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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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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 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.

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. 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").

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. He 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. 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 $\Delta VIM/\Delta P$. 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. 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).

To my opinion, the authors do a very good job in illustrating the implications of using a simple conceptual model ($\Delta P \sim \Delta VIM$) 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). 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). Also the comment of reviewer 3 is valid that the diagnostic plotted in fig 4 ($\Delta P/max(P_{REF}, P_{DRY})$) can lead to ambiguous conclusions.

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.

References

Hohenegger C., P. Brockhaus, C. S. Bretherton, and C. Schär, 2009: The soil-moisture precipitation feedback in simulations with explicit and parameterized convection. J. Climate, 22, 5003-5020.

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