Review of “Intensified future heat extremes linked with increasing ecosystem water limitation” by Denissen *et al*., 2023

Using a small ensemble of CMIP6 simulations, the authors show that areas with increasing ecosystem water limitation tend to feature stronger warm season maximum temperature trends (compared to mean temperature changes). While the mechanisms behind this have long been known, most analyses focus on past changes and it is an interesting, well-designed study that I consider to be relevant for a broad audience. Nevertheless, I list a few suggestions below that could be helpful in further improving the manuscript.

Comments

1.) I am not convinced by the choice of “mrso” to indicate root-zone soil moisture. “mrso” is simply the total column soil moisture, and the actual depth that is represented varies from model to model and can easily exceed 2 meters (Qiao et al., 2022). In the Supplementary, it becomes clear that you use ERA5-Land soil moisture down to 100 cm (first 3 layers), and I think this is a good choice as the bottom layer extending to nearly 3m depth is arguably more uncertain. However, it would probably make sense to use the very same definition for the CMIP6 models, and not rely on the column soil moisture. 1m soil moisture could be calculated by using all layers within 100 cm and adding a fraction of the respective lowermost layer (e.g., 0.5 if it extends from 80 to 120 cm).

2.) I am quite surprised to see how few models seem to have all the required variables, especially since you only need them in monthly resolution. I get at least 40 different models (not simulations, as for some models such as, e.g., CanESM5, MPI-ESM-LR or MIROC6, there are dozens of initial condition ensemble members) for each variable, and while I did not check the overlap for all variables, I am absolutely sure that far more than 8 models remain. It should be close to or even more than 30…
I would also like to point out that according to Qiao et al. (2022), the BCC-CSM2-MR model constitutes a rather unfortunate “choice”, as it does not perform well with regards to soil moisture. Moreover, to quote Qiao et al. (2022), “For deep soil moisture, the top-five best-performing models are CESM2, MPI-ESM1-2-LR, ACCESS-ESM1-5, CESM2-WACCM, and CNRM-ESM2-1, [...]”, of which only CNRM-ESM2-1 is used here. While such evaluations are particularly challenging for variables that are hardly observed/measured and notoriously spatially inhomogeneous, I still think it is a pity that a) only few models were used in the first place, and b) that state of the art models such as CESM2 with plant hydraulics (see, e.g., Zhao et al., 2022) are not included. I thus

strongly encourage the authors to check an alternative data source if they cannot obtain the required variables for more than the 8 models used thus far.

3.) I appreciate that the authors state that land–atmosphere coupling does not necessarily account for all of the “temperature excess”, but it also makes me wonder what else could contribute to stronger maximum than mean temperature trends. I agree that (changes in) advection could play a role, but I think there is another, perhaps even more important mechanism at play: in several regions around the world, aerosol emissions have decreased substantially and are projected to decrease further in the ongoing century. This results in more shortwave radiation reaching the surface compared to past decades due to higher atmospheric transmission, which noticeably alters the surface energy budget and hence near-surface temperatures (e.g., Nabat et al., 2014), particularly in the warm season when incoming shortwave radiation is typically highest. Maximum temperatures tend to occur between noon and late afternoon and are arguably closer related to incoming shortwave radiation than mean temperatures, which, during nighttime, are primarily governed by the longwave radiation budget (which is directly altered by anthropogenic greenhouse gas emissions and water vapor feedbacks). The study of Qian et al. (2011) supports this rationale by reporting that aerosol-related temperature effects mostly occur through (daytime) maximum temperatures. I would thus not be surprised if shortwave radiation changes — which can, of course, also be mediated by changes in cloudiness and not just aerosol absorption (although at least for central Europe, this aspect has been far less important since 1980; see, e.g., Wild et al., 2021) — also contributed to the temperature excess patterns shown in Fig. 1a. In some regions such as, e.g., China (Qian et al., 2011), India and central Africa, shortwave radiation has decreased in the last decades, so my example provided above should not be generalized. Showing downward shortwave radiation trends (rsds) for all models could be helpful to understand why areas where the sign of temperature excess and ELI trends is inconsistent.

Additional comments

- Some citations should be double-checked; e.g., “(Eyring et al., 2016))” comes with an additional right bracket.
- L. 85: I recommend changing “[...] please refer to Denissen et al. (Denissen et al., 2022)” to “please refer to Denissen et al. (2022)”. Same thing for “from Teuling et al. (Teuling, 2018)” on L. 321.
- L. 167 onwards: “Moreover, ET is generally significantly correlated with both temperature excess and ELI, respectively, establishing the physical link between these quantities”. The authors acknowledge themselves later on in the manuscript that their correlative analysis cannot establish causal links, so perhaps something like, e.g., “[...] , suggesting a physical link [...] ” would be more appropriate.

- L. 200 onwards: ERA5-Land is an offline land surface model simulation that does *not* assimilate any observations. The meteorological forcing provided by ERA5 does indeed make use of data assimilation, but this is largely restricted to “classic” variables such as 2-meter temperature and humidity. Surface soil moisture data from scatterometers is also assimilated, but this only affects the top soil layer and does not help much with regards to root-zone soil moisture.
- L. 315: “[...] increased entrainment of dry air above the atmospheric boundary layer”, I think rephrasing this to “[...] increased entrainment of dry air from above the [...]” or similar would be a good idea, the current version could be a bit confusing.

References

Pierre Nabat, P., Somot, S., Mallet, M., Sanchez-Lorenzo, A. & Wild, M (2014): Contribution of anthropogenic sulfate aerosols to the changing Euro-Mediterranean climate since 1980. *Geophys. Res. Lett.* **41**, 5605–5611. doi=10.1002/2014GL060798

To cite this article: Qian, Y., Leung, L. R., Ghan, S. J. & Giorgi, F. (2011): Regional climate effects of aerosols over China: modeling and observation. *Tellus B Chem. Phys. Meteorol.* **55**, 914–934, doi=10.3402/tellusb.v55i4.16379

Qiao, L., Zuo, Z. and Xiao, D. (2022): Evaluation of Soil Moisture in CMIP6 Simulations. *J. Clim.* **35**, 779–800, doi=10.1175/JCLI-D-20-0827.1

Wild, M., Wacker, S., Yang, S., Sanchez-Lorenzo, A (2021): Evidence for Clear-Sky Dimming and Brightening in Central Europe. *Geophys. Res. Lett.* **48**, e2020GL092216. doi=10.1029/2020GL092216

Zhao, M., A, G., Liu, Y. *et al.* (2022): Evapotranspiration frequently increases during droughts. *Nat. Clim. Chang.* 12, 1024–1030. doi=10.1038/s41558-022-01505-3