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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct and disequilibrium effects on precipitation in transient climates" *by* D. McInerney and E. Moyer

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Response to comments from Anonymous Referee #2

We thank Referee #2 for his/her review, and respond to their comments in detail below. Original text from Referee #2 is formatted in italics, and our responses are highlighted in blue.

Summary:

In this paper, the authors use climate model simulations (both existing CMIP3

simulations and new CCSM3 simulations which they carried out) to address the issue of why the transient global precipitation (P) response to temperature (T) changes is less than the equilibrium response. The simulations analyzed were driven by a variety of different CO2 and solar forcings. It is concluded that ocean heat uptake is the dominant driver of transient variability in the P-T relationship, rather than the direct effects of radiative forcing agents as has been suggested in several previous studies. This is to some extent a novel study that has some strong points. In particular, I applaud the authors for the idea of using regional information to try to better understand global mean changes. In the end, though, I do not believe that their conclusion of a dominant role for ocean heat uptake follows from their analysis. On the contrary, their results to me seem to be more supportive of the idea that the direct effects of radiative forcing are the primary factor producing transient variability in the P-T relationship. As such, I do not believe that the paper in its present form should be published in ACP.

In my three major comments below, I will discuss in greater detail some of the issues that I had with the authors' analysis and their conclusions that followed from it. First, though, I would like to say that I believe that the manuscript could be improved if more discussion was devoted to explaining why physically we should expect ocean heat uptake to produce transient variability in the P-T relationship.

I do not agree with the authors' explanation (p. 19652, lines 8-16) that ocean heat uptake "could be interpreted as relating to transient cooling at the ocean surface, which "reduces the energy available for evaporation and hence precipitation". How can ocean heat uptake (i.e., a positive energy imbalance at the surface) be accompanied by surface cooling? I agree that it would act to suppress the rate of surface warming over the ocean relative to land areas, since *the heat added at the ocean surface can be mixed over a much greater depth but the ocean surface would not actually cool.*

First, a clarification, and a very important one: Ocean heat uptake is a negative energy imbalance at the surface, not a positive imbalance. It is a downward flow of energy leaving the ocean surface. Also, our statement in the manuscript (cited above) has been misunderstood. We said that ocean heat flux, which is a net export of energy from the surface ocean to deeper layers, is what "reduces the energy available for evaporation and hence precipitation". We did not say that temperature changes reduce available energy for evaporation.

The poor word choice of "transient cooling" likely contributed to confusion. We did not mean an absolute cooling but cooling relative to the atmosphere above the surface. That is, the middle and upper atmosphere warm quickly, but warming at the ocean surface is retarded due to the thermal inertia of the oceans. We have altered the language to make this clear, and thank the reviewer for pointing out the issue.

On a broader note: this review has led us to realize that we have failed to adequately communicate one critical point to readers, which has likely caused this reviewer's discomfort with our explanation and with the core arguments of this paper. The reviewer asks reasonably for physical intuition about "why physically we should expect ocean heat uptake to produce transient variability in the P-T relationship." We thought we had provided that physical intuition: that the downward transport of heat is a net loss of energy that must be balanced by either an increase in radiation energy or sensible heat in to the surface or a decrease in latent heat losses, and that in practice balance is achieved largely by reduction of latent heat loss. However, we realize now that we did not do a good enough job of setting this physical framework to make our arguments clear. If the reader is left with the impression that ocean heat transport represents an

addition of energy to the ocean surface, rather than an export of energy, then all our arguments would seem incoherent.

Factors in the original manuscript that can contribute to misunderstandings of the energy budget include

- We do not give the energy-budget argument in detail until Section 6 of the paper, and the physical framework is only thinly described in the introduction
- The use of "transient cooling" in the introduction
- A typo in the caption of the important Figure 9 in the section on surface energy budgets, that mistakenly described energy fluxes as being into the surface, while we actually show fluxes out of the surface. Here "out of the surface" means upward for all fluxes, except the residual (ocean heat transport) which is down into the ocean.

