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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

R. J. van der Ent (Referee)

r.j.vanderent@tudelft.nl

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Overall

The work presented in this manuscript investigates the capability of continental precipitation recycling ratios to estimate to effect of land-use changes on precipitation. Apart from some style considerations (I will come back to that) the manuscript is generally easy-to-read, with a decent structure and a good level of English.

In summary, the work calculates moisture recycling estimates similar to previous

work (Bosilovich et al., 2002; van der Ent et al., 2010; Yoshimura et al., 2004), but with different data. The new element is that the authors also run a rigorous land-use change scenario in order to assess the importance of the moisture recycling in predicting the effect of land-use change. The idea in itself seems useful, but I think that using a rigorous land-use change scenario alone, namely complete elimination of continental evaporation, is too extreme to answer this question completely. Realising they chose an extreme scenario, the authors do interpret their results with caution in some parts of the manuscript, but not in others.

Although, the results still seem very interesting, I disagree with the authors on the interpretation. I have the feeling that the authors' interpretation of the results is somewhat biased to the conclusion that moisture recycling estimates are mostly useless, except in particular cases. My interpretation is that moisture recycling estimates are very useful, except in cases where other climatic effects are dominant, which admittedly happens a lot in this manuscript, but would probably be less in less extreme land-use change scenarios. Considering that my interpretation might be biased to the conclusion that moisture recycling estimates are actually very useful, this opinion difference should not be considered an argument in the acceptance of the manuscript, but I do think that the authors should be even more aware of the rigorousness (up to 16K temperature increase) of their land-use change scenario. Therefore, I would like to see the interpretation structure turned around in first discussing the regions where moisture recycling is important, and thereafter where it is not and also why not. I mean that the authors should discuss whether the fact that moisture recycling estimates do not seem useful in certain regions is a general rule, or that it only is true for this extreme scenario?

Specific comments

Recycled moisture fraction (RMF) and vertically integrated moisture (VIM). I know

it is a matter of taste, but I personally think it is not very elegant to use such an abbreviation rather than a single italic displayed symbol (e.g. R for RMF and M for VIM). In the text I would write it out in most of the cases. I know this is more words, but at least somebody will also have the possibility to cross-read the manuscript. Furthermore, there is RMF_{reg} and RMF, while the latter usually refers to continental recycling, it would be more clear than to call this RMF_{con}. Or better a short symbol like R_c.

Evaporation-precipitation interactions or coupling. Usually, the term interaction is used when the authors refer to something which is different from the interaction through the water budget change (which is also an interaction/coupling). Probably they mean a change in the energy budget or something similar, but I recommend the authors to be more explicit (throughout the manuscript) in what they mean rather than talking about "interactions" or "coupling".

Although similar, RMF as defined in Eq. (2) is not necessarily the same as the continental moisture recycling estimates given by (Bosilovich et al., 2002; van der Ent et al., 2010; Yoshimura et al., 2004). Because these studies referred to a fraction of precipitation rather than a fraction of atmospheric moisture. This should be noted somewhere.

3508-7 - 3508-10: Recent studies indicate that at small scales (up to 1000 km) local to regional evaporation-precipitation coupling by far dominates the atmospheric precipitation response, while the water balance effect from moisture recycling in the traditional sense seems to be of minor importance.

I cannot find this scale (up to 1000 km) explicitly back elsewhere in the manuscript. Please make the cross-reference between abstract and the rest of the manuscript more clear or refrain from using such strong statements in the abstract.

3508-26 - 3509-1: Over the ocean the hydrological response is ambigious, even where under present-day conditions large fractions of the atmospheric moisture stem from continental evaporation. This suggests that continental moisture recycling can not act across large ocean basins.

Ambiguous is misspelled ambigious. The last sentence is strange. Continental moisture recycling is the feedback of moisture from continental surface to continental surface, so by definition it does not act over oceans. What the authors probably want to say is that continental moisture sources play little role in affecting continental precipitation. I wonder whether they should say this at all, but I will come back to that.

