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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.

1. *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.

1. *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.

1. *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 curve. With that background we wanted to ensure that readers not familiar with Hydrologic applications of the Budyko curve could peruse a variety of key literature to see the generality of the approach.

In response we have removed two references from the list and note that we would greatly prefer to keep the remainder as they represent some of the key works that readers could follow up.

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

The value of n that best 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 modified the text following Eqn 1 to address the questions raised.

1. *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*

As noted by another reviewer (Sivapalan), it is not so straightforward. While the asymptotes may be straightforward the approaches to them are not. The curvature in the Budyko curve is an emergent relation and depends on many things (e.g. water, energy, soil-vegetation, etc.). No change was made.

1. *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 modified the sentence as per the reviewer’s suggestion.

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.

We replaced that sentence 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.

1. *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. In addition, the explained variance would have increased if we used the best fit value for n (=1.5) instead of the recommended default value for n (= 1.8).

1. *p 15279, L15: add the word “averaged” before “model ensemble”, as the word “en-semble” 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).

We have fixed all instances of this problem throughout the text.

1. *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).

**Response to Comments by Referee (Prof. Savenije)**

*Referee comments in Italics*

*This is a very interesting and well prepared paper, which clearly shows that interpretation of climate change effects only make sense if we distinguish between land and ocean, where the land is moisture constrained whereas the ocean is not. The Budyko framework is a very useful way to analyse the sensitivity of the hydrological fluxes to changes in energy and precipitation input. This paper is brilliant in its simplicity, and I very much welcome a resubmission.*

We thank the reviewer for the positive comments.

*There are two sets of comments that I would like to make. The first set refers to the correct use of units, the second to an additional feature that becomes clear if a different functional form of the Budyko curve is chosen.*

*1a. In the paper the evaporation is sometimes expressed as a volumetric flux per unit area (mm year) and sometimes as an energy flux per unit area (W m-2). In the latter case the evaporative flux is symbolized by LE. Although the paper doesn’t say so, the convention for L (Specific latent heat) is the energy (heat) required to evaporate 1 kg of water (=2.45 kJ/g). It then still requires multiplication with the density to become a volumetric flux per unit area. So the equation should use ρLE for LE (if E is expressed in a volume flux per unit area, as the paper does). Unless of course L has the unit J m-3. So preferably include the density, or explain that L includes the density. This also applies to all the places where LE or LE are used (eqs. 5, 6, 7, 8, 11, 12; p15275, L10, L19; p15276, L1, L5; p15277, L8 and caption of Table 2).*

Good point. We use common hydrologic units (volume per area per time) but we agree that it is dimensionally inconsistent with the equations we use. The equations themselves are dimensionally correct – it is in the tables and in the Budyko analysis where we make this dimensional error.

We have now added a new paragraph to section 2 explaining the unit convention adopted.

*1b. Another mistake often made is that G in eq.(5) is not the storage of heat, but the storage increase over time. It is the temporal derivative of the heat storage and not the storage itself, as is suggested on p15274, L7 and in the caption of Table 2. This may seem like nitpicking, but in HESS we want to be precise in the correct use of units and dimensions. Of course one might say that the term storage means the process of storing, but this is not what in hydrology is the convention. The storage of water is the water stored. The term dS/dt in the water balance means the temporal change of storage and not the storage; similarly G is the temporal change of heat stored in the earth and not the amount stored. The temporal derivative of the storage is the process of storing or depleting. Maybe a way out is to replace the term ’heat storage’ into ’the process of heat storage’, or to replace it with ’heating up’.*

Good point. We agree. We have refined our description and now use change in enthalpy for *G* throughout.

*2. The authors prefer to use a Budyko curve of the type presented in (1). A simpler form of the Budyko curve is*

**

*This exponential equation may have the disadvantage that it does not have the additional parameter n, which allows additional tuning, but the authors don’t make use of n anyway. An advantage of this equation is that it links to the probability distribution of rainfall (see: De Groen and Savenije, 2006, and Gerrits et al., 2009), and hence has some physical reasoning behind it. A further advantage of this equation is that εo and εp have physical meaning. It can be shown that εo equals the runoff coefficient:*

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*This follows simply from partial differentiation of the exponential Budyko curve. In fact,*

*Figure 3b shows the global distribution of the runoff coefficient. If the colour scale is*

*changed to a maximum of 1.0 (now it is scaled at a maximum of 16\*10-1=1.6, which is*

*a physically impossible number), then we recognise immediately the distribution of the*

*runoff coefficient over the world. I think using the exponential definition of the Budyko*

