Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-368-RC3, 2020

Geoscientific Model Development Discussions

Interactive comment on "Evaluation of regional climate models ALARO-0 and REMO2015 at 0.22° resolution over the CORDEX Central Asia domain" *by* Sara Top et al.

Anonymous Referee #3

Received and published: 26 April 2020

This paper describes the results of two models (REMO and ALARO-0) simulations over CORDEX Central Asia domain. Authors compared simulated temperature and precipitation climatology and concluded that both the models are capable to reproduce CAS climate.

Reading the paper I had an impression it is a kind of technical report but not a scientific manuscript suitable for GMD. I do not see any science by describing how large biases in models are without any reasonable explanation where they come from. Authors took models which were tuned for Europe, implemented them for CAS, obtained huge biases and concluded: "That's it." Therefore I would recommend the manuscript for

publication only in case it will be substantially revised.

Major points

1.Analysis (but not referring to other models results) of model biases is required. Where they come from? Is it large scale atmospheric circulation or local processes, e.g. atmosphere – land heat/moisture exchange? In this sense it would be interesting to look in mean sea level pressure (MSLP) biases. For example, the warm temperature DJF bias as well as huge overestimation of DJF precipitation in REMO could be because of underestimation of Siberian High.

2.The models show quite a substantial differences in biases. Considering the eastern part of CAS it is clearly seen that in cold seasons REMO simulates 2m temperature much better then ALRO. Furthermore ALRO results with almost 10K bias over quarter of the domain are inacceptable. The opposite is seen for precipitation which is simulated by ALRO better. Based on these results authors can take heat (moisture) fluxes as well as heat (moisture) transports from both the models (assuming that "better" model reproduces better fluxes (transports)) and try to analyze which of them leads to produce mentioned above biases.

3.For better understanding I would also recommend to analyze the climatological annual cycle of some quantities, like temperature, precipitation and heat fluxes at least for the eastern part of the domain (from Mongolia to the east), where the biases are really large. For such a big domain with a plenty of climatological zones Taylor diagrams are more a kind of speculation. E.g. in case the climatological temperature varies from +30C in the South to -30C in the North spatial correlation will be high with any kind of model.

4.Authors should have a more deeper look into previous studies done with the same models. In particular ones were done with REMO. Since REMO existence (more then 20 years) there are many papers with REMO simulation results over regions partially included in CAS, e.g. whole the northern part: Niederdrenk, 2013 (PhD), Niederdrenk

et al., 2016 (Clim. Dyn.), Sein et al., 2014 (Tellus); south-eastern part: Xu et al, 2018 (Clim. Dyn.).

5.Authors claim that some of the biases come from the ERA-Interim forcing. That is quite an ambitious conclusion, in particular for Siberian continental climate. This conclusion has to be proven with some additional simulations. It is not a big deal to take a lateral boundary conditions from some of the global climate model, to simulate ca. 10 years and to look if the large scale biases are similar or not. I think with available computer recourses it should be just 3-4 working days.

Minor points

L. 23: I do not think that with large scale 8-10K 2m temperature biases and more then 100% precipitation biases over quarter of the model area both models reproduce climate "reasonably well".

L.24-25: It has to be done in this work, but not postponed to the unclear future

L.35: Even being a not an expert in CORDEX and even for CORDEX domains mentioned by authors, I know much more works based on multi-model regional simulations. E.g. Africa: Paxian et al. (JGR-Atmos, 2016); Mediterranean: Damaraki et al. (Clim.Dyn, 2019), Gaertner et al. (Clim. Dyn, 2017), Soto-Navarra et al. (2020, Clim.Dyn).

L.61: "Absence of reliable observational data sets". Over China and Russia? Maybe 20 years ago "yes" (describing CRU data authors site work from 1999), but at the present time it sounds at least strange.

2. Methods. See above (L.35) Central America: Cabos et al. (2019, Clim. Dyn.), Southeast Asia: Zhu et al. (2020, TAC), Arctic: Akperov et al. (2019, Global and Planetary Change; 2018, JGR)

L.94: I would remove word "sea". In a middle school I have learned that Black, Caspian Red and Baltic seas are seas, but it is hard to say that they are barely covered with

CAS domain.

L.96: Before claiming it, authors should "google" a word "HighResMIP". In the framework of this project there are many global climate model simulating climate on 25 km resolution, i.e. the same resolution as authors use for their regional simulations.

L.106 and in other places: I would suggest to use not "coupled zone", but "sponge zone". Forcing a regional model with reanalysis has nothing to do with coupling.

L.129: But what about dynamical core itself? Please explain at least in the way it is done for ALRO above, i.e. special discretization, advection (e.g. in ALRO it is based on semi-Lagrangian algorithm and what about REMO?)

L.137-138: What about upper boundary? Which height does it have? 10hPa? 50hPa?

L.202: As far as I know almost all the atmospheric models (including REMO and ALADIN) provide direct output of Tmax and Tmin which are obtained every model time step. Why not to use them directly?

3. Results: As I mentioned in "major points", not only seasonal means but also climatological annual cycle for the quantities averaged over different areas has to be included.

L.229: Exceeded. How much does it exceeded? On the plot I can only see that it is larger then 10K.

L.234: What has Scandinavia to do with Mongolia? They have completely different climate. In the same way REMO group can write: Paxian et al. (2016) showed a strong precipitation bias over Guinea in Africa. Maybe that is also a reason of REMO prcip. bias over East Siberia?

L238: Actually the strongest cold bias over Europe in REMO is at Spring. It is not visible in most of the papers, because mainly they show DJF and JJA only.

L.360 (Fig.8) Relative difference in mm/month? I think it should be in (%)

To all the figures with biases: For the biases I would avoid linear color bar and extend it for larger values. E.g. for the temperature something like: 0,1,2,3,5,7.5,10,12.5,15 and for precip. (%) 0,10,20,30,50,75,100,125,150,200

L.405: What the Czech Republic has to do with Central Asia? Do they have similar climate? I have here the same claim as at L.234. Authors should provide arguments which has something to do with CAS and not speculations like: we have warm bias in Mongolia, because in French Polynesia is to rainy.

L.414: I would not say that up to 10K large scale temperature bias is something which is VERY well

L.423: "..assigned to this forcing". As it was mentioned above (Major points), before speculating about it, please do some simulations with different forcing.

L.433: "Ozturk et al. . . ., but they did not explain it." And? If Ozturk did not explain it, it is over? Why don't you try to explain it in your manuscript.

L.428, 448, etc. New et al. (1999). You discuss present climate and present observational data set citing a work from 1999? There is a quite a big difference between the number of observations before 1999 and now.

Fig. 11: I think should be MW, but not WM. As well as (%), but not mm/month

Conclusion: In the scientific sense conclusion is very poor simply describing how large model biases are only. The only one "explanation" of their origin is "models are good, but observations are bed", based on results obtained more then 20 years ago, in 1999. I would suggest to authors to bring more "scientific analysis" into the manuscript considering comments written above. Maybe it will bring the paper from "technical report" to "scientific manuscript".

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-368, 2020.