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Andrea Alessandri

Italian National Agency for New Technologies, Energy
and Sustainable Development (ENEA), Rome, Italy

International Pacific Research Centre (IPRC),
University of Hawaii at Manoa, Honolulu, HI, USA.

Review of MS No.: esd-2013-9
MS Type: Research Article
ESD Title: The sensitivity of the energy budget and hydrological cycle to CO2 and solar forcing.
Author(s): N. Schaller, J. Cermak, M. Wild, and R. Knutti.

This paper studies the transient response of the energy budget and the hydrological cycle to CO2 and solar forcing, both globally and zonally. To this aim, a set of idealized transient simulations are designed and performed using the NCAR Community Climate System Model version 3.5. The paper analyzes the transient response and compares the sensitivity to CO2 and solar forcings. The main aim is to test the assumption that the response to the forcings is linearly additive, i.e. wheter the response to individual forcings can be added to estimate the response to the combined forcing.

Main comment:
This paper designed a novel set of transient simulations to specifically test the assumption that the response to the forcings is linearly additive. As far as I know this is the first time a state-of-the-art CGCM was used for such an analysis and by using specifically conceived ad-hoc transient simulations. The authors show that, for the global climate model considered, the responses of most variables (regardless of the sub-region considered) of the energy budget and hydrological cycle, including surface temperature, do not add linearly. This has important implications for policy makers, who often use lower complexity models, which behave most likely following the linear additivity assumption.

Overall, the manuscript describes an interesting study and contains original results that are worth of publication. However, there is room for further improvements by (i) clarifying the text, (ii) better explain the method in relation to the equations of the energetics in the atmosphere (see comments) and (iii) possibly by adding some further analysis. I recommend publishing this paper after return for some modifications to address the specific points reported in the following:

-In this work it is used only one CGCM. Please, state that the results are likely to be model dependent.

-The title appears too general and should better reflect what is the new contribution of the paper.

-In the introduction (page 3, lines 10-12): “Precipitation, and its energy equivalent, latent heat, are variables that belong to both the energy budget and hydrological cycle (e.g. Bosilovich et al., 2008), hence the need to analyze them jointly.”
In this respect, Alessandri et al. (2012) developed a method to analyze the precipitation change that is based on both water and energy conservation principles in the atmosphere. The method generalizes the approach in Liepert and Previdi (2009) as it can also be applied to regional domains and not only to the global average. It is suggested to use the method developed in Alessandri et al.(2012) in this work to possibly strengthen the outcomes and the conclusions of the paper.

Alessandri, A., Fogli, P. G., Vichi, M., and Zeng, N.: Strengthening of the hydrological cycle in future scenarios: atmospheric energy and water balance perspective, Earth Syst. Dynam. Discuss., 3, 523-560, doi:10.5194/esdd-3-523-2012, 2012.

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

- Introduction (page 3, lines 15-18): “It is widely accepted that global mean precipitation change per unit temperature change is more sensitive to changes in solar radiation than to changes in CO2 concentrations (Allen and Ingram, 2002; Gillett et al., 2004; Andrews et al., 2009; Bala et al., 2010).”

It should be also mentioned here the relation between aerosols and solar forcing in the context of the already performed climate scenario studies (comparing the sensitivity to GHGs and Aerosols). The importance of the anthropogenic sources and the possible mitigation appears of particular relevance. For instance, Liepert and Previdi (2009) explicitly showed that the precipitation in coupled GCM can be more than three times more sensitive to aerosols compared to GHGs forcing. Alessandri et al (2012) warns that mitigation policies that promote aerosol abatement, may lead to an unexpected stronger intensification of the hydrological cycle and associated changes that may last for decades after global warming is effectively mitigated.

-Introduction (Page 4, lines 16-29 and page 5, lines 1-13): I’d suggest to put this discussion before stating the aim of the paper (page 4, lines 10-14).

-Section 2: A table summarizing the main characteristics of each transient simulation performed would be very helpful.

-Section 3.1: Fig.2 introduced in the text before Fig.1;

-Section 3.1 (page 9, line 11): “The values shown represent non-linearities arising from long-term feedbacks.”

Please, try to clarify and discuss further this sentence.

-Section 3.2 (Page 10, lines 15-16): “First, scaling the responses would make the assumption that each variable at each grid point scales linearly with the adjusted forcing. While scaling the responses of diagnostic variables might be justified, other quantities such as the zonal mean profile of specific humidity or residence time of water vapor in the atmosphere cannot necessarily be scaled with the adjusted forcing.”

Not clear, I cannot understand. Please consider substantial revision.

-Section 3.2 (page 10, line 26): Please consider revision. E.g: replace “due to the fact that” with “indicating that”.

-Section 3.2 (page 14, lines 9-11): “In addition, changes in surface temperature are larger in CO2 scenarios, which in itself causes a larger LW back radiation, and consequently larger increases in water vapor.”

Causes and effects are mixed here. The direction of causality is not clear. Please revise or remove text.

