Atmos. Chem. Phys. Discuss., 12, C10403–C10408, 2012
www.atmos-chem-phys-discuss.net/12/C10403/2012/

Atmospheric Chemistry and Physics Discussions

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

D. McInerney and E. Moyer

dmcinern@gmail.com

Received and published: 10 December 2012

Response to comments from Anonymous Referee #1

We thank Referee #1 for his/her positive and thoughtful comments, which will improve the quality of the final manuscript.

We respond to the specific comments from the referee in detail below. We have formatted original referee comments in italics and provide our response in blue

text.

The authors present an analysis of the transient response of precipitation to greenhouse gas forcing, framed in terms of two "pathways", one related to the disequilibrium between oceanic and atmospheric heating and the other related to fast and slow responses of the climate to such forcing. Overall, I find this analysis to be well-motivated and interesting and believe it should be of appeal to the readers of Atmospheric Chemistry and Physics.

Major Comments: *Since the aim here is to investigate precipitation, the authors may want to consider a diagnostic (vertically-integrated) moisture budget analysis for precipitation perturbations, namely:*

$$P' \approx E' - < \dot{\Delta(} q'\bar{v}) + \dot{\Delta(}\bar{q}v' > - < \partial_t q' > \quad (1)$$

where overbars indicate mean quantities, primes departures from means, and $<>$ mass weighted vertical integration (see, e.g., Seager et al. 2010). Globally averaged, (1) requires precipitation and evaporation perturbations to balance, although on very short timescales, the last term on the RHS, the atmospheric moisture storage term, could be important. On smaller scales even perhaps the global-scale land/ocean partitioning discussion in Section 3 the circulation-related terms are likely to be important. A moisture budget analysis could certainly complement the discussion in Section 6 on the surface energy budget.

We agree that moisture budget analysis is useful and relevant, because changes in local precipitation may not correspond to changes in local evaporation. Especially as we are arguing that precipitation suppression is a response to radiative constraints, it is useful to keep the distinction between precipitation and evaporation in mind.

For the purposes of this paper, we believe the importance of moisture circulation can be examined by computing (P-E) rather than vertically integrating moisture fluxes directly. Since one of our arguments in favour of ocean heat uptake as the driver of precipitation suppression is the apparent lack of precipitation suppression over land, it is important that we demonstrate that land/ocean contrast is not simply due to moisture fluxes. We have now done so, with new discussion in Section 3 and a new figure (Figure 4 of the revised manuscript).

Moisture flows within the atmosphere over the ocean are themselves interesting, and we are treating them in an upcoming manuscript that will hopefully complement the Section 6 discussion here as the reviewer had suggested. Because this manuscript was already very long, we felt that discussion was better left for a separate complementary paper.

At the same time, I would also suggest broadening the discussion in 6 to include energetic considerations from an atmospheric column perspective. Indeed, both the surface and column energy budgets affect (1), through interactions with evaporation but also through circulation.

We agree that it would be useful to discuss changes in atmospheric structure and temperature profile as well. We do think this is outside the scope of this paper, but it is the subject of ongoing work that follows on this research. Globally, the net precipitation suppression that we and others see must be the result of a global reduction in evaporation (and as we show in Figure 9, that reduction is confined to the oceans). But horizontal transfers of energy and moisture will contribute to the local conditions that produce this suppression of evaporation.

In this paper we have attempted to definitively show that transient precipitation suppression is related to ocean heat uptake, and that precipitation suppression

is the means by which the global energy budget is balanced in warming climates. We have not yet demonstrated HOW global radiation constraints are physically translated to a reduction in convection and precipitation. We hope however that the identification of ocean heat uptake as key can help guide future work on mechanisms.

The authors may want to consider adding some further discussion of the physical mechanisms for precipitation change under global warming, or at least cite some additional references on the development of such mechanistic understanding, e.g., the work of Chou and Neelin (2004).

We have now added some further discussion of physical mechanisms by which both equilibrium and transient climate change can affect precipitation. We have also included a wider range of citations.

On a related note, some studies of the El Ni√±o/Southern Oscillation (ENSO) impact on climate have used applied the disequilibrium concept (see, e.g., Chiang and Sobel 2002 and additional references below).

We did not know that work, and are very pleased to learn about it. We now mention and cite it.

Minor Comments:

Pg19652, Lines 14-16: The authors note "... the direct effect would relate to changes in surface energy fluxes other than ocean heat uptake ..." Should "ocean heat uptake" be "latent heat flux"?

Not quite, but we think that the reviewer's misunderstanding results from the fact that lines 14-16 are poorly worded. We are grateful for the reviewer for pointing that out, and have reworded them in the revised manuscript.

The "effects" being discussed here are those that affect precipitation, i.e. latent heat flux itself. The “effects" must then be confined to the terms in the surface energy budget other than latent heat flux. In Allen and Ingram’s equation (1), heat uptake is called out in a separate term cN (that we call the “disequilibrium effect"). Our point was that the Allen and Ingram’s “direct effect" ($b\Delta R$ in equation 1) must then necessarily reflect changes in the remaining energy budget terms: i.e. longwave and shortwave radiation and sensible heat flux.

Pg19656, Line 7: subject/verb agreement: “bulk ... involve" should be “bulk ... involves"

Corrected.

Pg19657, Lines 13-15: The authors state “These experiments were however hampered by low signal-to-noise: four used slab-ocean models and the single coupled model run involved only a single realization." I wouldn’t characterize that the use of slab ocean models as hampering by low signal-to-noise, but perhaps I’m missing something?

We must not have explained well enough. The problem is that SOMs equilibrate rapidly, so any transient effect is compressed into a few years, making signal to noise lower than in fully coupled models. We have adjusted the wording to try to make this clearer.

Pg19657, Lines 19-20: Given the availability of CMIP5 data, it may be worthwhile to “justify" use of CMIP3. Apart from the continuity with the cited study of Andrews and Forster (2010), is it because of the simplicity of the “1pctto2x" experimental design?

We used CMIP3 because at the inception of this study, CMIP5 data was not available. We used 1pctto2x to be analogous to Andrews and Forster (2010).

Pg19658, Line 24: subject/verb agreement: “data ... is" should be “data ... are"

Corrected.

Pg19664, Lines 17-18: The statement “The latent heat flux is constrained by the fact that surface energy fluxes must balance" is not strictly correct, since ocean heat flux divergence will create regions of positive and negative net surface flux.

Yes, it is not strictly true locally. Our statement was meant to apply to ocean-wide average fluxes, though we did not specify that explicitly and so the word choice was poor. We show in Figure 9 only global, all-ocean, and all-land fluxes, and over the entire ocean, surface fluxes must balance (barring very small transfers via runoff from land to ocean). We have adjusted language to make it clear that the balance applies only at the scale of the ocean.

Pg19665, Lines 16-17: subject/verb agreement: “abrupt forcing increase and imposition drives" should be “abrupt forcing increase and imposition drive"

Corrected.

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