*curve makes the paper even more transparent. In addition it can be shown that:*

**

*So εp is proportional to the runoff coefficient and is strengthened by the aridity index. These expressions can also be obtained directly by derivation of the exponential Budyko curve:*

**

*or:*

**

*We see that the main control on runoff change is the runoff coefficient itself. The larger the runoff coefficient, the larger the runoff increases with increasing precipitation. Further, since the change in the potential evaporation is not so large, the runoff change is affected by the aridity index E0=P. The aridity index strengthens the sensitivity of runoff to rainfall.*

Very good point/s and a very clear commentary. We first note that this sensitivity framework is not actually in the cited references – as far as we are aware it is presented in this particular review for the first time! We agree that the resulting partial differentials are easier to interpret. We have also plotted the suggested form for the Budyko curve. In the modified Fig. 2e (below) we show the new form suggested by Prof Savenije:



Modified Fig. 2e New form of the Budyko curve is shown as a full heavy line and falls slightly below the original dashed line.

We conclude that the Savenije equation actually follows the data more closely than our original default equation. If we modified our default equation to use n=1.5 (as noted in the original figure caption) then it would be more or less the same as the Savenije equation.

Thus the new framework offered by Prof Savenije has some clear advantages. Given that we chose not to use the tunable parameter (n) it was suggested we could use this new and simpler form. Alternatively, in real catchments the additional parameter is actually needed to adequately account for the observed variation (Donohue et al 2011 J Hydrology) and a theoretical basis is currently being developed (Roderick & Farquhar 2011 WRR, Donohue et al 2012 J Hydrology) to express that parameter in terms of standard hydrologic quantities; mean storm depth, soil water holding capacity and effective rooting depth of the vegetation. That form also has a strong theoretical basis in terms of satisfying mathematical boundary conditions (Yang et al 2008 WRR) although we expect that the Savenije form is also likely well behaved at the boundaries.

We conclude that the presentation by Prof Savenije is novel and worthy of publication in its own right.

In response we have added a new Appendix to include this new derivation with appropriate modifications in the main text. The appendix includes a new figure (Fig. 7) that shows the excellent agreement with model output.

Further, we were aware that the maximum physical value is 1 in Fig. 3b. We simply could not draw the figure appropriately using our software. We have now tried harder and have made colour scales in Fig. 3 that scale from 0 to 1 and are more suitable from a physical point of view.

**Response to Anonymous Comments by Referee 3**

*Referee comments in Italics*

1. *Overall comments: This is a well written and clear article, which addresses an important research topic in climate science (why changes in precipitation under enhanced greenhouse gas concentrations are not changing at the Clausius-Clapeyron rate). It also highlights some potential pitfalls when applying rules derived (and valid) for latitude bands including both land and ocean areas (proportionality of changes in P-E to background P-E) to changes in land-only areas or to local changes over the oceans. Furthermore, it highlights an important misconception, i.e. that it is evapotranspiration that drives temperature changes under enhanced greenhouse gas forcing rather than the opposite, because most of the enhanced incoming longwave radiation is used for enhanced outgoing radiation at the surface. I have only minor comments on the article (see below) and thus recommend it being accepted subject to minor revisions.*

We thank the reviewer for the positive comments.

1. *Detailed comments: 1) P. 15264, lines 17-20: The construction of this sentence is a little convoluted and confusing. The three terms that are referred to should be more clearly highlighted and recognizable upon first reading. In addition, the sentence should note that most of the evapotranspiration changes occur over the oceans rather than land areas. Here is a suggested revision of this sentence: "In terms of global averages, we find [that] the climate climate model projections are dominated by changes in only three terms of the surface energy balance: 1) an increase in the incoming long-wave irradiance, and the respective responses in 2) outgoing longwave irradiance and 3) evaporative flux (the latter being much smaller than the other two terms and mostly restricted to the oceans).*

That is better. We will incorporate that into the revision. Thank you.

1. *2) P. 15274, line 11: The following paragraph is mostly based on the numbers in Table 2, but I found Fig. 5 more straightforward to interpret upon first reading. I would suggest to add in the parenthesis "(Table 2)" also a reference to Fig. 5, e.g: "(Table 2; see also Fig. 5 for a summary of the changes between the two periods)".*

Again – that is better. We will incorporate that into the revision. Thank you.

1. *3) P. 15275, line 6: Add "over the oceans" after "in the latent heat flux" (same comment as 1): most of the changes in evaporation occur over the oceans)*

Agreed and done.

