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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential evaporation trends over land between 1983–2008: driven by radiative or turbulent fluxes?" *by* C. Matsoukas et al.

Anonymous Referee #2

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This study investigates trends in potential evaporation over land between 1983 and 2008. The knowledge of magnitude and causes of trends in evaporation on global and regional scales is a critical and highly debated aspect of climate change, so this study touches upon an important issue. The authors model the evolution of potential evaporation over global land surfaces using the Penman equation and input from radiative transfer models and reanalyses. This approach allows to disentangle the importance of different forcing factors in the determination of the potential evaporation trends. The authors conclude that the changes in radiation fluxes, particularly their solar components, and not so much water vapor transfer considerations are the main drivers of those trends. I found the manuscript clearly written and the applied methods and ap-

proaches sound. I recommend the acceptance of the manuscript after revisions as outlined below.

Specific comments:

1. Of course it would be good if the trends in potential evaporation determined by the authors could be compared with observed trends in pan evaporation, which should be proportional as the authors state. While there is an extended literature on decreasing pan evaporation from the 1950s to the 1980s (largely prior to the period considered by the authors), I am not aware of too many studies with updated pan evaporation trends for more recent years as would be required in this study. Nevertheless I think it would be good if the authors could discuss what is known about trends in pan evaporation in the period of relevance to this study and how this would fit to their findings of increasing potential evaporation. The expectation from this study and some earlier studies would be that pan evaporation should have rather increased than decreased over these more recent years.

2. Although it is mentioned in the abstract, that a particular interest of this study is the temporal evolution of the potential evaporation, I cannot find a quantitative information of these change in absolute terms. Results are only shown normalized to the variability so we do not know the absolute magnitude of the estimated changes, in order to judge their climatological relevance. In that sense, it would be interesting, if the authors could provide, in addition to the trends based on the normalized values, trends in the absolute values to get a better idea on the magnitude of the changes under discussion (absolute magnitude of windspeed, radiative fluxes etc .). Therefore, an additional table similar to table 2 with the trends of the absolute values would be helpful.

3 . L. 347ff. The reason to start the ERA interim in 1989 was to exclude major adjustments in the satellite assimilation which lead to spurious changes. In the study the authors use data reanalysis prior to that already from 1983 onward. Could this cause some spurious trends? While inhomogeneity problems in the reanalyses data are dis-

cussed in this paragraph, potential inhomogeneity issues in the radiative transfer calculations should also be mentioned. The ISCCP data used as input, for example, have led to controversial discussions in the community with respect to their homogeneity.

4. This study is based on one specific surface radiation budget product and one reanalysis. Other comparable datasets for surface radiation (e.g. GEWEX SRB, ISCCP FD, University of Maryland SRB) and reanalyes (e.g. NCEP, MERRA) are available in the community. It would be very interesting to see how robust the findings in this study are with respect to a replacement of either the radiation or reanalyses datasets (or both) with other products as mentioned above. This is particularly relevant as studies point to considerable differences in the various products in terms of their representation of the temporal evolution of the quantities under consideration. The sensitivity of the obtained results with respect to these differences would be good to know. I am aware that such an analysis would go beyond the scope this study, but a brief discussion or an outlook in this respect would be useful.

5. The expression “interannual trends” used at various places throughout the text (e.g. abstract) sounds awkward to me and a contradiction in terms. Interannual timescales are too short for trends. I suggest to use “interannual changes” or “interannual variations”, or, if linear trends are applied, “decadal trends” or “secular trends”to be internally consistent.

Details:

L 30. There are also examples of GCM projections of both a warming and a drying world, particularly once aerosol direct and indirect effects are considered (e.g., Roeckner et al. 1999, J Climate).

L94. Six instead of five priorities according to the text that follows.

L163: I do not fully understand the meaning of G “energy flux advected to the surface”, from where is this energy flux advected?

L167: Does this imply that all surface radiative net energy goes into the latent heat flux? What about the sensible heat flux? Similarly L272: “If we assume that all this energy flux is used in evaporation”, is this a good assumption, can we entirely neglect the sensible heat flux?

L 371, how about a comparison to Vautard et al. 2010, Nature Geoscience 3.

Figures 4-6. It would be useful to add a zero anomaly line as a straight line into the Figures, so that the trends are better discernible, as they are not so easy to see.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10935, 2011.