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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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Response to referee #1

Please read the general response prior to this response.

RC: Referee Comment

AR: Authors' Response

RC1: There is much talk throughout the paper of "intuition" and results seeming "counterintuitive". I fear this shows a lack of background on the part of the authors in basic

meteorology. The lack of geographic correspondence between changes in VIM, precipitation, etc., would not come as such a surprise to someone who has studied dynamic meteorology, particularly moist atmospheric dynamics and physics. Basic analytical approaches like perturbational analysis, scale analysis, and an understanding of the coding and operation of a GCM (especially parameterizations of physical processes like moist convection) provide a researcher with the insight to recognize and diagnose many of the results presented. I believe the paper would benefit greatly from a coauthor with a background in atmospheric science to provide this missing perspective.

AR1: First of all we acknowledge that many of the referee's critical comments have really helped us to improve the manuscript. However, regarding the form of the criticism, we consider the comments on our alleged lack of background in atmospheric science inappropriate. We think that it is not the task of a referee to judge the authors' background, but to focus on the content of the manuscript. In our opinion such judgements regarding the authors' competence are generally inappropriate in scientific reviews, no matter if the referee's impression is correct or not. Such dismissive formulations, although not decent, may be tolerable in a private review process, but are particularly improper in a public review process as in HESS. We feel supported by paragraph 3 of the "General Obligations for Referees" of HESS, which says "[...] In no case is personal criticism appropriate."

Regarding the content we believe that there are two main deficiencies of our manuscript that lead to the referee's impression:

First our language involving terms like "intuitive" has not been chosen to express our own surprise about the model results, but relates to the "simple conception" of a straight-forward relation between source-target relations and the actual response to changes at the source. This simple conception is not pure invention. Just to give an example, de Groen and Savenije (1995) write in their abstract that "During rainfall events a portion of the precipitable water in the atmosphere reaches the ground. [...] the atmosphere can only generate rainfall downwind if the precipitable moisture is again replenished by evaporation. Since the actual evaporation on average is less than the rainfall, the rainfall downwind decreases. Land degradation decreases the replenishment and hence the rainfall downwind. Southeastern Africa suffered from severe droughts in the past decennia. In the same period large scale land use change took place. [...] If the method proves applicable, than estimates can be given about the consequences of land use changes in certain regions on rainfall elsewhere.“ Van der Ent et al. (2010) write "This study makes use of new definitions of moisture recycling to study the complete process of continental moisture feedback.", and "The magnitude of moisture recycling can be used as an indicator for the sensitivity of climate to landâĂŘuse changes." We are not saying that this reasoning is a priori wrong, but we argue that the validity of this conception and/or the conditions under which this conception holds have to our knowledge so far not been investigated. The referee writes in an own paper (Dirmeyer and Brubaker, 2007): "A change in regional evapotranspiration affects not only the supply of water carried by the circulation of the atmosphere, but can thermodynamically alter the atmosphere itself by changing the partitioning of surface heat fluxes, triggering changes to the circulation patterns as well. Nevertheless, the basic linear model behind many people's conception of recycling has been hard to shake." We argue that "the basic linear model behind many people's conception of recycling" is still wide-spread and is currently based exclusively on intuition rather than a rigorous derivation. Our paper is meant to make a clear point, showing that this linear model is not necessarily useful, at least for large perturbations. We understand from the referee's papers, in particular from the sentence we just cited, that we agree on this aspect. We have reorganised the paper to make things much more clear, in particular the introductory part, and we hope that it does not seem anymore as if we were "surprised" that the response does not conform to "the basic linear model behind many people's conception of recycling".

Second, in the original version we have on purpose chosen not to focus on the atmospheric processes that are responsible for the atmospheric response. Instead we treated the atmosphere almost as a "black box" and tried to make a clear point showing

only that the simple conception of moisture recycling is not necessarily valid. However, we have to admit that in the few loose remarks we made concerning the underlying reasons of the observed results we have underestimated the changes in the large-scale circulation which actually explain a great deal of the response. A substantial fraction of the manuscript is now dedicated to the response of the large-scale circulation (Sec. 2.3 and 5.2).

RC2: The terms "recycling" and "RMF" used throughout the paper are misnomers. Recycling can be defined either relative to the input or output side of the atmospheric branch of the hydrologic cycle. The most common definition is the fraction of precipitation over a defined area that originated as evaporation from the same area. Similar but not identical is the definition of the fraction of evaporation from a defined area that fell as precipitation over the same area. van der Ent et al. (2010) compares these two views. Figure 1 suggests neither definition is in effect, as plots of RMF have values over land and ocean (they should be blank over ocean in the standard sense). What is shown, if I am not mistaken, would be better called the fraction of "terrestrial" moisture in the air than recycling fraction or ratio. Likewise, Mr, defined as "recycled" moisture, is really terrestrial moisture (as opposed to Mo = oceanic moisture). I would think something cannot be "recycled" until it is returned to the location of origin; while still in the air, none of the moisture is "recycled". This terminology should be corrected throughout the paper, as it is quite confusing to interpretation of results.