3509-3 - 3509-5: In large parts of the continents the precipitation decrease compensates for much of the missing evaporation, such that the continental moisture-sink is not much amplified.

I fail to understand especially the last part of the sentence, amplified in what sense?

3509-25 - 3509-27: This water loss is particularly pronounced where the atmosphere's supply of moisture (precipitation) and demand for moisture (potential evaporation) are markedly displaced in time.

I would understand better if the authors could just explain this in more hydrological terms: wet and dry season, precipitation and evaporation, rather than using economical terminology.

3510-16 - 3510-19: Early studies on this issue aimed at estimating the contribution of evaporation from a particular region to precipitation inside the same region (e.g. Benton et al., 1950; Budyko, 1974; Lettau et al., 1979; Brubaker et al., 1993; Eltahir and Bras, 1994; Savenije, 1995a; Trenberth, 1999; Burde and Zangvil, 2001; Fitzmaurice, 2007).

I would add: Burde, (2006) and Schär et al., (1999), because those are also uniquely different bulk recycling methods. I suggest to omit Savenije (1995a) from this list, because in that work in fact an atmospheric streamline is followed to compute the recycling ratio at a certain point rather than an areal average predictor is given (see also van der Ent and Savenije, 2011).

At the end of 3511-7 I suggest to refer to van der Ent and Savenije (2011), because this paper extensively discusses the scale- and shape-dependency problems of regional moisture recycling ratios, and in fact also proposes a solution.

3511-15 - 3511-17: This approach has been adopted by Numaguti (1999), Bosilovich et al. (2002), Yoshimura et al. (2004),and van der Ent et al. (2010). Although the reference to Bosilovich in GEWEX News is given I suggest to add reference to Bosilovich and Schubert (2002) as well, because that paper provides more details of their applied model.

3512-20: evapotranspiration

I very much appreciate that the authors throughout the manuscript describe the phase transition of water to water vapour with the term evaporation rather than the ambiguous term evapotranspiration, but please also do it in this sentence.

3512-27 - 3512-29: Even if an important aspect for understanding evaporation-precipitation interactions lies in the local to regional interactions, traditional moisture recycling may have its place in the large-scale picture.

Be explicit, also in the next paragraph, where the term local interactions is used. What are they exactly?

3513-17 - 3513-18: Would recycling estimates still be able to tell something about downstream consequences of upstream land-cover change?

This is the most important point of the paper. Yet, the problem with the extreme experiment is that not only the upstream land-cover is changed, but also the downstream and therefore one cannot distinguish anymore between the local and the upstream causes of downstream precipitation change. Thus, the authors should interpret results with caution and provide an outlook for further research on how this question could be assessed better.

3513: I am, and probably many HESS readers are, not familiar with the term equilibrium experiments.

3513-27: Typo, per definition, should be by definition.

I suggest to put the dimensions in brackets behind the equations, especially in Eqs. 5 and 6.

I like Eq. 5, this makes the calculation a bit easier compared to my own model (van der Ent et al., 2010), but has the disadvantage that RMF cannot be calculated relative to precipitation, i.e. yielding real precipitation recycling ratios, rather than atmospheric states? Could the authors comment on that (not necessarily in the manuscript)?

3517: Steep RMF gradients occur where strong evaporation combines with moderate horizontal moistureflux density (e.g. tropical Africa), or where the air flows perpendicular to a steep evaporation gradient (e.g. Sahel, particularly in January), or a combina-

tion thereof (e.g. China in July).

It should be noted that a transition from 10 to 20% recycling is not the same moisture exchange with the atmosphere as a transition between 60 and 70%. Thus, the gradients alone do not say everything.

3519-8 - 3519-11: Although it seems that the changes in the atmospheric moisture content can partly be explained with the RMF patterns in some regions, for example around India and China during January, the overall contradiction casts doubt on a general causal relation.

I personally think it is more important to focus on these regions where moisture recycling estimates seem to work (also South America in both January and July, seem quite ok and also Congo seems not so bad in January), rather than the Arctic and Antarctic regions, which are discussed elsewhere.

