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Earth System Dynamics Discussions

Interactive comment on "Strengthening of the hydrological cycle in future scenarios: atmospheric energy and water balance perspective" *by* A. Alessandri et al.

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General Comments:

In this manuscript the authors describe an investigation into the anticipated future energy and water mass constraints on the hydrological cycle in two 21st century climate model experiments. As far as I know this is the first time an Earth system model was used for such an analysis. The new results that are presented here are qualitatively consistent with a series of papers we published on the same subject. In one paper (Liepert and Previdi 2009) we used a similar Bowen ratio approach as described here (albeit with a somewhat different derivation, see "specific comments"). We focused our study on the differences of aerosol versus greenhouse gas forced changes in the hydrological cycle. Here the two scenarios of the model experiments are comprised of a 21st C mitigation strategy E1 and a medium scenario A1B. The importance of the mitigation scenario is the restriction of global warming to 2K. Hence the current study extends the previously published results by utilizing a state-of-the-art Earth system model and focusing on what will happen with the water cycle under global warming mitigation. This is an important contribution and worth publishing. The study however, has generally more potential than described in this text. The conclusion that a global stabilization below 2K comes with a significant increase in global precipitation over the mitigation period is an important message that could be discussed more prominently (see specific comments). A more thoughtful model description that focuses on the specifics that are analyzed in this study would be advantageous. The carbon cycle feedbacks are described in the "model" section but not referred to in the analysis and interpretation of the results. Land carbon cycle feedbacks are described at length but not ocean carbon cycle feedbacks. Is there a specific reason for this? Natural aerosol feedbacks in the model are also not described in spite of their importance for the conclusions. The testing of the model capability of simulating the water and energy cycle correctly cannot be underestimated since several studies have shown that model deficiencies of the order of climate changes exist in some models (Lucarini and Ragone, 2011; Liepert and Previdi, 2012). Please add some quality control statements to the text. Some information listed in tables is redundant in figures. Overall, in my opinion, the manuscript describes an interesting study of the temporal behavior of the water cycle under extreme global warming mitigation scenarios that is worth publishing after clarifying the method issues (see below), streamlining the model section of the text, and adding some more ESM specific analysis.

Specific Comments:

p525/25: We wrote the Feichter et al. 2004 paper as overview paper and wrote a companion paper Liepert et al. 2004 specifically on precipitation changes under GHG

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and aerosol forcings. This would be the more appropriate citation.

p526/6-8: Again precipitation sensitivity is more extensively described in several other papers (see Wentz et al. 2007 for observation discrepancies with models. In Liepert and Previdi (2009) we explicitly showed that precipitation in coupled GCM is more than three times more sensitive to aerosols compared to GHGs forcing.

p528: It is not clear why the land surface vegetation component of the model is described in such details here. It would be more interesting to list aerosol-climate interactions (emissions of natural aerosols) and greenhouse gas feedbacks in ocean and land.

p529/8-10: Why is land carbon uptake described and not the ocean carbon uptake as well?

p531-2, (5-10): It is not clear that the total potential energy budget of the atmosphere is the best approach here. Enthalpy changes are calculated in (6) but include only LP as source term of latent heating. A sink term LE of latent heating should also be included (see Peixoto/Oort). Sensible heating of the atmospheric column as defined in (7) is separated into a source term and a horizontal advection term. Again shouldn't there be a sink term?

In Liepert and Previdi (2009) we avoided these issues and unnecessary assumptions such as neglecting kinetic energy, by using the surface energy balance equation instead of the atmospheric total potential energy equation. We derive an equation similar to (10) by combining atmospheric moisture budget and surface energy budget. The one difference is: the change in sensible heat convergence is replaced by the change in latent heat convergence. (Note, in a global mean we consider latent heat convergence as small compared to evaporation and precipitation and neglected it in our equation 3. It can easily be added here.)

p533, (11): In Liepert and Previdi 2009 we also derive a modified Bowen ratio (here

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Bowen ratio potential) that is defined by: atmospheric radiative flux change, the moisture convergence change but not the sensible heating convergence change as equation 11. I am not convinced the sensible heat convergence change is correct in eq. 11. We interpret the distinctly different behavior of the modified Bowen ratio for an aerosol only, a GHG only, and a combined GHG and aerosol experiment with a fully coupled GCM. The modified Bowen ratio changes signs in our experiments, which is also mentioned in the text as bifurcation.

p533: The discussion of aerosol effects is difficult to comprehend from Figure 4 alone. It is even unclear whether Figure 4 includes natural sea salt aerosols. More information on natural aerosol feedbacks and anthropogenic aerosol forcing is needed. For example: Does natural aerosol burden change with increasing global warming, e.g. more forest fires?

Fig.1: Not sure whether this figure is necessary.

Fig.2: Please see comments above. There is no sink term for latent and sensible heating in the atmospheric column.

Fig.4: This is only sulfur burden. How about carbonaceous aerosols and other anthropogenic aerosols? Does Fig.4 include sea salt aerosol? How about a figure for total (natural and anthropogenic) aerosol burden?

Fig.5: In my opinion one of the key results of this study is that in the mitigation scenario E1 global warming can be constrained to 2K but precipitation continues to increase beyond 2070. This behavior could be shown more clearly in a separate figure.

Fig.8: The information here is redundant and is better presented in table 2.

Fig.10: Again I think this information fits better in table 3.

Fig.11: Please see comments above. I find this diagram more confusing than explanatory. Is there a better way to convene the message?

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Technical Corrections:

p530/3: "The water vapor content of the atmospheric column is a balance . . ."

p533, 12-15: This result is originally from Liepert et al. 2004 and not Feichter et al. 2004.

p541/25: see Liepert and Previdi 2010 which shows this result explicitly.

References:

Liepert B.G. and M. Previdi, 2012: Inter-model variability and biases of the global water cycle in CMIP3 coupled climate models, Environ. Res. Lett. 7 (2012) 014006.

Liepert, B. G., and M. Previdi, 2009: Do models and observations disagree on the rainfall response to global warming? Journal of Climate, 22, 11, 3156–3166.

Liepert, B., J. Feichter, U. Lohmann, and E. Roeckner, 2004: Can aerosols spin down the hydrological cycle in a moister and warmer world? Geophy. Res. Lett. Vol 31, L06207, doi: 10.1029/2003GL019060.

Lucarini, V., and F. Ragone (2011), Energetics of climate models: net energy balance and meridional enthalpy transport, Rev. Geophys., 49(1), RG1001.

Wentz, F.J, L. Ricciardulli, K. Hilburn, and C. Mears, 2007: How Much More Rain Will Global Warming Bring? Science 317 (5835), 233-235, DOI:10.1126/science.1140746.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 523, 2012.