

l.21ff p.3925: does it really make sense to include Figure 15 and comments given

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Interactive Comment

the fact that no river routing is included and all inflow is based on climatology? The conclusions are rather limited.

l.1ff p.3927: Similar temporal pattern? My eyes tell that the modeled annual cycle has a larger amplitude than the observations in Fig.16.

Technical comments ————————

Some figures contain empty spots, for example in Fig.1 the spring pictures for ATM.CPL is missing. Is that on purpose or did just some of the figures disappear when producing the pdf?

Interactive comment on Geosci. Model Dev. Discuss., 5, 3907, 2012.