Earth System Dynamics Discussions

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Comment on esd-2021-65

Anonymous Referee #3

Referee comment on "The Mediterranean climate change hotspot in the CMIP5 and CMIP6 projections" by Josep Cos et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-65-RC3, 2021

The manuscript compares regional climate changes in the Mediterranean area between CMIP5 and CMIP6. It is a good complement to the general efforts of the international scientific community exploring the added-value of CMIP6, with its predecessor CMIP5 as a basic reference. The manuscript also includes an attempt to improve the multi-model ensemble-processing methodology by implementing an algorithm considering both models performance and inter-dependence. The manuscript is generally well written and organized in a logic way. I think it can be accepted for publication after a few minor revisions.

1. The manuscript would be of higher value if there are some comparisons with similar works performed in other geographic sectors of the world. For example, there are some recent efforts focusing on regional climate issues in China (Zhu et al. 2020, 2021, Li et al. 2021: https://doi.org/10.1007/s00376-020-9289-1 ; https://doi.org/10.1016/j.scib.2021.07.026 ; https://doi.org/10.1007/s13351-021-0067-5).
2. Although CMIP5's RCP scenarios are close to CMIP6's SSP scenarios with the relevant nomenclature, there are indeed subtle differences for greenhouse gases, especially for emission of aerosols. This seems ignored in the present manuscript. In a more general manner, differences between CMIP5 and CMIP6, as analysed in the manuscript, include many aspects involving both anthropogenic emissions and improvement of models' physics and resolution. It seems that one cannot make a clear idea or conclusion, with what presented in the manuscript.
3. The ensemble-processing algorithm, based on models' performance and independence, imposes an observation constraint. The authors state that its use can make closer the results of CMIP5 and CMIP6, and make smaller the spreading of each ensemble among its members. They also point out a few exceptions. Are there any explanations? Generally speaking, the manuscript seems a little too descriptive and lacks physical interpretation.
4. It is a little disappointing to see only mean climate (for both surface air temperature and precipitation) is processed here, without consideration of any extreme climate events or their representative indices.
5. Line 165, Figure 1. The figure legend and associated descriptions are confusing for me. "…with respect to the 1986-2005 GLOBAL mean…"; "with respect to the MEAN GLOBAL temperature change and the MEAN 30º N-45º N LATITUDINAL BELT precipitation change". The authors need to clearly indicate what are particular in the displayed graphics, compared to usual practices.