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HESSD

8, C3629–C3635, 2011

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Comment

***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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Response to the editor (Bart van den Hurk)

Please read the general response prior to this response.

EC: Editor Comment

AR: Author's Response

EC1: The three reviews of this paper use a different style to convey essentially a very similar set of messages: the study by Goessling and Reick touches an interesting

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topic, but the presentation of their results suffers from a lack of clarity of the terminology, the omission of the perspective of atmospheric dynamics and related precipitation responses in the interpretation of results, and a tendency to come to somewhat overstated conclusions given the strong perturbation imposed to the model experiments. However, I surely do encourage the authors to continue with the publication of their manuscript, since it provides a valuable contribution to the scientific discussion around the effects of large scale land use change, the definition of proper diagnostics to monitor these effects, and the understanding of the complex interactions that operate in this area.

AR1: Please see the general response.

EC2: Concerning the terminology, all reviewers point at the difference between recycling ratio and the (terrestrial/oceanic) origin of the evaporation. I surely recommend the authors to appreciate these concerns and relabel their diagnostics accordingly.

AR2: In the new manuscript we have accounted for the reviewers recommendations. To prevent similar confusions provoked by our original manuscript, we made changes (I) with respect to the recycling quantities shown in Fig. 2, and (II) concerning clarification of the recycling concept used in our study. Ad (I): We have now replaced the uniformly weighted time-means we use in the original manuscript (as Numaguti (1999)) by precipitation-weighted time-means as used by Bosilovich et al. (2002), Yoshimura et al. (2004), and van der Ent et al. (2010). We provide a figure (Fig. S1) with explanatory notes in the supplementary material to elaborate on the (for monthly means very small) differences that arise from these different techniques of temporal aggregation. Ad (II): Part of the confusions concerning the recycling concept used in our study is related to the way results were presented in other related studies. In the case of continental recycling ratios that we consider in our study "recycled moisture" is indeed identical with "moisture of continental origin". This is true also for the four studies cited above. We think the confusion arose because this fact is hidden in the maps shown by Bosilovich et al. (2002) and van der Ent et al. (2010) because they only show the fraction of con-

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tinental moisture in continental precipitation or, in other words, they mask the ocean. In Sec. 2 we now give an overview not only on moisture recycling and the different measures that have been used to quantify it, but also on local coupling and the effects of continental evaporation on the large-scale circulation. We believe that the clarity of our paper as a whole is now strongly enhanced.

EC3: The same holds for terminology that labels bulk recycling methods as “traditional”, the use of the term “response” rather than “coupling” or “interaction” when referring to e.g. $\Delta P/\Delta E$, the assumed identity between “runoff” and “P-E” on sub-annual time scales, the use of the term “compensation” in a system that is shown to have a “positive feedback” (3513), and the terms used to identify spatial scales (“local”, “regional”, “<1000km”).

AR3: We have rethought and changed many aspect of our terminology. For example, we have in the new manuscript abandoned the term "traditional" completely and use the terms "coupling" and "compensation" only where appropriate. Furthermore our terminology is now explicitly introduced in sections 1 and 2, including a figure (Fig. 1) that contrasts three different mechanisms: "Moisture recycling", "local coupling", and "circulation". The scale-issue has now become a central aspect of our paper, see for example Sec. 2.4. We must however contradict the statement that we have assumed "runoff" and "P-E" to be identical on sub-annual time scales. Rather, we used the term "aerial runoff" (moisture flux divergence) and assumed it to be identical with " $-1*(P-E)$ " on the monthly time scale, which is also not exactly correct because of the change in precipitable water, but a much better approximation because the monthly change in soil-water storage is typically considerably larger than the monthly change in precipitable water. We now do not use the term "aerial runoff" anymore.

EC4: The first reviewer (Paul Dirmeyer) has phrased his concerns about a lack of atmospheric dynamic perspective in fairly strong phrases addressing directly the assumed lack of relevant expertise of the authors. Although I do understand that these comments could be interpreted as a personal accusation to the address of the authors,

I prefer to consider the intentions of his remarks, namely to improve the underlying analysis.

AR4: Yes, we indeed interpret the sentence "I fear this shows a lack of background on the part of the authors in basic meteorology" as a personal accusation that in our opinion is inappropriate in a scientific review, in particular in a public one. We are wondering if it would not have been the editor's responsibility to take care that the referee's comment conforms to paragraph 3 of HESS's obligations for referees: "In no case is personal criticism appropriate." We however condone the inappropriate formulation and focus on the content of the remarks.

EC5: He (Paul Dirmeyer) is not very explicit about the processes that are being overlooked nor about the hypothesis that one would use as starting point to study the convection parameterization in the MPI model.

AR5: We now discuss the uncertainty associated with local coupling in climate models (Sec. 1.2) and address this uncertainty by additional experiments with another convection scheme (Sec. 3.1, Sec. 5 para 2, Fig. S2).