We have corrected these in the revised manuscript, and take much greater care now in describing a physical framework at the very beginning of the paper, laying out the surface energy perspective. The intent is to provide the reader with a solid and intuitive physical picture from the start, as the reviewer points out is needed.

It is this effect of ocean heat uptake on the regional pattern of surface T change, in fact, that is invoked by Allen and Ingram (2002) to explain transient variability in the P-T relationship. Specifically, in a transient climate, the regional pattern of surface T change associated with a given change in global mean surface T is different than the regional pattern of surface T change at equilibrium. This produces different sensitivities in the two cases of the atmospheric radiative cooling (and thus P) to changes in global mean T.

We can consider the equation for global precipitation changes ΔP that is given by Allen and Ingram (2002) (reproduced in the present study in equation (1)). The ocean heat uptake term in this equation (cN), as far as I can tell, is essentially a correction term for disequilibrium. It is needed because the coefficient a in Allen and Ingram (2002) (which represents the sensitivity of the atmospheric radiative cooling to global T changes) is assumed to take on its equilibrium value. However, if the transient value(s) of the coefficient a was to be used instead, would we still need the ocean heat uptake term in equation (1) to correctly predict ΔP?

We agree that Allen and Ingram invoke a different rationale for the role of ocean heat uptake than we do. Allen and Ingram (2002) suggested that "systematic differences between transient and equilibrium patterns of warming" may play a role in transient precipitation. Allen and Ingram (2002) included the ocean heat transport term as first-order term to account for these temperature discrepancies. In our manuscript we argue that ocean heat transport plays a still larger role in transient precipitation change: it removes available energy from the surface, which must be accommodated by some adjustment of the surface radiative budget, and in practice is accommodated largely by reducing the loss as latent heat flux hence reducing precipitation. This argument follows Wu (2010). Allen and Ingram have altered their thinking on this matter since 2002 (personal communication), and we did no think it important to emphasize their original argument.

We do not understand what is meant by "the transient value of a since a is considered constant in Allen and Ingram 2002, so cannot address that point directly, but add two points that may be relevant to the reviewer's concerns:

First, note that although differences in regional patterns of warming could indeed alter the rate of precipitation increase with temperature as the Earth

warms, no such effect appears in the model output we consider here. And differences in regional patterns of warming could not produce the initial net suppression of precipitation at DT=0 that we have set out to explain in this manuscript.

Second, note that we do not "need" the heat uptake term to correctly predict ΔP any more or less than we need the radiative term. We show in this paper that the equation $\Delta P = a\Delta T + b\Delta R + cN$ is degenerate if heat uptake is linear with ΔT, so that global mean precipitation can be explained equally well using either only a radiative term or only an ocean heat uptake term. This is one of our main points, that since the fast/slow or disequilibrium representations are mathematically equivalent, it is not possible to determine their relative importance from any study of global precipitation alone. The bulk of the paper therefore consists of other tests that can indicate whether a direct radiative effect or ocean heat uptake is the primary physical explanation for the observed initial suppression of precipitation in warming climates.

In any event, I hope that the authors will consider these comments, and those given below.

We thank the reviewer for the comments, which have usefully guided us in clarifying the manuscript.

Major comments:

1) *In Section 3, the authors examine the P-T relationship separately for the global ocean and land domains using output from different climate model experiments. They conclude that since this relationship exhibits greater transient variability over the ocean, the ocean heat uptake must be the primary driver of the variability, as opposed to the direct effects of radiative forcing agents.*

We are not quite sure what the reviewer means by "greater transient variability", but assume that the intent is something like "greater change in behaviour after stabilization." What we found is a consistent (amongst most models) increase in precipitation response after stabilization over the ocean, and a consistent decrease over land (although land precipitation was noisier). We followed Andrews et al's analysis, now dividing data between land and oceans, and found that the characteristic change in response that Andrews et al ascribe to transient precipitation suppression seemed to occur only over the oceans (though land precipitation is inherently noisy, making definitive detection difficult).

It seems that the reviewer is not disagreeing with our interpretation of the CMIP3 data but with our inference that if transient precipitation suppression occurs only over the ocean, it is likely due to ocean heat uptake. We agree that the land/ocean contrast test is not definitive proof of the role of heat uptake. But, it seems very probable that direct radiative effects would be relatively globally homogeneous, so we take the land/ocean contrast as support for the specific involvement of the ocean itself.