-Page 14 (line 13): “Changes in global annual mean precipitation can be understood either from an atmospheric (Mitchell et al., 1987; Allen and Ingram, 2002)”

Please, cite Liepert and Previdi (2009) and Alessandri et al. (2012).

-Page 14 (bottom): ”and some of this excess energy will be taken up by the ocean ΔNET_{surf}.”

Please replace with “and the excess energy will be taken up by the ocean or land surface (ΔNET_{surf}).”

-Section 3.3.1 (page 14, lines 27-28 and page 15, lines 1-10):

The changes in partitioning at the surface between latent and sensible fluxes are strongly coupled through the surface energy balance. Specifically, LH and SH compete for the available energy and this is not considered adequately in the text when analyzing SH decrease. For instance, over oceans and wet lands (and increasingly towards equator) the increase in temperature is expected to affect more LH (since potential evaporation is proportional to saturation specific humidity at the surface; i.e. exponential function of Temperature) with respect to SH (linearly increasing with temperature). Therefore, the larger SH decrease over ocean appears more consistent with the fact that LH is at its potential value there [I suppose deltaLH largely dominates and deltaSH acts by partially compensating the LH increase].

Surface warming is present in all transient simulations (see Fig.1), while SH is seen to mostly decrease. Therefore, we can hardly infer causality as in following sentence: “over land, SH slightly increases (around 1Wm-2) due to surface warming”. Conversely, much

colleagues would probably believe causality is going in the opposite direction (i.e: surface temperature increases more where LH do not dominate, e.g: over land with large SH/LH; See Sutton et al., 2007).

I'd like to encourage the authors to consider using the method in Alessandri et al (2012) when analyzing the change in partitioning at the surface between SH and LH (Bowen ratio potential; equation 11 in Alessandri et al., 2012), both globally and zonally.

Sutton, R. T., Dong, B., and Gregory, J. M.: Land/sea warming ratio in response to climate change: IPCC AR4 model results and comparison with observations, Geophys. Res. Lett., 34, L02701, doi:10.1029/2006GL028164, 2007.

-Section 3.3.2 (Page 16, line 13): please replace "heat balance" with "energy balance".

-Section 3.3.2 (Page 16, line 18): please replace "decreases" with "is expected to decrease".

-Section 3.3.2 (Page 17, lines 14-19): "Focusing again on the differences between CO2 and solar scenarios, the changes in convective precipitation seem to follow the changes in surface temperature shown in Fig. 1b–d. Convective precipitation increases more in the tropics and mid-latitudes in solar scenarios due to the stronger warming while in the high latitudes, the convective precipitation response is larger in CO2 scenarios due to the stronger polar amplification in these scenarios (see Figs. 5c and 1d)."

Consider revision, e.g. as follows:

"The different changes in convective precipitation, between CO2 and solar scenarios, seem to follow the changes in surface temperature shown in Fig. 1b–d. Convective precipitation increases more in the tropics and mid-latitudes in solar scenarios due to the stronger warming, while in the high latitudes the convective precipitation response is larger in CO2 scenarios (see Figs. 5c and 1d)."

-Section 3.3.2 (page 18): Please, define and explain briefly meridional temperature gradient (MTG).

-Section 3.3.2 (page 18, 15-29 and page 19):

(i) Please, replace "poleward energy transport" with "energy convergence at the mid-high latitudes"

(ii) I recommend you to give a rigorous mathematical derivation of the energetics here, when discussing the atmospheric energy convergence at the mid-high latitudes. Please, show equations and motivate the simplifications and neglected terms in your computations. [See for instance equation 10 in Alessandri et al (2012). You can obtain atmospheric energy convergence easily by substituting equation 4 in equation 10.]

(iii) This treatment of the energy budget is better suited for Section 3.3.1. I suggest moving text. Instead the atmospheric water budget and the combination of both water and energy principles can better be used in 3.3.2 (see iv).

(iv) It is suggested to use the analysis as in Alessandri et al. (2012) to strengthen the outcomes and the conclusions of the paper. The method in Alessandri et al. (2012) is based on both water (its equation 4) and energy conservation principles (its equation 10) in the atmosphere. It can be applied to regional domains as well as to the global average. Applied to the mid to high latitude zonal band, the method allows to answer why "large-scale precipitation increases more in solar scenarios compared to CO2 scenarios at higher latitudes".
Note that, by substituting equation 4 in equation 10 of Alessandri et al (2012) you can obtain the following equation for the computation of the moisture convergence.

$$\Delta\{-\overline{\nabla_h \cdot Q}\} = \frac{1}{L}(-\Delta\{-\overline{\nabla_h \cdot SH}\} - \Delta\{-S_{net}\} - \Delta\{-T_{net}\} - \Delta\{\overline{SH}_{\uparrow}\} - \Delta\{L\bar{E}_{\uparrow}\})$$

Please, also note that $-\Delta\{-\overline{\nabla_h \cdot SH}\}$ can be obtained as the residual using equation 10.