**Response to Comments by Referee (Prof. Sivapalan)**

*Referee comments in Italics*

*I enjoyed reading the paper: some time ago eminent physicist S. Chandrasekhar published a voluminous book titled "Principia Mathematica for the Common Reader", which was still over my head (in spite of written in English as opposed to Newton’s Latin). This paper could be termed "climate change for the common hydrologist": just like some eminent climatologists are wont to do (e.g., Ramanathan), it aims to capture the bare essentials so the common hydrologist reader can get to the bare essentials. I want to add a few more comments and suggestions that the authors can make this even better.*

We thank the reviewer for the positive comments.

1. *I felt that the authors went too far in simplifying to the point some parts of the text seem rather cryptic. In view of the potential educational value of this paper, it may be more useful to make this a bit more clear. I give an example: the sentence "... the atmospheric humidity is projected to increase at the Clausius-Clayperon (CC) value of around 7%/K". A similar statement is made later about P, which was clear, but I was confused by the CC reference here.*

We were unsure of the point being made. We think it refers to the spelling mistake (should be Clapeyron) and we can fix that. When we came to correct the spelling we could not locate a spelling mistake in the manuscript?

*(2) I can follow the arguments on the authors’ interpretation and clarification of the results of Held and Soden. However I am unclear about the take-home message from this. Is the message meant for climate scientists or for hydrologists? As a hydrologist, I don’t know what to make of this for my work - may be the authors can clarify.*

The message is for both atmospheric scientists and for hydrologists. In particular, one often hears the statement in the press or even in scientific papers that the wet get wetter and dry get drier. However, as we note, in terms of *P*-*E* at least, climate models in the CMIP3 archive do not actually project this at the local scales that matter for impacts and for management. Those local scales are of primary interest for hydrologists.

After reading this comment and comment (6) below, we think that the problems highlighted by the reviewer arise because of the abstract.

In response we have completely rewritten the abstract to highlight key results for hydrologists and for atmospheric/climate scientists along the lines we indicated in the original response to the review.

*(3) I will say something similar about the authors’ findings about Budyko. It is clearly reassuring that climate models "on average" satisfy the Budyko theory of annual water balance partitioning. Is this the take home message? I agree that this is important: some 20 years ago during the PILPS experiment (inter-comparison of land surface parameterizations) that climate models did not satisfy Budyko, which was a major concern. In spite of the good result, I remain curious - how did this happen? Unlike the comments of one of the reviewers, this is not just a matter of balancing water and energy: it is about co-evolution of climate, soils, vegetation etc. Any insights by the authors would be very valuable.*

*(4) One more query on the Budyko: again, what is the take home message? Is it that climate models are now able to satisfy Budyko "on average"? Of course they should, if they are to be used with confidence? I am wondering if there is a deeper message here.*

These (3 & 4) are good questions. We were aware of earlier results with PILPS and we admit to being (pleasantly) surprised by the finding that the model ensemble average does follow the standard Budyko curve. As the reviewer implies, that result should give a clear indication to hydrologists that the climate models are not without some skill in terms of the partitioning of water and heat at the surface. As to deeper insights - we really do not know why. We assume that the models strictly enforce mass and energy closure schemes over the land surface. That would explain the close adherence to the water and energy limits but it is a little more difficult to explain why the model ensemble average gets the apparent curvature correct as well. This is particularly interesting since we know from work by Prof Graeme Stephens and others that the rainfall dynamics are not well simulated by climate models. That remains an important research question.

*(5) Compared to these interpretations (above), to me the more interesting conclusion of climate models is for a global increase of P by around 1-3 %/K. Isn’t this the essence of the "response of the water cycle to global warming" (from the title of the paper). I was expecting that the paper would also address this point, as this would be of a lot of value to hydrologists. I looked for discussion of this and did not find it (or did I miss it). I felt that the second part of the paper skirted this issue, but I could not make the connection. May be the authors can clarify.*

Perhaps we need to rewrite this? As we noted, the response of global P is equal to the response of global E. In the second half of the paper (section 5) we decomposed the response of global E in terms of projected changes in the surface energy balance over the globe and separately for both land and ocean.

*(6) In conclusion, it may be good if the paper can be organized so that clear take-home messages that hydrologists can use. These are already there probably, and only need to be brought out more clearly.*

See response to comment 2 above.

*Additional Comment by Prof Sivapalan (received by email after the review)*

*It was suggested we examine a recent paper and contrast their results with our results as follows:*

Kumar, S., Lawrence, D. M., Dirmeyer, P. A., and Sheffield, J.: Less reliable water availability in the 21st century climate projections, Earth's Future, n/a-n/a, 10.1002/2013ef000159, 2014.

This turned out to be a highly relevant paper and we have added a comparative analysis of those results in two new paragraphs in the discussion.