

## ***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***

**H. F. Goessling and C. H. Reick**

helge.goessling@zmaw.de

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Response to referee #2 (Ruud van der Ent)

Please read the general response prior to this response.

RC: Referee Comment

AR: Authors' Response

RC1: 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

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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.

AR1: We completely agree that our study can not completely answer the question whether recycling estimates can be used to predict the effect of land-use change, and we are not claiming it can. We have realised that we have not made this clear enough in the original manuscript, but we think that the new version makes this point very clear.

RC2: 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 rigorosity (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?

AR2: Again, we agree that the way our study is designed we are not able to com-

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prehensively answer the question whether moisture recycling estimates are useful for more realistic perturbations, and we have not made this point clear enough in the original manuscript. However, at the moment we see no scientifically sound basis for the referee's claim that "moisture recycling estimates are very useful", at least if by "useful" the applicability of recycling estimates to infer the response to changes in land evaporation is meant. Indeed, exactly because of this missing justification of usefulness we conducted our study. We are convinced that in order to understand under which conditions recycling estimates are useful, one needs to get into view other processes that may confound their prognostic value. Insofar, our case study only serves the purpose to let those other processes (local coupling, large scale circulation changes) show up clearly. And this is exactly why we choose to use such a strong perturbation, because then all relevant processes must show up. Moreover, we would like to remark that we consider it very honest of the referee to admit the possibility that his view might be biased in a certain direction, and very much appreciate how he comments on the consequences of this potential bias for the acceptance of the manuscript.

RC3: 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<sub>reg</sub> and RMF, while the latter usually refers to continental recycling, it would be more clear than to call this RMF<sub>con</sub>. Or better a short symbol like *R<sub>c</sub>*.

AR3: We also consider this to be a matter of taste, yet we have changed many terms: For example, "Recycled moisture fraction" (RMF) is now in the new manuscript termed "continental recycling ratio" (*R<sub>c</sub>*) and "vertically integrated moisture" (VIM) is now "precipitable water" (*W*).

RC4: Evaporation-precipitation interactions or coupling. Usually, the term interaction is used when the authors refer to something which is different from the interaction through

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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".

AR4: We hope that by our changes this point is now very clear in the new manuscript, see Sec. 1 and 2.

RC5: 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.

AR5: We have now changed to the more common way to aggregate continental recycling ratios over time, so we now show the same quantity as Bosilovich et al. (2002), Yoshimura et al. (2004), and van der Ent et al. (2010). We provide an additional figure showing the differences (which are quite small for monthly means) in the supplementary material (Fig. S1).

RC6: 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.

AR6: The scale-issue is now a central point of the paper (Sec. 2.4). The 1000 km estimate stems from the domains (e.g. "France") considered in Schär et al. (1999).

RC7: 3508-26 - 3509-1: "Over the ocean the hydrological response is ambiguous,

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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 ambiguous. 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.

AR7: We do not say that "continental moisture sources play little role in affecting continental precipitation" in general, but rather that moisture sources on one continent play little role in affecting precipitation on another continent (at least not through moisture recycling) when there is a large ocean in between that separates the two continents. The referee writes that "Continental moisture recycling [...] by definition [...] does not act over oceans." We are not sure if the referee intentionally used the term "over" instead of "across". Assuming that he means the same as "across", we do not completely agree with this statement. In his own study (van der Ent et al. 2010) the referee keeps track of continental moisture also over the ocean, as we (together with Numaguti (1999), Bosilovich et al. (2002), and Yoshimura et al. (2004)) do in our study. In consequence the continental (precipitation) recycling ratio of moisture arriving in Eurasia from the Atlantic in northern summer is substantially larger than zero, mainly due to moisture stemming from North America. (Accordingly, the "continental evaporation recycling ratio" of moisture evaporated from North America in July is partly attributable to the fraction that precipitates in Eurasia.) Our interpretation of this type of evaluation of recycling ratios is that recycling is "allowed" to (or believed to) act across large ocean basins, meaning that continental moisture recycling is not by definition constrained to be an intra-continental phenomenon only. However, we guess that in reality continental moisture recycling actually is more or less constrained to be an intra-continental phenomenon only, at least if the ocean in between is sufficiently large. An interesting question could be how large is "sufficiently" large, but that is beyond the scope of our

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current study.

RC8: 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?