I don't agree with this assertion, and, in fact, the authors' results suggest to me that the direct effects of radiative forcing are the dominant driver of the transient variability in the P-T relationship (or at the very least, the results are consistent with this explanation). This is because the P-T relationship changes abruptly (particularly over the ocean) once atmospheric CO2 concentrations are stabilized (Figures 2 and 3), which is consistent with a fast response of the atmospheric radiative cooling (and thus P) to the stabilization of the forcing.

We agree that the results are consistent with a fast response (direct effects), but they are also consistent with a disequilibrium response. As discussed above, the abrupt change can be fit equally well by an equation that posits either direct radiative effects on precipitation or by one that posits heat uptake as the control.

The ocean heat uptake, however, will continue for decades to centuries (or longer) after the forcing is stabilized, due to the inertia of the ocean. If the ocean heat uptake is in fact driving transient variability in the P-T relationship, as the authors claim, then there should be some indication of this variability in the period after the forcing is stabilized (when ocean heat uptake is continuing). I see no evidence for this in Figures 2-4, though, with P versus T appearing to have a constant slope following forcing stabilization.

If we represent transient precipitation as driven by heat uptake via Equation 3

$$\Delta P = c_1 \Delta T - c_2 (\Delta T_{eq} \Delta T)$$

then the slope of precipitation evolution after stabilization will indeed be constant, since

$$\Delta P / \Delta T = c_1 + c_2.$$

There is no change in slope as ΔT evolves if heat uptake is linear with ΔT, as occurs in our model output. (See Figure 9). Therefore we would not expect to see a change in slope during the post-stabilization period in this model output, regardless of the cause of transient precipitation effects.

More generally, we could not expect different behaviour from the fast the fast/slow formulation and the ocean heat uptake representation of transient precipitation because they are mathematically equivalent. We make this point in the manuscript and this reviewer actually does so as well in Comment #2 (that follows), stating that equations (3) and (4) are equivalent and that each effect can be rolled into the coefficients of another.

It seems that this subject is inherently counterintuitive, so we take away the message that we should make greater efforts to provide clarity in the writing.

Or, since ocean heat uptake is also assumed proportional to ($\Delta T_{eq} - \Delta T$), $\Delta P = c_1 \Delta T + cN$

See above discussion.

***2)** In Section 4, the authors discuss equations for the global precipitation change ΔP that were derived in Section 1 (equations 2-4). Equation (2) states that ΔP arises due to T changes ($a\Delta T$), the direct effects of radiative forcing ($b\Delta T_{eq}$), and ocean heat uptake (c($\Delta T_{eq} - \Delta T$)). (The equation is based on an analogous expression presented by Allen and Ingram (2002).) The authors then go on to rewrite equation (2) in two alternative forms. In one form (equation 3), they claim that "transient precipitation is driven only by climate disequilibrium" (assumed to be proportional to ocean heat uptake). In the other form (equation 4), they claim that "the only driver [of transient P] is a direct effect of the forcing agent". In Section 4, the authors evaluate the correlations between the coefficients in equations 3-4 and based on this, they conclude that equation (3) is more physically meaningful since its coefficients are not significantly correlated with each other. I don't quite understand this. Equations (3) and (4) are exactly the same equation, just written two different ways. How then can one be any more physically meaningful than the other?*

While equations (3) and (4) are mathematically equivalent, the terms in these equations have different interpretations. In equation (3) the second term represents the importance of disequilibrium, while in equation (4) the second term represents the importance of radiative forcing (which is linearly related to ΔT_{eq}). Note also that the different representations produce a different direct dependence of precipitation on temperature.

When we say one equation is more physically meaningful than the other, we mean the interpretation of the terms in these equations is more relevant to actual physical causes of transient precipitation behaviour. We have clarified

this in the manuscript.

