

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-15

Anonymous Referee #1

Referee comment on "Large discrepancies in the representation of compound long-duration dry and hot spells over Europe in CMIP5" by Colin Manning et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-15-RC1>, 2022

Review of manuscript submitted to Weather and Climate Dynamics

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Title: Large discrepancies in the representation of compound long-duration dry and hot spells over Europe in CMIP5

Authors: Colin Manning, Emanuele Bevacqua, Martin Widmann, Douglas Maraun, Anne F. Van Loon

Recommendation: Rejection

General Comments

The authors analyse the links between the occurrence of anticyclones, dry spells and heat waves in a large ensemble of CMIP5 GCM simulations for the historical period. Additionally, ERA5 and E-OBS data is considered in the evaluation. The authors conclude that the discrepancies between the GCMs identified in terms of the duration of dry spells and extreme temperatures are related with the GCM biases regarding temperature and precipitation themselves. While the topic of the manuscript is surely interesting and I acknowledge that there was a huge amount of data processing involved, there are also severe shortcomings, the main one being the way that "anticyclones" are considered, which is methodological not sound (see major point). Given that this was the only aspect broadly related with atmospheric dynamics (the core of WCD), I'd argue that the manuscript in its present form is also out of scope of the journal. For both reasons, and given that this shortcoming is pretty fundamental to the whole analysis, I'd like to recommend its rejection in the present form (as the whole calculations and analysis would need to be redone).

This say, I'd strongly encourage the authors to take up this task and resubmit the manuscript for further evaluation. If resubmitted to WCD, the aspect of atmospheric dynamics should be strongly strengthened so it fits the scope of the journal (e.g. also links to blocking, synoptic weather types, anticyclone dynamics). If the authors do not which to change the focus of the manuscript so strongly, I'd suggest the resubmission to a different journal – notably NHESS, which focus primarily on impacts. I would be willing to review the paper again upon resubmission.

Main Comment

The main shortcoming in the present study is the way "anticyclones" are dealt with. For me, an anticyclone is a high-pressure centre with clockwise rotating winds and large-scale divergence at the surface. At upper levels, it is typically associated with a cut off low / ridge / blocking system, where upper-level convergence occurs, thus leading to large-scale subsidence in the area of the surface high-pressure centre. As described e.g. in a recent review paper published in WCD (Kautz et al. 2022; <https://doi.org/10.5194/wcd-3-305-2022>), a persistent anticyclone / blocking over Central Europe in the summer typically leads to heat waves and dry spells collocated with the centre of the system (see their Figure 2b). However, on both flanks of the system you often observe heavy precipitation associated e.g. with moist air intrusions on the western flank of the system (same figure).

Given this, I am really puzzled that the authors "define an anticyclone" as local exceedance of MSLP above 1012 hPa over five days. This value is below the average mean MSLP for a considerable part of Europe (particularly in the summer months!), and even below the global average MSLP value! Given the often quite stationary weather conditions in the summer, five days is also no real constraint. So we are not even looking at above

average pressure conditions. And of course, this has nothing to do with atmospheric dynamics and cannot provide any insight either on the exact location of the anticyclone and where different types of extremes may be expected. If a simple metric is needed, I'd use something based on MSLP anomalies to the monthly mean (or summer) MSLP fields, thus as an indication of the anomalous circulation associated with the high-pressure anomaly.

This explains why several of the following evaluations/results do not really meet the expectations (e.g. compared to what you'd expect for blocking, e.g. around lines 265) or are not really understandable (at least to me), e.g. Figs 3 and low collocation of dry spells and anticyclones for Southern Europe (as acknowledged by the authors also in the text), and several other figures, particularly Figure 8. While I do like the idea of the paper (which is my I accepted the review), I do think the methodology is flawed and the presented results are thus unfortunately not sound.

Minor comments

a) It is not clear to me why version 16.0 of E-OBS is being used, as we are currently on version 24.0 (see <https://www.ecad.eu/download/ensembles/download.php>). There have been quite a few important updates since.

b) I think it is a very strong statement to say that the "combination of dry spells and extreme temperatures" has not yet been assessed in CMIP5 models. Please weaken the statement.

c) The description of the results is often not understandable, e.g. the description of figure 3b (likes 228-236). Please enhance.

d) Several of the references are missing page ranges, issue numbers, particularly for AGU journals, please enhance.