Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-381-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Mediterranean Specific Climate Classification and Future Evolution Under RCP Scenarios" *by* Antoine Allam et al.

Anonymous Referee #1

Received and published: 25 August 2019

The manuscript of Allam et al. is a introducing a novel regional climate classification for the Mediterranean region. About the style, the manuscript needs copy-editing for English language check, about the content, despite the scientific relevance of the topic I have two majors concerns that would require substantial modifications to the manuscript:

1- The classification methodology is not robust enough. They authors choose 5 classes without any justification. The authors should provide a robust evaluation of the classification proposed, with the different datasets available. The part about Decision Tree is not sufficiently explained (see specific comments below). The authors should also better highlight the novelty of their approach, by comparison to two recent papers, Barredo et al. 2019 in the reference list, and : Koutroulis A., Dryland changes un-

der different levels of global warming. Science of The Total Environment 655, DOI: 10.1016/j.scitotenv.2018.11.215

2- On the climate change aspect, the use of one single regional climate model simulation is not enough to assess the uncertainties. I suggest either to remove this part or alternatively to strongly upgrade it. The literature review on the topic is very weak and there is a need to include relevant references providing climate scenarios for the whole Mediterranean domain and its different sub-regions. If the authors want to include a climate change study, they could use the ensemble of 50km simulations available in the MedCORDEX experiment. When studying climate change impacts, it is very important to consider the uncertainties from different GCM and RCM simulations, in addition to the uncertainties stemming from the emission scenario.

Specific comments:

Page 1, first lines of introduction: Obviously these sentences are from a text book. Please add the reference.

Page 2 line 11: add reference for MedCORDEX

Page 2, line 15: this part should be moved to a data section later in the text to present the RCM simulations

Page 2, line 25: Ref Tramblay et al 2013 is only for a basin in Morocco. Please add references relevant for the whole Mediterranean.

Page 3, line 1, Rivoire et al 2019 also provided a Mediterranean classification based on P-PET computed from CRU database.

Rivoire, P., Tramblay, Y., Neppel, L., Hertig, E., and Vicente-Serrano, S. M.: Impact of the dry-day definition on Mediterranean extreme dry-spell analysis, Nat. Hazards Earth Syst. Sci., 19, 1629–1638, https://doi.org/10.5194/nhess-19-1629-2019, 2019. Page 3, line 32: "Practiced discipline" = ?

Page 4 line 16: “to be treated in a personal way”, strange wording.

Page 4, section 2.2: Not clear what types of catchments are extracted. The authors should precise for which stream orders they extracted the catchment boundaries. Is it for all elementary catchments? Or is there a minimum basin size? For example in the JRC data or HYDROSHED the Pfafstetter coding system is used (de Jager and Vogt, 2010, in the reference list).

Page 4, section 2.3: it would be very useful to also provide a database as an output of this article, a map or GIS layer to have the climatic class for each catchment.

Page 4, section climatic data: The authors should provide a map with the number of station used to build the WordClim database in the Mediterranean and the locations of the 144 weather stations. Several authors have pointed out the strong variability of station density across the Mediterranean region, see:

Zittis G. (2017) Observed rainfall trends and precipitation uncertainty in the vicinity of the Mediterranean, Middle East and North Africa, Theoretical and Applied Climatology. https://doi.org/10.1007/s00704-017-2333-0.

Romera R., Sánchez E., Domínguez M., Gaertner M.Á., Gallardo C. (2015) Evaluation of present-climate precipitation in 25 km resolution regional climate model simulations over Northwest Africa. Clim Res 66(2):125–139.

Raymond, F., Ullmann, A., Camberlin, P., Drobinski, P., and Chateau Smith, C.: Extreme dry spell detection and climatology over the Mediterranean Basin during the wet season, Geophys. Res. Lett., 43, 7196–7204, https://doi.org/10.1002/2016GL069758, 2016.

In addition, the origin of this data is not provided. To which database do they belong? GHCN ?

Page 5, line 1: “5 and 3000” give locations/stations where these values are recorded

Page 5, line 1: Strange that the authors talk about taxonomy for a few lines later explain that it is not useful for climate classifications.