The authors refer to equations 3-4 as "end-member cases", i.e., suggesting that equation (3) includes only ocean heat uptake effects on P (and excludes direct effects of radiative forcing), while equation (4) includes only the direct effects of radiative forcing. This is not correct, however. Both equations (3) and (4) include ocean heat uptake AND direct forcing effects; in equation (3), the direct forcing effects are simply rolled into the coefficients, while in equation (4) the ocean heat uptake effects are rolled into the coefficients.

We believe this is not a point of disagreement at all. Equations (3) and (4) are representations that posit either only ocean heat uptake effects (3) or only direct radiative forcing effects (4). Since the equations are mathematically exactly equivalent – this is one of the major points of the paper – they can of course represent equally well climates dominated by either factor, with true effects misleadingly "rolled" into the coefficients of the "wrong" representation. If the Earth's transient precipitation response is driven largely by ocean heat uptake, but transient precipitation is described by a "fast/slow" formulation, the ocean heat uptake effects would be rolled into the fast/slow coefficients exactly as the reviewer describes. That is exactly our point, and we discuss how coefficients trade off against each other on pg 19653 lines 14 and 17.

(The physical meaning of these coefficients, by the way, seems rather unclear.) Perhaps I am just missing something here, in which case I welcome clarification by the authors.

We had included a discussion of coefficients in the supplementary documents, but we agree with the reviewer's suggestion that we should also include a more intuitive description in the main text, and have now done that.

c_1 is the equilibrium hydrological sensitivity to temperature: $c_1 = \Delta P_{eq}/\Delta T_{eq}$. c_2

is the hydrological sensitivity to climate disequilibrium, and can be calculated as $c_2 = -\Delta P(T=0)/\Delta T_{eq}$.

3) *In Section 6, the authors discuss the evolution of the surface energy budget in CCSM3 simulations driven by instantaneous CO2 and solar forcings. They calculate the ocean heat uptake as the residual in the energy budget (i.e., the sum of the surface radiative and non-radiative fluxes). I have one major issue with the discussion here. Specifically, the authors seem to imply that the initial reduction of surface latent heat flux and P suppression that occur in the CCSM3 runs are a response to ocean heat uptake (e.g., p. 19667, lines 6-10). This does not physically make sense, however, as ocean heat uptake (and the associated ocean warming) would act to increase the latent heat flux (and thus P) in order to remove the positive energy imbalance at the surface.*

Again, we fear that confusion has been introduced by our not clarifying the definition of ocean heat uptake earlier in the paper. Ocean heat uptake represents a negative energy balance at the surface. Both heat uptake and latent heat transport represent export of energy from the surface layer. If the rest of the surface energy budget is fixed, an increase in ocean heat transport must be compensated for by a reduction, not an increase, in latent heat transport. This reviewer's comments have highlighted for us the importance of making this physical picture clear to readers.

Note that ocean heat uptake is not associated with warming (ΔT) but rather is anticorrelated with it: heat uptake it is proportional to ($\Delta T_{eq} - \Delta T$). The largest ocean heat uptake occurs immediately after the forcing change, when surface warming is minimal.

The surface energy budget response to abruptly increased CO2 forcing in our experiments is shown in our figure 9. Ocean heat uptake that matches the top-of-atmosphere radiative imbalance develops quickly, within the resolution

of the annual data shown. The inferred value of this heat uptake over the ocean at t=0 is over 6 W/m^2. Increased IR opacity (and possibly atmospheric warming)and cloud feedbacks do produce a small increase in downward longwave and shortwave radiation ($\approx$ 2 W/m^2 total), but this is too little to accommodate the full ocean heat uptake. The remaining budget is balanced by reduced export of energy out of the surface via latent heat.

Clearly, the initial reduction in latent heat flux in the CCSM3 runs must be the cause of the ocean heat uptake rather than a response to it. (Of course, changes in other surface energy fluxes also contribute to the ocean heat uptake, such as, e.g., the increased shortwave radiation input in the solar-forced runs.)

The comment “reduction in latent heat flux must be the cause of the ocean heat uptake" seems to invert the previous concern that “ocean heat uptake would act to increase the latent heat flux". Again however this points to the need for us to provide a clearer physical explanation in the introduction.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19649, 2012.