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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 Dr Allan

We thank Dr Allan for the time he took in writing such a thorough and helpful review. He has suggested useful additional discussions that we have incorporated into our revised manuscript, and has contributed to improving the paper.

We respond to the specific comments from Dr Allan in detail below. We have

formatted original comments in italics, and provide our response in blue text.

The authors make a valuable contribution to the discussion on physical mechanisms of transient precipitation (P) changes at the largest scales, particularly in considering the land and ocean responses separately and linking this to disequilibrium of the climate system. This extends recent advances (e.g. Allen and Ingram, 2002; Andrews et al. 2010), and I consider that the work should be published. I found the article challenging (excuse any of my misunderstandings) and I have a number of comments, which were invited by the authors. In particular, some further clarification of the physical mechanisms outlined (and physical meaning of disequilibrium coefficients c1 and c2) with some consideration of moisture and energy fluxes between land and ocean would be welcome.

I supply a short list of additional references that would be useful to consider, in particular the study by Cao et al. (2012) which is highly relevant.

We agree that Cao et al 2012 is relevant. This paper appeared after our manuscript appeared in ACPD, so we could not have mentioned it previously, but we will cite it now and discuss its conclusions.

1) Energetic constraints

My interpretation of Allen and Ingram (2002) is that eq (4) (p.19654, line 13) remains applicable (energy budget constraint) but that the "alpha" term is dependent upon the spatial distribution of surface temperature change (dT) which determines precisely how atmospheric radiative cooling responds to global warming. This is sensitive to the land/ocean warming contrast which is partly influenced by how far from equilibrium the climate system is (although feedbacks independent of disequilibrium also contribute, e.g. Lambert et al. 2011).

We do not think the land/ocean temperature contrast is the cause of suppression in global precipitation. In our view, global precipitation suppression is the means by which the surface radiation budget is balanced to account for ocean heat uptake. In that framework global precipitation suppression would occur regardless of the presence of land. This hypothesis could readily be tested by running an aqua-planet model. To our knowledge no one has yet performed this test, but we will add it to our list of future work, and would encourage others to do these experiments as well.

It would appear to me that both the Andrews and Forster (2010) and Wu et al. (2010) work (mentioned p. 19652, line 20) is compatible with eq (4) since in the CO2 rampup phase the radiative forcing increase is suppressing P (smaller global $\Delta P/\Delta T$) while in the ramp-down phase radiative forcing is diminishing yet T continues to rise due to time-scales associated with ocean heat uptake and radiative feedbacks (larger $\Delta P/\Delta T$). This is also discussed in O'Gorman et al. (2012).

We agree that both Andrews and Forster (2010) and Wu et al. (2010) are consistent with equation (4). We have added a citation to O'Gorman et al. (2012).

2) Physical Mechanisms

I found the some of the descriptions of physical mechanisms rather confusing (e.g. p.19654, line 25-30). The effects of CO2/Solar forcing on rapid adjustments over land and ocean are well described by Cao et al. (2012) and it would be useful to refer to this analysis and consider horizontal fluxes of energy and moisture between land and ocean.

Given that land T rises more than ocean T in response to positive radiative forcings (even at equilibrium) this has implications for land $\Delta P/\Delta T$. The land minus ocean T influences circulation strength, certainly for monsoon systems

(e.g. Levermann et al. 2009), while the ocean T sets the moisture burden destined for the land (through the Clausius Clapeyron equation).

Note that the phenomenon we set out to explore is a net global suppression of precipitation in warming climates. Changing horizontal fluxes of moisture can redistribute precipitation but cannot produce a net global reduction in precipitation. Moisture transports are important, however, in that they could deepen land-ocean contrast by further reducing precipitation over the ocean and enhancing precipitation over land. We agree that for this reason it is important to check the contribution of moisture transport to land/ocean contrast when arguing that observed land/ocean contrast supports heat uptake as the driver of global precipitation behaviour. We did not include that analysis in the submitted manuscript, but have added it now.

