Hydrol. Earth Syst. Sci. Discuss., 8, C3662–C3667, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C3662/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "What do moisture recycling estimates tell? Lessons from an extreme global land-cover change model experiment" *by* H. F. Goessling and C. H. Reick

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Received and published: 25 August 2011

Response to referee #3

Please read the general response prior to this response.

RC: Referee Comment

AR: Authors' Response

RC1: The used terms of "recycled" and "recycling" have different meanings from the classical definitions of "precipitation recycling". It is necessary to address what they

exactly mean in this paper. In the text, these terms are mostly used to describe evaporation of continental origin. However, in section 7 (P3529:L24-25), the authors reached the conclusion "that moisture recycling estimates are of limited use to deduce hydrological impacts of land-cover change activities". If we understand the "moisture recycling estimates" as the estimated continental evaporation, this conclusion contradicts with the authors' data presentations. Apparently, the cutting off of continental evaporation has much impact on the moisture (Figure 2), precipitation (Figure 4), and temperature (Figure 4) responses in the DRY experiment. It is not clear "What do moisture recycling estimates tell".

AR1: In response to the referees comments we have made some changes to the terminology and discuss the terminology in much more detail in the new version (Sec. 1 and 2). However, our continental recycling ratios (formerly "RMF") are basically exactly what is commonly referred to as continental recycling ratios (Numaguti et al. (1999), Bosilovich et al. (2002), Yoshimura et al. (2004), and van der Ent et al. (2010)), see the new Fig. S1 in the supplementary material. We do not really understand what the referee means with "If we understand the moisture recycling estimates as the estimated continental evaporation", because "moisture recycling estimates" (a unitless ratio) are completely different objects than "continental evaporation" (a moisture flux). Regarding the last sentence of RC1, we fully agree that the suppression of continental evaporation strongly impacts climate, but this fact alone does not show that moisture recycling is responsible for the response, and even less that moisture recycling estimates are useful indicators for the response. Nevertheless we agree that it remains an open question what moisture recycling estimates actually tell, in particular for perturbations much weaker than those considered by us.

RC2: The data analysis is rather simplified without considering the moisture transportation from oceans but dynamically "forced" by DRY land. In the extremely DRY experiment, the dry surface impacts (local) atmosphere by reduced evaporation. Meanwhile, the enhanced land-ocean thermal contrast may enhance large-scale circulations. This changes not only the amount but also the tracks of the moisture carried by atmosphere circulations. The authors have observed this phenomenon (section 4.4), but haven't sufficiently integrated it into their data interpretations. I suggest the data analyses in this manuscript are only valid on the incorrect assumption that the DRY land has no influence on large-scale circulations. Considering this DRY land forcing, it is not surprising to find the "mismatches" of VIM and precipitation responses. Principally this mechanism is expected to happen in the monsoon regimes, where the authors show indeed strongest "mismatches" to occur. It is very likely that the reduction of continental moisture due to suppressed evaporation is compensated for by the enhanced moisture transportation from oceans. I suggest the authors to check if this mechanism exists in their simulations; otherwise, exclude this possibility in the next version. Instead, the authors have attributed the mismatches to the decreased precipitation owing to surface heating of atmosphere, reaching the major conclusion of this paper "Decreased precipitation rates compensate for the missing moisture input from evaporation, such that moisture content of an atmospheric column traveling over a continent may evolve similarly both with and without suppressed continental evaporation" (P3523). While the mechanism, "surface heating -> precipitation decrease", widely exists in nature, I am afraid the authors have overstated its consequence. The existence of this phenomenon can suggest nothing without knowing the magnitudes of its influences. Of more importance is "how much". Only if the decreased precipitation, importantly, owing to temperature increase, compensates for nearly 100% of the decreased evaporation, this conclusion is correct. I advise the authors to provide this quantification. Something relevant is in Fig 5; however, we can not simply attribute a water balance problem to temperature increase.

AR2: We concede that the analysis of the changes in large-scale circulation have been insufficient in the original manuscript. In the new manuscript, Sec. 2.3 and 5.2, the response of the atmospheric circulation is now extensively discussed. Indeed the response from the circulation is more important than we originally thought, and the role of positive local coupling less dominant, please see our general response to the

referees comments. Fig. 9 and 10 in the new manuscript provide the quantification of changes in P-E which the referee asks for.