AR8: What is meant here is the effect on P-E, which is the degree to which the surface is a sink for atmospheric moisture. Assuming that P-E is positive in the reference situation (precipitation exceeds evaporation, which in the long-term mean reflects terrestrial runoff), the elimination of evaporation - without a response of precipitation - results in accordingly increased P-E because E is zero in this case: The continental moisture-sink is amplified. If on the other hand precipitation is reduced strongly in response to the elimination of evaporation, then the continental moisture-sink is not that much amplified (or it may even be weakened). We hope that this is clearer in the new manuscript (Sec. 5.3).

RC9: 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.

AR9: The term "atmospheric demand" is widely used in atmospheric science and described in the Glossary of Meteorology of the American Meteorological Society as the following: "The evapotranspiration that would be achieved from a well-aerated soil/plant surface at field water-holding capacity". However, in the new manuscript we are using these terms anymore (Sec. 5.3).

RC10: 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;

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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.

AR10: We have added Burde (2006), removed Savenije (1995a), and now refer to van der Ent and Savenije (2011) at several places. However, we have not added Schär (1999) here, because the study focuses mainly on local coupling.

RC11: 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.

AR11: We decided to use only one reference for each of the four studies, so in case of Bosilovich et al. we decided to give the reference where the global results are shown. People interested in the methodological details will find the reference therein.

RC12: 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.

AR12: Here we cite Seneviratne et al. (2010) and prefer to leave the original citation unchanged.

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RC13: 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?

AR13: The terminology is now introduced explicitly in Sec. 1 and 2.

RC14: 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.

AR14: The new manuscript version is more cautious (see also AR1 and AR2). We also agree that additional studies are needed to be better able to disentangle local and remote influences. Again, in the new version the scale-issue is a central point.

RC15: 3513: I am, and probably many HESS readers are, not familiar with the term equilibrium experiments.

AR15: Numerical climate models with constant external forcing (without change in e.g. solar forcing) usually need some time (the transient phase) after being started from some initial state to reach a quasi-equilibrium. In quasi-equilibrium the model's state vector fluctuates around the mean climate, while the mean climate is stable and does not drift anymore as in the transient phase. Such model experiments in which one applies a constant external forcing (in our case including climatological SSTs) and lets the model develop a quasi-equilibrium state are commonly referred to as "equilibrium experiments". How long it takes to reach quasi-equilibrium depends on the time scale of the slowest involved processes. While this can be many centuries in models com-

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prising an interactive ocean or "dynamic" vegetation, the slowest component in our experiment is soil moisture with a time-scale up to a few years. This is why we omit the first few years (the transient phase) from our analysis. The term is widely used in climate science, but we have added a short explanatory remark in brackets (Sec. 3.1 3rd para).

RC16: 3513-27: Typo, per definition, should be by definition.

AR16: Yes.

RC17: I suggest to put the dimensions in brackets behind the equations, especially in Eqs. 5 and 6.

AR17: We prefer to leave the equations without dimensions.

RC18: 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)?

AR18: For single time-steps the measures are identical. Differences arise only when temporal averages are formed. While in the original version we used simple temporal means, "real precipitation recycling ratios" are obtained by weighting with precipitation rates. Therefore, if data on precipitation rates are available, it is possible to compute both "real precipitation recycling ratios" and simple temporal means. We comment on this in the new manuscript (Sec. 3.1, last paragraph) and provide a figure showing the results from both averaging-techniques in the supplementary material (Fig. S1). For details please see our response to referee #1, AR2 and AR6.

RC19: 3517: "Steep RMF gradients occur where strong evaporation combines with moderate horizontal moisture flux density (e.g. tropical Africa), or where the air flows perpendicular to a steep evaporation gradient (e.g. Sahel, particularly in January), or a

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combination 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.

AR19: Yes. The new manuscript contains the following sentence: "Since  $R_c$  is bound between zero and one (as follows from Eq. 5) and, hence, saturates when approaching these bounds, the  $R_c$ -gradient also depends on the value of  $R_c$  itself." (Sec. 4, 1st para)

RC20: 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.

AR20: The essence is that recycling estimates are not generally applicable to "predict" the hydrological consequences of evaporation changes, at least in response to continental-scale perturbations. That recycling estimates are not generally applicable does not mean they could not still be applicable in less extreme scenarios. The scale-issue is now a central point in our paper (e.g. Sec. 2.4).

RC21: 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.

AR21: The vertical profiles are no longer part of the manuscript, because they are not central to the study.

RC22: 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

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with (van der Ent et al., 2010, Table 2, column 5) or perhaps better their own data from the REF experiment.