Cao et al 2012 appears to show that in runs of the UKMO HadCM3L model, less than 1/4 of observed initial precipitation suppression over the ocean is accounted for by transfer of moisture from oceans to land. (The remaining 3/4 is then due to reduction in evaporation from the oceans). That is, moisture transport contributes to but does not dominate land/ocean contrast in precipitation. (The comparison we are using is Cao et al.'s figure 1b, which seems to indicate that quadrupling of CO2 produces a global precipitation suppression of .08 m/yr by day 10, and his Figure 5f, which seems to show increased moisture fluxes from ocean to land of about .02 m/yr. Although no figure in Cao et al 2012 shows precipitation suppression over the ocean only, ocean suppression would be slightly deeper than the global value.)

Our results with the CCSM3 model are quite similar to those of Cao et al 2012: moisture transport from ocean to land accounts for less than 1/4 of initial precipitation suppression over the ocean. That is, the land/ocean contrast is slightly deeper than would be produced simply from observed changes in the ocean evaporation. We have now shown this in Figure 4 of the revised manuscript. Figure 4 also confirms that global precipitation suppression is due only to ocean behaviour: reduction in evaporative flux occurs over the ocean but not over land. (This is also shown in Figure 9 of the original manuscript).

Outlining physical mechanisms for transient changes in this context may be useful in relation to p.19658-p.19659, including discussion of moisture and energy transport changes. The physical basis for c_1 and c_2 (eq(3) and Fig. 5) may need further clarification.

We agree that explicitly determining the atmospheric adjustments that lead to precipitation suppression is important. We did judge this to be outside the scope of this paper (note that the fast/slow mechanism is also purely descriptive), but we are pursuing it in an upcoming manuscript.

We see this paper's primary role as firmly demonstrating the connection between transient precipitation suppression and ocean heat uptake and so guiding later explorations of mechanisms, so that this important issue can be understood.

We agree with Dr. Allan's suggestion, however, that that reader may feel the story is incomplete without more discussion of the mechanisms by which radiative constraints are translated into changes in convection and therefore precipitation. We have therefore added further discussion to address this need.

As far as I can tell, c_1 seems to be the total $\Delta P_{eq}/\Delta T_{eq}$ which, unlike the fast/slow framework, is dependent upon forcing agent which is not a beneficial property.

Yes, c_1 is the total $\Delta P_{eq}/\Delta T_{eq}$. We have also now moved the definitions of c_1 and c_2 up from the Supplementary Documents into the main manuscript.

We agree that the fact that c_1 is dependent on the forcing agent suggests that transient precipitation effects in solar and CO2-forced climates are not both pure disequilibrium effects, but instead result from a combination of disequilibrium and direct effects. We had discussed this at length in the manuscript, but Dr Allan's discomfort suggests that we should make the writing still more clear.

In the submitted manuscript, we discuss the possibility of both direct and disequilibrium effects on p19653 lines 26-27: The true atmospheric response can of course lie anywhere on the spectrum of relative importance of these different mechanisms. The title of the paper ("Direct and disequilibrium effects on precipitation in transient climates") was chosen to emphasize that both types of effects occur. The offset in ΔP_{eq} between solar- and CO2-forced cases is prima facie evidence that some direct effect exists in one or both of those cases, and we state this on p19654 lines 14-16: "The offset in global mean precipitation between the solar- and CO2-forced cases is difficult to explain in a purely disequilibrium framework and seems to require some direct effect."

We do believe, however, that the difference between solar- and CO2-forced cases results not from negative direct effects in the CO2-forced case but from an increased positive direct effect in the solar-forced case. In the solar-forced case, the additional surface radiative input from a brighter sun constitutes a large positive direct effect and drives additional evaporation. Direct effects in the CO2-forced case are also positive (see Figure 9 of original ms), thereby lessening precipitation suppression and bringing the CO2-forced case closer to the solar forced case than would occur in their absence.

Because this point is the central conclusion of our paper, we have taken the reviewer's discomfort very seriously and have attempted to clarify and amplify this point in the revised manuscript.