Line 6, line 26: why choose a priori 5 classes? This is a major methodological problem since it is a subjective choice. Usually when performing classifications with kmeans, diagnostic tools such as the Scree plot or Silhouette plot are used to identify and choose the optimal number of clusters. The authors need to clarify and improve this point about the “optimal” number of clusters.

Page 6, section 3.3: What is a Decision Tree? There are no bibliographic references in this section and this is clearly lacking. Do the authors refer to Classification and Regression Trees? (CART, Breiman 1984). How the method is applied is not clear. No need for this type of method to validate a kmeans classification.

Page 7, section 3.4: No presentation of the RCM simulations is provided. In addition, the use of a single simulation is not recommended to provide future scenarios, due to strong differences between different model simulations (Kotlarski et al 2014). This is for sure a weak point in the analysis presented.

See:

Kotlarski, S., Keuler, K., Christensen, O. B., Colette, A., Déqué, M., Gobiet, A., Goergen, K., Jacob, D., Lüthi, D., van Meijgaard, E., Nikulin, G., Schär, C., Teichmann, C., Vautard, R., Warrach-Sagi, K., and Wulfmeyer, V.: Regional climate modeling on European scales: a joint standard evaluation of the EURO-CORDEX RCM ensemble, Geosci. Model Dev., 7, 1297–1333, https://doi.org/10.5194/gmd-7-1297-2014, 2014.

Page 7, section 4.1: It is only in the table 3 that the reader can discover that potential evapotranspiration is computed from the Thornthwaite formula. This formula is most probably not adapted to the Mediterranean context, in particular for climate change scenarios. At least a discussion would be welcome to address this point.

See:

Beguería,S.,Vicente-Serrano,S.M.,Reig,F.,andLatorre,B.:Standardized precipitation evapotranspiration index (SPEI) revisited: parameter ïňĄtting, evapotranspiration models, tools, datasets and drought monitoring, Int. J. Climatol., 34, 3001–3023, 2014.

McMahon, T. A., Peel, M. C., Lowe, L., Srikanthan, R., and McVicar, T. R.: Estimating actual, potential, reference crop and pan evaporation using standard meteorological data: a pragmatic synthesis, Hydrol. Earth Syst. Sci., 17, 1331-1363, https://doi.org/10.5194/hess-17-1331-2013, 2013.

Page 8, line 24: “5 classes was the most suitable” this is a contradiction with the methodology described above, where the authors state Line 6, line 26 that they choose 5 classes.

Page 9, line 15: “a similar spatial distribution”, similar to what?

Page 9, section 4.4: Usually “validation” refer to the application of a model (or a classification) to data that has not been used for its calibration or training. This is not the case here.

Page 10, section 4.4.3: Again we don’t understand what is done here. A validation with a “decision tree”?

Page 10, line 16: “proximity analysis and spatial joint” are not statistical terms but rather obviously Geographic Information Systems (GIS) operations. Please explain clearly which method has been applied.

Page 11, line 3: The reference Colmet-Daage et al 2018 is about the Lez and Aude located in France, and Muga located in northeastern Spain. That is not representative of the whole Mediterranean basin. As mentioned before, the bibliography about climate change projections is rather weak and the authors should cite the relevant literature.

See for example (and the references herein):

Lionello, P., and Scarascia, L.: The relation between climate change in the Mediterranean region and global warming, Reg. Env. Change, 18, 1481-1493, doi: 10.1007/s10113-018-1290-1, 2018.

Wolfgang Cramer, Joël Guiot, Marianela Fader, Joaquim Garrabou, Jean-Pierre Gattuso, Ana Iglesias, Manfred A. Lange, Piero Lionello, Maria Carmen Llasat, Shlomit Paz, Josep Peñuelas, Maria Snoussi, Andrea Toreti, Michael N. Tsimplis, Elena Xoplaki. Climate change and interconnected risks to sustainable development in the Mediterranean. Nature Climate Change, 2018; DOI: 10.1038/s41558-018-0299-2

http://www.medecc.org/climate-and-environmental-change-in-the-mediterranean-main-facts/

Page 11, line 19: It is pretty obvious that “climate is continuous” and should not be mentioned in the conclusions.

Figure 1: Topographic boundaries should be replaced by hydrological boundaries, since what is shown on the map are catchment boundaries.

Figure 4: Green on green is hard to see (for olive boundaries)

Table 6 is very hard to understand

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-381, 2019.