AR22: The new manuscript contains a table with areal averages for continental-scale regions. The values support the idea that moisture recycling is actually taking place. However, we argue further that the drying occurs at the "wrong" places of the continents and focus on the intra-continental Rc-gradients to show this. The comparison with the annual-mean table in van der Ent et al. (2010) is problematic because we focus on July.

RC23: 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.

AR23: We fully agree and are currently exploring such kind of experiments.

RC24: 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.

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AR24: The way our experiments are designed we can not well distinguish local (non-budget) and remote (budget) influences over land because all land is perturbed at the same time (see RC14/AR14). That is why one can find only few "regions where one expects a big precipitation drop (relative and absolute) [...], but where it does not happen". On the other hand there are regions without direct perturbation in the current experiments, namely the oceans. Here, many regions with high continental recycling ratio in the REF experiment do not experience drier conditions in the DRY experiment. This is particularly obvious in the Arctic in July, but is not constrained to the high latitudes: The downstream "tails" with high RMF at the western coasts of South America and Africa are not reflected in the response of precipitation or precipitable water. We agree that the precipitation response is not so "shocking" given that large parts of these regions experience few absolute precipitation already in the reference case because they largely overlap with the subsiding branches of the Hadley/Walker circulation. The new manuscript contains figures of absolute values to complement the relative difference plots. But there is also no such response in the atmospheric moisture content which should also be taken into account when investigating the validity of the simple moisture-budget conception. After all, further work is needed to come to more conclusive results.

RC25: Paragraph 4.4 Response of the atmospheric circulation. A figure would be really helpful in understanding the text.

AR25: The changes in the circulation are now a central aspect in our new manuscript (Sec. 2.3 and 5.2).

RC26: 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.

AR26: The new version of the manuscript reflects the limits of our study much more pronounced.

RC27: 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.

AR27: Yes, and the general drying of the continents is surely at least partly due to moisture recycling (see Sec. 5.1 in the new manuscript). However, the spatial pattern of the precipitation changes does not agree well with what continental recycling ratios suggest because other factors, in particular the large-scale circulation, dominate the response.

RC28: 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.

AR28: To us it looks like the references have page numbers in front of the year, the latter being the last entry of each reference. Is that not correct? We added initials where we found them missing.

RC29: 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.

AR29: Any projection has its advantages and disadvantages, and we consider this to be rather a matter of taste. We did not choose this projection with the intention to highlight the polar regions, and we prefer to stay with the current projection.

RC30: 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

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there is a 50% rainfall decrease between the REF and the DRY, can it then 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.

AR30: In the new version of the manuscript we show the whole response pattern regardless of statistical significance. This is OK because all striking patterns are also statistically significant. Based on the significance test only a few noisy values in regions of infrequent precipitation in both experiments were removed. We must however contradict the statement that statistical testing is constrained to "trend or correlation analysis". Statistical testing can also be important for the comparison of different data, e.g. from two model experiments, to see whether differences are probably "real" or just due to "noise". The applied test is only applicable to uncorrelated data. This is why in the original manuscript the "cryptic sentence" on the autocorrelation in the data was necessary. The referee poses the following question: "Suppose there is a 50% rainfall decrease between the REF and the DRY, can it then still be that it is shown in white?" Yes. If for example in a grid cell in a very dry region it rains only once in 30 years in one experiment, and not at all in the other experiment, the relative difference would be 100% (or infinity, depending on which experiment you put in the denominator), but the difference would not be statistically significant.

RC31: 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).

AR31: We agree that the contrast between the colours is suboptimal, although this may be display/printer dependent. Because of the suboptimal contrast we additionally included contour lines in Fig. 2 and Fig. 5, which are relatively smooth fields (whereas the field of precipitation differences, Fig. 4, is not so smooth). Despite the suboptimal contrast we decided to use only red colours in the Rc field in order to allow for direct

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comparison to Fig. 4 (bottom) and Fig. 5 (bottom). The colour bars of the latter have to accommodate also values of opposite sign, which we realised by using blue colours. After all we would like to stay with the current colour bars. However, Fig. S1 in the supplementary material shows the continental recycling ratios with a better resolvable colour scale.

RC32: 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.

AR32: We think the new manuscript fulfills this demand.

RC33: 2. Provide a graph from which the change in atmospheric circulation can be interpreted.

AR33: We realised this in Sec. 5.2 (Fig. 7).

RC34: 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.

AR34: In the new manuscript we make clear that our study is only a first step to assess the meaning of moisture recycling estimates, and we argue repeatedly that smaller-scale perturbations are an important aspect that should be addressed by future work.

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