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Climate of the Past Discussions

Interactive comment on "Comparing proxy and model estimates of hydroclimate variability and change over the Common Era" *by* Jason E. Smerdon et al.

Anonymous Referee #2

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The manuscript presents a nice overview of the present knowledge of the hydroclimate variability over the past millennium from the joint perspective of climate simulations and proxy reconstructions. It illustrates some of the problems that arise when comparing both sources of information about past hydroclimate variability. I see very clear positive aspects in this manuscript , and I certainly recommend it for publication in Climate of the Past. I just have some suggestions that the authors may want to consider in a revised version, but most of them are more a matter of perspective. I also want to congratulate the leading authors for accomplishing a rather difficult task, namely to web a coherent and actually very informative text out of a workshop with so many participants and probably very diverse interests.

My suggestions follow:

1. When listing the most important limitations and deficiencies that climate models still show and that can be important for the simulation of hydroclimate, I missed that extratropical blocking is not part of this list (around line 540) . It is rather well known that climate models still suffer from clear limitations in this regard, although they are getting better at that. Nevertheless, blocking is a very important phenomenon for seasonal drought in extratropical land masses. North Atlantic blocking is not particularly well represented in virtually all CMIP5 models and it is very relevant for drought episodes in Europe.

2. Around line 590 the manuscript discusses the mechanism 'rich gets richer' and essentially accepts it as established truth. However, although this mechanism has been derived from basic theoretical considerations for the tropical oceans (Held and Soden, 2006), it is still far from clear that it is the main mechanism that can explain the response of hydroclimate to external forcing, here and in other areas. For instance, other later studies have looked into CMIP3/5 simulations and found that this mechanism may be overlaid by others that also impact on the patterns of change of hydroclimate, so that in the end the correlation between the pattern of response to CO2 and the climatological pattern of hydroclimate is almost zero, even in the tropics. In the extratropics, the situations may be even more complex, with multiple factors (or different aspects of the same factor) interacting in clear ways, ranging from increased humidity, shifts in the strom tracks, expansion of the Hadley cells, and shifts in SSTs gradients. Perhaps the authors may want to briefly discuss the papers mentioned below and others. I am aware that the space is limited and this is a very complex question, but I think it would be important to at least convey the message that the 'rich-gets-richer mechanism, though plausible and brilliant when it was put forward, may not be the whole truth.

Chadwick et al, Spatial Patterns of Precipitation Change in CMIP5: Why the Rich Do Not Get Richer in the Tropics doi:10.1175/JCLI-D-12-00543.1

Chou et al.Evaluating the “Rich-Get-Richer” Mechanism in Tropical Precipitation Change under Global Warming. doi:10.1175/2008JCLI2471.1

Huang et al: Patterns of the seasonal response of tropical rainfall to global warming, doi: DOI: 10.1038/NGEO1792

Sheff and Frierson, Robust future precipitation declines in CMIP5the poleward expansion of model subtropical largely reflect dry zones. doi:10.1029/2012GL052910

3. Around line 593

' In the tropics, areas experiencing post-eruption drying coincide with climatologically wet regions, while dry regions get wetter on average, but the changes are spatially heterogeneous. This pattern is of opposite sign to, but physically consistent with, projections under global warming.'

This sentence sounds a bit strange at first sight (consistent and yet of opposite sign). Perhaps reformulate as 'consistent with projections under increased greenhouse gas forcing, since volcanic forcing has the opposite sign'

4. I was also a bit surprised that regional modelling was very briefly discussed (just a couple of sentences), whereas other issues that are also not very well developed are considered in much more detailed was , for instance estimation of future drought risks. This may reflect missing regional climate modellers among the authors, but the manuscript gives the impression that regional climate modelling is considered not very important for simulations of hydroclimate variability or even for the comparison of proxies. Given the stress on the limited resolution of global models, this seems odd.

5. A few thoughts on data assimilation: data assimilation is indeed very important for weather prediction and in general for any kind of prediction. For this purpose, what counts is a skillful prediction and the understanding of the physical mechanisms remains a bit in the background. Thus it is permissible to violate the model physics with data assimilation if this really leads to an improvement (Data assimilation is also im-

portant to initialise the model, but I think this is not the issue here). My point is that, in the context of paleoclimate, the benefits of data assimilation are much less clear than in weather prediction. It may serve, for instance, to identify physical inconsistencies in the proxies (when the model 'rejects' to accept information from two diverging proxies, or it may help to physically explain a particular proxy configuration if DA is able to bring the model to a consistent state that is compatible with all proxy records. However, it is not that clear to me that DA can help ' assess the physical causes of past climate'. Actually, DA produces an output that deviates from what the model wants to provide, or when DA assimilation is used to drive the model towards the observed trajectory, it also bends, nudges, or combines model physics with observations. The results is certainly not as physically consistent as the raw model output, and it is unclear how it can help to understand mechanisms. It does help for predictions (or in this case reconstructions) though

6 . Line 795 ' warm climate intervals, and the mean state of the Indo-Pacific system. The paleoclimate record showed that the 20th century was actually dominated by a strong drying trend unprecedented in the last millennium, indicating that greenhouse gases may in fact be driving the East African region toward a drier state; the wetter prediction of the models associated with greenhouse gas forcing therefore deserves further investigation'

The paragraph sounds a bit strange. Why use the paleoclimate record in the 20th century ? The conclusion of this paragraph also sounds a bit weak (deserves further investigation), and this gives me the opportunity to comment on an aspect that seems to be missing in the whole manuscript. Are paleo reconstructions useful to identify 'bad models' that should not be used for climate projections ? Is this not what reducing projection uncertainty means in the end ?

7. Line 1080. It is nice to have the conclusions summarized in a few sentences, but the first conclusion looks unclear or weak: ' Expectations of temporal or spatial consistency between proxies and models should be critically evaluated' . I would suggest to clearly

state that temporal consistency can only be expected if and only if the variability is externally forced

8. Colose, C. M., LeGrande, A. N., and Vuille, M.: The influence of volcanic eruptions on the climate of tropical South America during the last millennium in an isotope-enabled general circulation model, Climate of the Past, 12, 961-979, 10.5194/op-12-961-2016, 2016a.

There is a typo in the doi of this paper. It should be cp and not op.

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