The manuscript presents results of a sensitivity experiment for setting up the regional climate model - COSMO-CLM in the CORDEX Central Asia domain. The experiment allows finding a combination of different physical parameterizations, which gives the best CCLM performance over the considered domain. Such sensitivity experiments are the first and necessary step before starting downscaling of multi-decadal GCM climate projections and aim to understand what regional processes are more important in different CORDEX domains. A lot of work has been done and the results of this study are certainly interesting to the CORDEX community.

Although these general positive considerations, I regret to inform you that I cannot recommend the publication of the manuscript in its present form, being the manuscript affected by serious issues that needs careful attention, as highlighted in the General comments.

General comments

The level of the manuscript is quite poor; especially the Introduction is too "educational" and seems more similar to a technical tutorial for PhD students than to a scientific paper for experts. Many concepts are explained in details, but they are well known by the scientific community working with RCMs, and could be replaced by proper references. On the other side, a detailed synthesis of the state of art is completely missing, especially for what concerns previous sensitivity studies performed with CCLM or other regional models. There are specific works by Bellprat et al. (2011, 2012) or Bucchignani et al. (2016) that have been referenced, but were not mentioned properly. In the COSMO consortium, specific Priority Projects (CALMO, CALMO-MAX) have been established in order to optimize the model configurations, but they were not mentioned. In this view, I suggest the following papers to be analyzed and referenced:

Avgoustoglou E, Voudouri A, Khain P, Grazzini F, Bettems JM (2017) Design and Evaluation of Sensitivity Tests of COSMO model over the Mediterranean area. Perspectives on Atmospheric Sciences, 1:49-55

Voudouri A, Khain P, Carmona I, Avgoustoglou E, Kaufmann P, Grazzini F, Bettems JM (2018) Optimization of high resolution COSMO model performance over Switzerland and Northern Italy, Atmospheric Research, 213:70-85.

My major concern is however related with robustness and significance of the results. A key aspect is that none of the simulations represents correctly the observed fields used as reference. Consequently, there is no value in analyzing the relative performance of the simulations, simply because all do in a terrible bad way. A temperature bias larger than 15° C or a precipitation bias larger than 200% is not acceptable.

A key question is about the reason of this shortcoming. Is it due to errors in the reference CCLM model configuration? A key parameter is certainly the time step adopted (dt), whose value is not specified in the manuscript, but it needs particular care. Alternatively, is it due to errors in the boundary conditions? The authors decided to use NCEP2, but I personally would prefer ERA-Interim, which are characterized by higher resolution, so reducing the resolution jump (other critical aspect).

The paper does not investigate the origins of these strong biases. Section 3 contains only a rough (boring, in some places) description of the figures, but no significant insights are provided. Some considerations are provided in the Conclusions, but this is not the right place, since Conclusions should be focused on the benefit of sensitivity runs with respect to the reference one.

In Sec. 2.3, you have properly defined some subdomains, but then they are used only for the analysis of variance. Instead, the results in terms of MAE (presented in Figs. 6 and 7) are averaged over the whole domain, which is too big and includes very different climate areas. I recommend that further investigations in terms of MAE be performed considering the single subdomains.

Finally, the differences among observational datasets are not discussed and the possible reasons for these discrepancies are not investigated.

Specific comments

Pag 2, Lines 11-18: "Among the… at once". This paragraph contains too many geographical and economical details and in my opinion does not fit well into the Introduction.

Pag 2, Lines 25–33 and Pag 2 Lines 1-9: "The countries… due to climate warming". These paragraphs are rather an analysis of the implications of climate changes on this area, and in my opinion do not fit well with the aim of the work. They should be significantly shortened.

Pag 2, lines 9-11: "All the reported… strategies". This sentence is a repetition of concepts already expressed.

Pag 2, line 14 "Assessing …. evaluation". This definition is well known by climatologists and can be removed.

Pag 2, line 17: "Model evaluation… development". This sentence is prosaic and can be removed.

Pag 3, line 27: "A series… simulation". This sentence is prosaic and can be removed.

Pag 4, lines 8-11: "Such analyses … configurations". This concept has already been explained previously and can be removed.

Pag 4, lines 11-14: "In the light… are presented". From this sentence, I do not see a relationship between your sensitivity and the CORDEX-CORE activities. Please explain better this relation and, at the same time, explain what CORDEX-CORE is.

Pag 4, line 32: "This study… domain". This concept has already been expressed in the Introduction. Please put it only once.

Figure 1: It is preferable to show the domain using the geographical coordinates, since the reader is generally not interested in the rotated coordinates (being rotated coordinates used only internally for COSMO-CLM calculations).

Pag 5, lines 11-13: "The model configuration… .en).". These details are not necessary, especially because readers are generally not authorized to download the model configuration from the website of CLM Community. Please add more details about model configuration in Table 2.

Table 2 (caption): The general description of model setup of the reference simulation is very poor. It contains details that have already been explained in the text (e.g. spatial resolution, domain extent). Btw, the domain extent must be expressed in terms of max/min longitude/latitude and not in terms of number of grid points. Some important details of the model setup are missing. For example, in Table 1 you write that in **b** configuration Tegen aerosol is used , but what is the aerosol

used in **a**? I guess the default Tanre, but you have to specify it. Similarly, for albedo: what is the default one? I guess albedo as function of soil type.

Pag 5, lines 19-20: "since their… simulations". This is not a good reason to employ NCEP reanalysis as driving data. Generally, data at higher resolution are preferred. Btw, what is the resolution of GCM normally employed in CORDEX simulations?

Sec 2.2: It is not clear if the original resolution of datasets CRU, UDEL, MERRA GPCC is 0.5° or if they have been interpolated on a common grid with 0.5° resolution.

Pag 6, lines 19-20: "The Climate … interpolation". This technical detail (usage of CDO) is not necessary and can be removed.

Pag 7, line 25: It is not specified if variances (observed and simulated) are evaluated starting from monthly values.

Pag 8, line 12: A bias of 15° or larger is not acceptable.

Pag 8, line 12-21: In this paragraph you are commenting Figure 3, which is related to simulation **a**, so it is not wise to comment here also the simulations SOIL and SNOW.

Pag 9, line 1: Why do you claim that this sentence is "interestingly" ?

Pag 10, line 10: Why in this case analyses focused on a single observational dataset?

Pag 11, lines 17-18: "For the experiments… experiment **q**". This sentence is just a repetition of the sentence at lines 13-15. Please combine them.

Pag 11, line 24-25: "This indicates…driving dataset". This sentence is very strong and must be supported by results that are more robust. The few numbers shown in Table 4 are not sufficient. Moreover, you should add in Table 4 the improvements achieved when using NECP2, in order to have a quantitative comparison.

Pag 11, lines 32-33: "Values closer… observations". These statements are obvious and can be removed.

Minor corrections

Pag 6, line 23: You have already explained that the reference configuration is the **a**. Please remove "(a, Tab.2)".

Pag 7, line 6: Do you mean Tab. 3 (instead of Tab .4)? Otherwise, Tab.3 is never referenced.

Title of Fig.6: If SS is defined according with equation (1), why did you add (%) next to SS?

Pag 11, lines 16-17: In the title you use NCEP, in the text NCEP2, please use always the same acronym.