If $c_2 = -\beta_{CO2}/\Delta T_{eq}$, the difference in patterns shown in Fig. 7 are solely be-

tween c_1 and α (the total $\Delta P_{eq}/\Delta T_eq$) which is dependent on forcing type verses the slow temperature-dependent $\Delta P/\Delta T$ response which is independent of forcing type but has a sensitivity to the pattern of ΔT which depends partly upon disequilibrium).

This is almost true. We calculate c_2 using local disequilibrium ($\Delta T_{eq,local} - \Delta T_{local}$). If we had calculated beta using $\Delta T_{eq,local}$, they would be exactly equivalent. But instead, following the conventions of the field, we calculate β_{CO2} using global CO2 values (analogous to global ΔT_{eq}). So there is a slight additional difference introduced between coefficients because of the spatial pattern of ΔT_{eq}. That difference does not affect our conclusions, however. We show in supplementary document Figure 8 that the exact formulation of beta does not change our conclusion: both α vs. β and α vs. β_{CO2} are highly correlated.

Note that we do not agree that α represents the slow temperature-dependent response. In fact we think the correlation between α and β_{CO2} occurs precisely because the coefficient α includes a part of what should properly be considered the disequilibrium response. This is our central point of this section – that the correlation demonstrates that α is NOT the physically meaningful slow response. We have adjusted the language in the manuscript in an attempt to make this important point more clear.

A surface energy perspective is presented by the authors in Section 6. Some clarification with regard to the physical mechanisms would be beneficial. For example, on line 1 it is stated that "In the CO2 forcing case, more than half of the initial heat uptake is accommodated by a reduction in latent heat and therefore precipitation." It is difficult to see how CO2 increases can directly increase evaporation since the immediate effect will be heating of the atmosphere.

This is possibly a typo on the reviewer's part. In the CO2-forced case, evaporation is immediately reduced, not increased. If global precipitation is suppressed,

evaporation MUST be reduced, else the hydrological system would not be balanced. The global reduction in evaporation over the ocean is shown in figure 9.

As mentioned earlier, we have now added a larger discussion of the physical mechanisms by which radiative imbalances translate into precipitation suppression, and thank Dr. Allan for pointing out that this would be useful.

It is only be reduced P that water vapour may not be removed from the surface layers, thus inhibiting evaporation.

In a balanced system, P and E must be equal, so reduced precipitation would naturally go with reduced evaporation. We did not fully explain in this manuscript why heat uptake would lead to reduced precipitation, but in our view, the imposition of a top-of-atmosphere radiative imbalance will begin to warm the atmosphere, and it must so happen that the initial slight warming sufficiently increases atmospheric stability and reduces convection and therefore both precipitation and evaporation, which then contribute to balancing the surface energy budget.

Similarly, more evaporation does not simply lead to more P since the low level water vapour must be uplifted through some mechanism (thereby ventilating the surface) which requires additional radiative cooling of the atmosphere.

Globally, in hydrological balance, any change in E must be reflected in P. We now show in a new figure (Figure 4 of the revised manuscript) that (P-E) anomalies for the ocean counterbalances that for the land. The (P-E) anomalies show that after imposition of a change in radiative forcing by adding CO2, some additional moisture is transported from ocean to land. But globally, the balance between P and E must be retained.

I did not understand the comment on line 16, "the direct effect is opposite sign *than that assumed by Andrews et al."*

Andrews et al assumed that a direct effect in the case of CO2-forced climates produced a net suppression of precipitation. We show that the direct radiative forcing effects present in CO2-forced climates are slight increases in both LW and SW surface downward radiation fluxes, which would enable an increase of precipitation. That is, Andrew's proposed direct would reduce precipitation, but the direct effect that we observe in CCSM3 would increase it.

Again, this point is important, and so we take this comment as very usefully suggesting that we should make the writing more clear.

The global response appears as predicted by the fast/slow framework; the land response requires rapid adjustments in moisture and energy fluxes between land and ocean (e.g. Cao et al. 2012).

