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Earth System Dynamics Discussions

Interactive comment on "A simple explanation for the sensitivity of the hydrologic cycle to global climate change" *by* A. Kleidon and M. Renner

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We thank Dr. Allan for his constructive comments and pointing out the relevant publication by Li et al. (2013). Here, we want to briefly respond to his comments, although most points have been addressed in the responses to the other reviews. In the following, the comments are included in italic typeface, followed by our response.

Comment: *Not being familiar with the maximum power concept, I consider that the impact of this work could be somewhat enhanced by explaining this more fully in terms of physical processes. For example, it is often argued that enhanced radiative cooling of a warmer atmosphere, constrained by thermodynamics and radiative transfer, is the fundamental control on global precipitation changes; stronger radiative cooling is primarily balanced by more latent heat flux (e.g. O'Gorman et al. 2012, Surv. Geo-*

phys). This of course is manifest through both atmospheric energy and surface energy balance processes. Specifically, more evaporation can only be sustained if this evaporated water is efficiently removed from the boundary layer through uplift into the free troposphere, determined to a large extent by radiative convective balance. Therefore some discussion of how the surface energy balance parameters chosen and the maximum power assumption are physically linked with the enhanced radiative cooling of a warmer troposphere would be valuable in making the results of the study accessible to a broader section of the climate community.

Response: We agree that we should better explain the maximum power limit, as pointed out by the other reviewers as well, because it is not well established in climatology. In terms of the common explanation for the hydrologic sensitivity, we refer to our reply to review #2, who raised the same question, and where we provide a response in terms of the atmospheric energy budget (http://www.earth-syst-dynam-discuss.net/4/C446/2013/esdd-4-C446-2013.pdf). What we have shown there is that the increase in latent heating of the atmosphere in our model is compensated for by the decrease in the sensible heat flux (if the warming is only due to a change in the greenhouse effect). We will include a more detailed explanation of the maximum power limit and its derivation and a discussion regarding the common explanation for the hydrologic sensitivity in the revision.

Comment: *p.855, line 13: discussion of the paleoclimate hydrological sensitivity would benefit from reference to the study of Li et al. (2013) GRL.*

Response: Thanks very much for pointing out this reference. This reference is relevant also because it interprets changes in the hydrologic cycle in terms of the equilibrium evaporation rate, which is very similar to our interpretation. We will include this reference in the revised version of the manuscript.

Comment: *Section 2: I found this section challenging. While the reference to KR13a is useful, a little more explanation of the terms and their physical meaning would be* *beneficial.*

Response: We will do so in the revision.

Comment: *Equation 2: The simple model assumes an opaque atmosphere of temperature T_a. In reality of course, emission to space originates from various levels of the atmosphere depending on the opacity of the spectral region. Indeed in cooler, drier climates, some emission can originate from the surface in window regions of the spectrum. Have the authors tested the sensitivity to this assumption?*

Response: We have not done such an analysis in the context of KR13a and this manuscript. We are currently evaluating our approach in more spatial detail on land, and generally find that the estimates hold rather well when compared to observations. Yet, there are, of course, regional derivations that can be explained by some of the aspects that we do not consider. One aspect is the assumption of an opaque atmosphere, as mentioned in the comment, which does not necessarily hold for dry (or cold) regions, another aspect is the local assumption, which does not apply to regions in which there are significant rates of atmospheric heat transport. In the revision, we will include some discussion on the assumptions that we have made and their potential implications.

Comment: *Equation (3): The optimum vertical exchange velocity parameter, w_{opt}, is derived in the authors previous work KR13a but some more physical explanation would be useful here.*

Response: This velocity can be interpreted as the wind speed of vertical convection that is caused by local surface heating. Its order of magnitude fits well with the typical values, e.g. as given in Peixoto and Oort (Physics of Climate, 1992). We refer to it as an exchange velocity because it accomplishes the exchange of heat and moisture in our two-box model. We will clarify its physical interpretation in the revision.

Comment: *Equation (4) I was unsure why the surface longwave radiation, $R_{l,opt}$ is*

equal to half the shortwave flux, R_s? Although KR13a find realistic energy fluxes, the R_l seems rather large compared to values estimated from observations, about 45 W m^{-2} (Wild et al. 2013 Clim Dyn). Is this an artifact of the assumption that there is no solar absorption by the atmosphere?

Response: The factor of 1/2 results from the maximization of power. We will refer the derivation of this limit in the appendix of the revised manuscript. The high value of $R_{l,opt}$ in our model results from two factors. First, as mentioned, we assume that all absorption of solar radiation takes place at the surface, while observations (e.g. Stephens et al. 2012, Nature Geoscience) suggest that it is only 165 W m^{-2} rather than 240 W m^{-2}. We used this assumption to keep the model as simple as possible (otherwise, we would need to account for atmospheric absorption in the expression for T_a). For the hydrologic sensitivity it plays a minor role, because the sensitivity is formulated in relative terms, which is independent of R_s (at least the first term in eqn. (7), $\partial E/\partial T_s$, as seen in eqn. (8)). The second bias in our estimate probably originated from not considering the effects of the large-scale circulation, which adds extra turbulence to the surface, thus shifting the partitioning towards turbulent heat fluxes (which in the framework of the equilibrium evaporation rate can be interpreted as the Priestley Taylor coefficient). As long as this shift is independent of T_s, the relative sensitivity is nevertheless not affected.

Comment: *Some diagrams illustrating how the flux terms respond to a warming or change in greenhouse effect may be useful for the reader. In reality there is a strong decrease in R_l with warming as the atmosphere becomes more opaque with extra water vapour. Is this represented by the model (this is implied on p.863, line 20-23)?*

Response: Yes, the change in greenhouse strength is indirectly accounted for. In fact, this is the way by which the surface temperature is increased in the model for a fixed, given value of R_s. This can be seen in eqn. (6), where a lower value of k_r results in a stronger greenhouse and a warmer surface temperature T_s for a given value of R_s. We will try and think about how the changes in the energy balance and water cycle can be put in a diagram for the revision, which would supplement the discussion of the sensitivities in the discussion section.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 853, 2013.