RC3: I am not deep into dynamic meteorology, and probably have missed something in the manuscript. But I really confused by the claim "moisture content of an atmospheric column traveling over a continent may evolve similarly both with and without suppressed continental evaporation" (P3523), no matter how this conclusion has been reached. It suggests to me that the atmospheric moisture is self-organizing and has nothing to do with the bottom surface, including land covers and oceans. So it is only a problem of external solar forcing, and thus latitude dependent. Is that real in nature? In section 4.1, the authors indeed have shown that the VIM is very sensitive to land cover and evaporation (Fig 2). They are contradictions.

AR3: In Fig. 8, right, of the new manuscript we show a highly idealised situation over land in which the decrease in precipitation exactly compensates for the missing evaporation. The ocean is a different story because it makes no sense to suppress all evaporation (in a model) and still call it an "ocean". If this highly idealised behaviour over land were generally valid – which of course is not the case – then one could say that "the atmospheric moisture is self-organizing and has nothing to do with the bottom surface". However, Fig. 8 is not meant to suggest that this is generally the case, but included in the paper to clarify the qualitative difference between an atmosphere in which precipitation responds "locally" through non-budget mechanisms (ideally positive local coupling), and an atmosphere where the atmospheric moisture budget is taken to be the main mechanism through which evaporation influences climate.

RC4: The relative differences of VIM and precipitation are calculated using (DRY-REF)/max(DRY, REF). This strategy makes the outcome values fall into the interval [-1,1]. However, the max(DRY, REF) is changing in these calculations. When REF>DRY, it becomes (DRY-REF)/REF; when DRY>REF, it becomes (DRY-REF)/DRY. The latter case is of no physical sense, although the obtained values are kind of beautified. This way, the quantities with different signs can not be compared with each other; thus they can not be plotted in one figure.

AR4: When presenting differences in this manner we have been repeatedly confronted with the same irritation the referee expresses. Therefore we will elaborate on this objection, although in the new manuscript we now use a more standard representation. In our opinion the opposite of what the referee suggests is true: Quantities with different signs can be compared better with each other in "our" way, i.e. (DRY-REF)/max(DRY,REF), than it is the case in the standard way (where the denominator is taken always from the same experiment, e.g. (DRY-REF)/REF). Suppose you change your mind and find that the other of the two experiments should be called your reference experiment and, hence, be put into the denominator (which makes the same physical sense as the other way round), and you would consistently also calculate the difference in the numerator the other way round, i.e. (REF-DRY)/DRY. The result would be that not just the sign but also the absolute value of the measure would change, e.g. -90% would become +900%. In effect you would still want to show the same thing, but absolute values are now deformed, i.e. depending on the reference experiment you choose, the quantitive comparison of absolute values gives a different impression of relative sizes. In contrast this does not happen when "our" way to calculate differences is used. However, we found an elegant compromise that at the same time demonstrates that there actually is no real controversy. In the new manuscript the colourbars of the difference plots are exactly as they were before, but the tick marks are changed from the "(DRY-REF)/max(DRY,REF)" version to the "(DRY-REF)/REF" version. This affects only the positive values and transforms the previous vector of values {-90%,-80%,. . ., -10%, 10%,. . ., 80%, 90%} into the vector {-90%, -80%,. . .,-10%, 11%, 25%, 43%, 67%, 100%, 150%, 230%, 400%, 900%} (the function to convert the positive values reads -x/(x-1)). This way the comparison remains symmetric in terms of colour but not any more in terms of values (because of the choice of tick marks that now may appear slightly peculiar) and at the same time the potential for irritation is reduced because the new values conform to the standard, i.e. (DRY-REF)/REF.

RC5: There is no information how the relative difference of temperature is calculated; DRY-REF, I guess. I advise to uniform these calculations.

AR5: The sign of the difference for the temperature plots is indicated above the plot (DRY-REF). This sign convention is used throughout the paper. For temperature differences we show absolute differences because relative temperature differences do not make much sense.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3507, 2011.