3519-20 - 3519-22: Irrespective of its causes, this distinct vertical structure indicates that results obtained with vertically integrative moisture-budget models should be taken with a grain of salt.

I am confused, does this mean I should not look at Fig. 2? Be specific.

Paragraph 4.2. It would be interesting if the authors could give a percentage of precipitation reduction for each continent and for the globe. This could be compared with (van der Ent et al., 2010, Table 2, column 5) or perhaps better their own data from the REF experiment.

3520-15 - 3520-18: This suggests that continental moisture recycling can not act across large ocean basins, i.e. inter-continental, but only intra-continental. To give

a simple example, Eurasia is not affected by North America's evaporation and vice versa, regardless of the substantial fraction of moisture they receive from each other.

In the DRY experiment, both continents are extremely hot (in July), bare rock deserts, so it is not more than logical that they are not so much affected by boundary conditions. To reach a stronger conclusion on this issue I suggest that the authors (perhaps not in this study but in a follow-up study) do a GCM run with only Eurasia's or North America's evaporation turned to zero and see whether this affects the other continent.

3520-28 - 3521-2: In July, precipitation in southern Africa, which is already low under present-day conditions, decreases by almost 100%, although the RMF indicates that under present-day conditions only about 10% of the atmospheric moisture is of continental origin. The situation is similar in Australia.

Here a response is observed in a region you would not expect from the recycling pattern (Fig. 1.). But it is not so shocking, in those regions it rains only of few millimetres in July also in the REF experiment. So, the absolute difference is not so big. I would be more interested in the regions where one expect a big precipitation drop (relative and absolute) based on Fig. 1, but where it does not happen. In fact, there are not so many of these regions.

Paragraph 4.4 Response of the atmospheric circulation. A figure would be really helpful in understanding the text.

3528-7 - 3528-12: This direct comparability is achieved at the expense of realism: the complete suppression of any continental evaporation is far from any realistic land-cover change scenario. We can not rule out that recycling estimates gain significance to infer precipitation changes when the land-cover modifications are more realistic, i.e. less extreme in spatial extent and in the degree of evaporation reduction.

I recommend this cautionary note to be given directly at the beginning of Sect. 4.

3530-6 - 3530-8: Apart from these exceptions, our results question the relevance of traditional moisture recycling estimates even for continental scales - an admittedly counterintuitive conclusion.

Well, there is much less precipitation in the DRY experiment, and that is what is expected.

In the references there are page numbers after the year. In other HESSD papers this does not seem to be the case. In some references I think that not all the initials of the authors are given.

In the figures the chosen projection results in a very big polar regions. Therefore, they draw more attention than they might actually deserve. If possible, I suggest the authors go for a Robinson projection in the revised manuscript.

In Figs. 4 and 6 only the 99% significant values are shown and the Wilcoxon rank-sum test is suddenly introduced, with a cryptic sentence about no significant 1-year lag autocorrelations in the data. I do not understand how to interpret this. Suppose there is a 50% rainfall decrease beteen the REF and the DRY, can it than still be that it is shown in white? Personally I just want to see the differences as calculated irrespective of their statistical significance, which seems strange in this context anyway, since the study is not a trend or correlation analysis.

The colours chosen in the figures are difficult to interpret. This is especially important in Fig. 1, left, because here one definitely wants to see the difference between each

box, but everything between 50 and 80% is the same colour for my eyes (and probably for many other eyes as well).

Conclusion

In conclusion, I think this is interesting work, certainly suitable for HESS, but I recommend that the authors in a revised manuscript at least:

1. Come up with a more nuanced way of describing their results whilst keeping in mind the limitations of this single extreme land-use change scenario.

2. Provide a graph from which the change in atmospheric circulation can be interpreted.

3. Provide some outlook for (their) future research, including a description of which additional GCM experiments should be run to come up with stronger conclusions on the capability of continental precipitation recycling ratios to estimate the effect of land-use changes on precipitation.

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