We do not understand this statement. The overarching point of the paper is that global responses could be explained by either a fast/slow or a disequilibrium framework, since the two are mathematically equivalent. There is no way to understand which framework is a better representation other than by careful examination of regional responses.

One of those lines of evidence is the land/ocean contrast in precipitation response. Dr. Allan is absolutely correct that if altered moisture fluxes between ocean and land completely explained the observed land/ocean contrast, our argument would be gravely weakened. As mentioned earlier, we now show explicitly that these alterations in moisture fluxes account for only a small part of the land/ocean contrast in precipitation (Figure 4 of revised ms).

The authors also argue that "increased solar forcing cannot produce a transient suppression of precipitation in the global average" (line 1 or p.19667). Radiative heating through absorption by water vapour and aerosols will heat the atmo-

sphere (e.g. Andrews et al. 2010), suppressing precipitation initially over the ocean, before the T begins to rise (Cao et al. 2012). This is of course not the case over land where direct warming of the surface can occur. Increases in surface evaporation may only be sustained if that water can be removed from the near surface (through convective processes for example) requiring enhanced radiative cooling or removal of energy through lateral fluxes (e.g. inflow of cool ocean air, see Levermann et al. 2009).

We do not fully follow this argument. We agree that precipitation suppression may occur over the ocean (and does in the CO2-forced case), while it does not do so over land. But it is not necessary that precipitation suppression occur, and in the solar-forced case it does not, as has been pointed out by many authors.

Our argument is that it is natural that no suppression is observed for solar forcing. Initially ocean heat uptake must balance the externally applied forcing, and in the case of increases in shortwave radiation, that additional forcing is essentially transferred directly to the surface, providing the necessary energy for ocean heat uptake without requiring any involvement of latent heat flux. We do think that our description of this argument was somewhat confusing, and we have taken this comment as impetus to improve it.

Consistent with the present study, however, the results from Cao et al. (2012) indeed suggest that the initial effect of increased solar radiative forcing is a global increase in P.

We agree that we are consistent with Cao et al 2012 (and previous works), but the consistency is in showing that the initial effect of increased solar radiative forcing is approximately zero (Figure 1 of our manuscript; Figure 1a of Cao et al). Note that the lack of initial response is identical in both cases despite model differences and despite the fact that Cao et al 2012 applied change in solar forcing is nearly twice ours. We take that consistency as strong support for our radiative argument that altering solar forcing will not drive an instantaneous net change in precipitation.

3) Regional Patterns

The regional P response (p.19662, line 20) are related to enhanced moisture fluxes from dry to wet regions (due to larger moisture burdens with warming) which act to enhance P-E patterns (e.g. Held and Soden 2006). Considering an energetic perspective has also been shown to be useful (e.g. Muller and O'Gorman 2011; Levermann et al. 2009 PNAS). The anti-correlation between alpha and beta in Fig. 6 is interesting. But are the precipitation change patterns merely aliased onto these parameters? Again, If c_2 is just equal to $-\beta_{CO2}/\Delta T_e q$ the pattern is essentially the same in Fig. 7b, 7d and Fig. 8b.

Yes, this is one of our points, that Fig. 7b, 7d should be similar, and that the differences between the fast/slow and disequilibrium frameworks lie only in (in our view) the fast/slow framework's misspecification of α. Figure 8b is similar, but note that 8b is 7b multiplied by the regional pattern of ΔT_{eq}.

The gradient of α and β is about -1.5 K for CO2 and -1.25 K for Solar which I think shows the ΔT at which the "fast" and "slow" components exactly cancel.

We think Dr. Allan is referring to slopes estimated from Figures 6b and 6c of the manuscript. If so, then yes, exactly, this plot implies that "fast" and "slow" components would cancel at a local DT of ≈1.5 C for ALL model gridpoints, regardless of the rate or warming, the ultimate magnitude of local warming and the amount (or even sign) of initial precipitation suppression. It is this universal anti-correlation that we find unphysical, since the "fast" and "slow" terms are supposed to refer to physically distinct processes.

