

Interactive comment on “Comparison of land–surface humidity between observations and CMIP5 models” by Robert J. H. Dunn et al.

Anonymous Referee #1

Received and published: 14 March 2017

The paper presents a comparison of climate model simulated humidity with that in a global observed dataset. As the authors mention, both specific and relative humidity are important quantities affecting health, productivity and resources. Therefore, the topic is important and relevant to the scientific community. However, there are some concerns, as I mention below, on the datasets and methods adopted in the paper that need to be addressed first. I am therefore recommending a major revision at this stage.

Firstly, unlike the abstract and introduction, the rest of the paper presents too much information (in many cases without stating the physical reasons or logically arising from the preceding section) that is hard to retain for the reader. Instead, one would expect concise sections which clearly explain in what aspects and why model simulations match with observations, where they do, and why they don't match with observations where they don't. For example, in Section 3: what causes the regional trends in tem-

Printer-friendly version

Discussion paper



perature which in turn influence humidity changes? While Section 4 states that water availability is not limited in the models, Section 6 states that the models are water limited to some extent: a contradiction?. Why do model simulations with greenhouse gases agree well with observations in the Northern Hemisphere (Section 4), and why do they don't agree for the Southern Hemisphere? Why does the T-q relationship in the models show better match with observations as compared to the T-rh? Moreover, all comparisons presented by the authors are either in the form of nature of trends or a plot of annual time series. What about the seasonal cycles and the spatial correlations?

Sections 4 and 6 each provide a helpful summary, while Section 5 does not have one. I am not sure how the addition of an atmosphere-only modeling experiment has added value to the analysis. Further, the authors present limited discussion on circulation patterns that play a major role in altering in drying or moistening of a region.

The observed dataset that the authors use are a single source global dataset that, as the authors themselves note, suffers from severe limitations including limited number of stations at several locations. How are the reported trends (or lack thereof) substantiated for such locations? And I wonder why satellite observations were not used in this study to augment the ground-based records. Moreover, for hydrometeorological variables, considerable disagreements exist between different reanalyses products. How does the other reanalyses products compare with respect to ERA-I and HadISDH? How could differences therein, if any, be resolved to arrive at a robust observed record of humidity in the first place?

I think it is a little unfair to expect climate models to represent relative humidity, particularly at regional scales, given that the models are not equipped with relevant physical processes at such scales. Isn't it already well-known to the climate science community that the GCMs are not the best tools for hydrometeorologic variables, particularly relative humidity? Therefore, the regional, inter-decadal and physical differences in humidity that the authors report are generic and obvious. It would be much more compelling to see the comparison between large scale atmospheric variables that are

[Printer-friendly version](#)[Discussion paper](#)

associated with humidity changes, in model simulations and observations so that the representation of such processes (and not humidity per se) may be improved in the next generation of climate models.

Further, since the authors are dealing with land-surface specific humidity and they suspect, perhaps correctly, that a major part of the unexplained differences are due to land-surface processes, would it not be logical to conduct a land-surface modeling experiment also, rather than that just using an atmosphere-only GCM?

The authors link the rising and falling trends in humidity with the so-called ‘hiatus’. It is worth mentioning that rising and falling trends lie very much within the natural climate variability in presence of long-term persistence (see Koutsoyiannis and Montanari, WRR, 2007) and estimating trends based on 10 or 15 years of data is not a statistically robust exercise at all. Therefore, the decadal trend comparisons (Fig. 1 – 4) are not the best metrics to consider.

Other comments: Section 3: ii) discussions about projections may be deleted to avoid confusion. Section 3: iii) the Indian region has marked drying trend in the last half-century, contradictory to the ‘moistening’ reported by the authors. Page 1, line 15: check grammar. Page 2, line 1: ‘Water vapor is the primary’: suggest replacing ‘the’ with ‘a’. Page 16, line 31: check grammar. Page 30, line 24: 2016 like to reach – we are already in 2017, therefore 2016 is not future anymore. Page 32, line 7: sentence is not clear.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2017-9, 2017.

Printer-friendly version

Discussion paper

