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A general framework for understanding the response of the water cycle to global warming over land and ocean

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Response to Referee Comments by Dr van der Hurk

Referee comments in Italics

1. *This manuscript describes in a clear and easy to follow manner the effect of green-house warming on the global balance of P and E, elaborating on earlier work by Held and Soden (2006). It shows that the zonally averaged estimates of d(P-E) do not hold at the grid box level and not over land, and present an alternative Budyko-based framework for this. It also illustrates the small role of changes in surface evaporation, and the dominance of longwave cooling at the surface. It reverts the (public) rationale that elevated temperatures lead to acceleration of the hydrological cycle: had surface evaporation increased more, the surface warming would have been correspondingly less.*

We thank the reviewer for the positive comments.

2. *The main question that then remains unanswered is the physical explanation of the current partitioning of dLin over dLout and dE: why is x in Eq 10 equal to 0.24? It probably is related to the notion that relative humidity changes relatively little, but it does not shed light on why this is the case. Do the authors have evidence for this?*

Very good point.

We did try but we were unable to find a definitive underlying physical reason for that finding. We agree that the so-called water vapour feedback is likely to be central but in the absence of a physical explanation the only option is to leave it as a model result that requires future explanation.

3. *The paper reads very well (very suitable for teaching at MSc/PhD level) and is certainly suitable for publication, apart from a number of minor comments:*

We thank the reviewer for the positive comments.

4. *p 15270: the number of references to the Budyko framework (19) is a bit overdone, I would say.*

When we originally asked an experienced atmospheric sciences colleague to comment on the work they responded by stating something along the lines of … "the Budyko thing must be pretty good because it agreed with climate models". In fact the reality is the other way round with the test being whether the climate models were consistent with hydrologic understanding embodied in the Budyko approach. With that background we wanted to ensure that readers not familiar with Hydrologic applications of the Budyko approach could peruse a variety of literature to see the generality of the approach. In retrospect we could shorten the reference list in line with the suggestion.

5. *Eq 1: can you give some indication of the typical value of n?*

The value of n that reproduces the original Budyko curve is 1.9 (Donohue, Roderick, McVicar, 2011 J Hydrology 406: 234-244). We used n = 1.8 in Figure 2ef as noted in the caption. In previous work we have calculated values of n using long term catchment data in Australia and found values ranging from 0.6 to 3.6 (op cit). That range has also been found in Chinese catchments. We can modify the manuscript text near Eqn 1 to indicate the values (and range) for n as per the above.

6. *p 15271, second para: the fact that the models obey the Budyko framework so well is not very surprising, as both are based on conservation of energy and water. A model data set can be expected to better comply with this framework than an observational data set where energy and water balance conservation are normally quite problematic*

We accept that point but we are still reassured by the excellent agreement.

7. *p 15272, last sentence: add a statement that the framework is not suitable for the cryosphere since additional long-term mass balance terms (storage in snow/ice packs) violate the balance assumptions*

We can modify the sentence to add the reasons noted by the reviewer.

Original sentence reads:

The Budyko framework is not intended for use in the cryosphere and we limit the calculations to the latitudinal range 60S to 60N.

To be replaced with:

The Budyko framework is not intended for use in the cryosphere since additional long-term mass balance terms (snow/ice) violate the mass balance assumptions. We limit the calculations to the latitudinal range 60S to 60N.

8. *p 15273: the 82% explained variance shown in fig 4 is actually a bit low, since all terms in eq 3 come from the same model archive. The missing terms that explain this limited fraction of explained variance are the (ignored) changes in n and the covariance terms, is that right?*

Yes, that is correct.

9. *p 15279, L15: add the word "averaged" before "model ensemble", as the word "ensemble" often points at a large collection of model data*

Good point. We also located several other instances of the same problem in the text (e.g., P15266, line 18; P15270, line 8 & 18; P15271, line 13; P15274, line 10; P15275, line 7 & 13; P15277, line 18; P15278, line 18; P15280, line 18; P15279, line 21) where we will modify the text as per the suggestion.

10. *it is a coincidence that the fractional changes in dP and dE over land (5.3 and 3.7) add up to the fractional change in d(P-E) (9%). It may be helpful to make this notion*

We did not notice that originally, but yes, it is a (peculiar numerical) coincidence (in Table 1).

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