In our framework, the universal anti-correlation of components is explained by

the fact that a part of the disequilibrium term c_2 has been folded into α: $\alpha = c_1 + c_2$. Both α and β then reflect the magnitude of the local disequilibrium term, and since the disequilibrium term c_2 is on average larger than the equilibrium term c_1, the result is an apparent tight anti-correlation.

4) Additional comments and clarifications

a) The altitude of instantaneous radiative forcing is also thought to be important. Ming et al. (2010) show that absorbing aerosol added to the boundary layer causes adjustments through sensible heat rather than latent heat which is governed by forcings above the lifting condensation level (e.g. O'Gorman et al. 2012).

We have not studied this in detail, and it is somewhat outside the scope of the paper, but it is a useful point for any study that explicitly focuses on detailed mechanisms of precipitation suppression, and we will take it into account in our ongoing work.

b) Although it is true that people tend to live on land (e.g. p.19657), a large proportion of the global population lives within 400km of the oceans so ocean changes and ocean-land transports are also important.

We did try to determine whether coastal points reflect more of the ocean signal, but the resolution in our data was not sufficient to be able to draw meaningful conclusions. We have now included in the text now a line saying that this issue should be studied in the future, using a higher spatial-resolution model.

Nevertheless I agree that understanding mechanisms for land and ocean responses is vital. Energetic constraints still apply regionally (accounting for lateral fluxes) as described by Muller and O'Gorman (2011).

It was an omission to not cite Muller and O'Gorman (2011). We had originally envisioned this paper as a shorter paper with a tight page limit, and had therefore pruned the citation list severely to try to hold the page count down. We will take advantage of ACP's open page limits and expand the citations back o their proper length.

c) Fig. 7: It would be helpful to increase the colour bar range to -30/+30 for Fig. 7a for consistency with Fig. 7b (to show that c_1 is smaller in magnitude than c_2).

We had deliberately adjusted the colour bars in the various panels of figure 7 so that the viewer could more readily see the similarity in patterns between the coefficients, which was the main point of the figure. The reviewer is correct that this obscures the important point that c_2 is significantly larger than c_1 (cf discussion above of the implications of the gradient of α vs. β_{CO2}). We would like to leave the figure the same for the purposes of pattern comparison, but we now point out the differences in scale in the figure caption and discuss their implications, and we have included a version of this figure with equal colour scales in the Supplementary Documents.

d) Fig. 8a It may be worth noting that ΔP_{eq} is just $c_1 \Delta T_e q$ so the pattern is essentially shown in Fig. 7a.

This is not quite true, since c_1 is the local precipitation response per local temperature change, and ΔT_{eq} has spatial structure. For that reason we show $\Delta P_e q$ separately in Figure 8a. We have added a line in the captions reminding the reader of this distinction between Figures 7a and 8a.

Fig 8b: Is "Initial intercept" simply β_{CO2} shown in Fig. 7d?

It is almost but not quite the same. The intercept is necessarily scaled according to the size of the forcing increase, since β_{CO2} is precipitation increase per CO2 doublings.

We did use a different color scale in 8b relative to 7d, hoping that this would produce more clarity rather than more confusion. The 7d color scale was inverted (red now being positive and blue negative) to better highlight similarities in patterns between α and β.

e) Fig. 9: The fluxes appear to be defined as upward, out of the surface, apart from the residual (which is the heat flux into the ocean) contrary to the caption. For example in the solar case there is an initial negative solar flux anomaly of 4 Wm-2 whereas the forcing is positive 4 Wm-2.

This is a typo in the caption, now fixed – many thanks for pointing that out.

Typos:

p.19656, line 10, typo: "presumably the same"

Corrected

p.19668, line 11 "all positive forcings"?

Yes. Corrected

p.19668, line 18 "the pure fast/slow"

Corrected

Fig. 3 caption, last line "appears to be acting"

Corrected

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