EC6: However, reviewer 3 gives a very good example of the potential role of atmospheric dynamics in this study: changing the large scale surface temperature structure in the DRY experiment may well alter the systematic moisture transport between ocean and atmosphere, which may be an important mechanism explaining part of the mismatch between the patterns of RMF and $\Delta VIM/\Delta P$.

AR6: A substantial fraction of the manuscript is now dedicated to the response of the large-scale circulation (Sec. 2.3 and 5.2), please see also the general response.

EC7: Also, I find it striking that the phrase "moisture flux convergence" does not appear in the manuscript, and I agree with reviewer 1 that this is an important diagnostic describing the current (and perturbed) state of the atmospheric moisture budget.

AR7: Instead of "moisture flux convergence" we are analysing the changes of P-E

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(which on the monthly time scale is not identical but quite similar because changes in precipitable water are comparatively small, see AR3). Furthermore we now analyse the changes in the large-scale circulation by means of the total mass-flux field of the lower half of the atmosphere and its divergence, which approximately translates into the vertical velocity at 500 hPa (Sec. 5.2, Fig. 7).

EC8: Finally, the degree to which the GCM is able to respond correctly to such a drastic change in the surface evaporation should be questioned, given e.g. the evidence that many models don't do a good job on representing parameterized convection/precipitation responses to surface perturbations (e.g. Hohenegger et al, 2009).

AR8: The issue of model-uncertainty associated with local coupling and the parameterisation of moist convection is now a major part of Sec. 2.2, where also the results of Hohenegger et al. (2009) are discussed. Furthermore we argue that, while effects due to local coupling are rather uncertain, the response of the large-scale circulation is probably to a smaller extent model dependent (see Sec. 6 para 1).

EC9: To my opinion, the authors do a very good job in illustrating the implications of using a simple conceptual model (ΔP proportional to ΔVIM) when trying to interpret effects of land use change on the local and remote hydrological cycle. With their (drastic) model experiment they demonstrate that the real response of the hydrological cycle does not obey that simple conceptual model. However, I do share the remarks of the reviewers that the doubts about the "traditional" recycling analysis are expressed somewhat too strongly, given their usefulness when looking at perturbation experiments (suggested by reviewers 1 and 2).

AR9: We agree that our conclusions have been too strong in the original version. Yet we do not fully understand where the conclusion of "their usefulness when looking at perturbation experiments" is based on. What kind of perturbation experiments are meant? As far as we know it has not yet been demonstrated that recycling estimates are indeed useful to infer responses to perturbations in evaporation. Instead, for us

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the situation looks as if, from a theoretical point of view, there is hope that at least responses to infinitesimal perturbations can be properly inferred from recycling estimates. But, as we discuss in Sec. 2.4, there are indications that for sufficiently small perturbations the response is dominated by local coupling rather than moisture recycling (see Schär et al. (1999)). We rather think (and discuss it in the new manuscript) that it remains an open question whether there is a scale of the perturbation at which moisture recycling estimates might be promising indicators (Sec. 2.4, 6, and 8).

EC10: I like the suggestion of reviewer 2 (Ruud vd Ent) to (a) make a distinction between areas where you can and where you cannot expect this conceptual model to be valid, and (b) to make some more quantitative assessment of the precipitation changes in response to the DRY experiment by plotting the absolute precipitation changes together with the results shown in Fig 4. Options (a) and (b) could be combined to separate analyses in (a) for areas that have been masked depending on the results gained by (b).

AR10: Regarding point (b) we have added figures showing absolute values to complement the relative differences. Point (a) is a very interesting aspect, and we do discuss to some extent where (in contrast to other regions) the simplified view seems to help (in July foremost in Eurasia and North America). We believe, however, that the strategy suggested by the referee could be realised much better with experiments where perturbations are imposed to smaller regions. With such experiments one could better disentangle the effects from local coupling and the effects from changes in the circulation (because we expect the latter to be comparatively weaker in response to spatially smaller perturbations). We believe in general that experiments with global models in which perturbations are imposed to smaller regions will help to advance the question we pose in our title.

EC11: Also the comment of reviewer 3 is valid that the diagnostic plotted in fig 4 ($\Delta P/\max(\text{PREF}, \text{PDRY})$) can lead to ambiguous conclusions.

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AR11: We do not completely share this concern. However, since our (as we think elegant) way of normalising relative differences obviously leads to irritations, we now apply a "trick" to show exactly the same figures without (hopefully) provoking confusion anymore, please see AR4 in our response to referee #3 for details.

EC12: The reviewers give some more useful hints and relevant citations, that I encourage the authors to consider in the next version of this paper. I am looking forward to seeing this next version.

AR12: We have taken all hints and comments of the referees and the editor very serious. They considerably helped us to improve the manuscript.

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

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