AR2: First we are aware of the two different types of recycling ratios, which in van der Ent et al. (2010) are termed the "continental precipitation recycling ratio" and the "continental evaporation recycling ratio". As Numaguti (1999), Bosilovich (2002), and Yoshimura (2004), in the current study we are dealing only with the "continental precipitation recycling ratio". Second in case of continental recycling ratios "recycled moisture" is indeed identical with "moisture of continental origin", not only in our study but in all the four existing studies that present continental recycling ratios (Numaguti (1999), Bosilovich (2002), Yoshimura (2004), and van der Ent (2010)). In the maps shown by

Bosilovich et al. (2002) and van der Ent et al. (2010) this fact is hidden because they only show the fraction of continental moisture in continental precipitation or, in other words, they masked out the ocean. To enhance clarity, we now do not show simple temporal means anymore (as Numaguti (1999)), but the more common precipitation-weighted means (as the authors of the other three studies cited above) and provide a discussion of the differences in the supplementary material (Fig. S1). It turns out that the differences are quite small for monthly means.

RC3: Curious that the recycling estimates performed using reanalysis-based back trajectory calculations have not been cited, as they are free from many (but not all) of the problems of bulk calculations and tracers in unconstrained GCMs. Papers most relevant to this study: [...]

AR3: We now provide a more complete overview on existing moisture-recycling studies (Sec. 2.1). Therein we also included the paper in which the back-trajectory method is applied globally.

RC4: Sec 2.1, Para 1: More relevant to the results will be the convective parameterization of the model. Please cite the scheme used, and describe the physics of convective triggering in the GCM. Is there both a dry and moist convective parameterization? Shallow and deep convection? This is highly relevant to your results and will help to provide understanding of the model's response in the dry case.

AR4: To account for this uncertainty we have repeated the experiments with a second convection scheme and briefly describe both of them in Sec. 3.1, last paragraph. However, the response is very similar.

RC5: P3514, L10: Because the atmospheric response to SST anomalies such as those with ENSO variations are not "equal and opposite", using climatological SSTs can introduces biases in the means of atmospheric quantities, relative to a long simulation with observed SSTs that have the same time mean. This should be acknowledged.

AR5: Given the large perturbation we consider this effect to be subordinate, but we mention this aspect in the new version of the manuscript.

RC6: Sec 3, Last Para: I don't think you can say that your estimates "agree well" with the cited papers, at least in terms of what is shown in Fig 1. Bosilovich et al. (2002) and van der Ent et al. (2010) do not show the same quantity - they show actual recycling ratios. Numaguti (1999) shows the same quantity (fraction of precipitable water originating as evaporation from land) but only an East Asian view of this term for summer and winter, and an annual mean for the global view. Yoshimura et al. (2004) show an average for a 7-month period, not seasons. Thus, direct comparisons cannot be made.

AR6: We show "actual recycling ratios" as do the other four cited studies, see AR2. Also van der Ent et al. (2010) and Bosilovich et al. (2002) have non-zero values over the ocean but mask it there, i.e. they show values only for land precipitation. We consider this masking over the ocean rather misleading and see our impression affirmed by the referee's comment. For example, this masking curtains the fact that continental moisture is transported from one continent across the ocean to another continent, the result being that the moisture advected from the ocean already has continental recycling ratios larger than zero when entering the continent. The contribution of this effect is not negligible, in particular in northern summer for Eurasia and North America, as our Fig. 2 clearly shows. We still think that our estimates agree well with the earlier estimates, and of course we have taken the different averaging-periods into account when comparing with the other studies, but we now comment better on the comparability of the quantities (Sec. 4 last 2 paragraphs).

RC7: P3518, L11: From the perspective of atmospheric dynamics and moist thermodynamics, this is not a surprising result (see general comments above).

AR7: To facilitate the discussion, we cite the relevant sentences from our original manuscript (P3518, L8-11): "Based on traditional moisture-budget considerations one would expect that the response of the atmospheric moisture content to the suppression of continental evaporation is converse to the RMF pattern in the REF experiment (Fig. 1, left). But this is not the case (Fig. 2). Strikingly, the patterns show hardly any correlation." As cited, we introduced this assessment with the phrase "Based on traditional moisture-budget considerations", indicating that we personally did not expect that the atmospheric response closely conforms to these simple moisture-budget considerations. In other words, we also were not exactly surprised either.

RC8: P3519, L17-19: This seems to be an unsubstantiated conjecture. One must consider both the 3-D general circulation of the atmosphere (particularly convergence/divergence and lifting/subsidence) as well as other diabatic processes to substantiate causes of the vertical profiles shown. A hydrodymanic atmospheric analysis of the predictive equations for water vapor (mean, eddy and transient flows) would fully diagnose the structure and its causes.

AR8: We have excluded the vertical profiles from the new manuscript because they are not central to our argumentation. The changes in the 3D-circulation are now discussed in some detail in Sec. 5.3.

RC9: Sec 4.2, Para 1 and later in paper: The term "traditional" approach is used. I am not familiar whose tradition this is. Perhaps "simple" or "idealized" would be a better term. What we might call the Eagleson or "bulk" approaches acknowledge the important role of moisture flux convergence and does not expect moisture to behave as a linearly advected tracer or a static column budget.

AR9: Concerning the term "traditional" we agree with the referee and abandoned it from the manuscript.

RC10: P3520, L17-18: "Eurasia is not affected by North America's evaporation and vice versa, regardless of the substantial fraction of moisture they receive from each other." This is an important result that should be clarified here - precisely what is meant by "not affected"?

AR10: We elaborate on this in the new manuscript in Sec. 5.1, 5th paragraph.

RC11: P3521, L8: "Aerial runoff" is defined incorrectly. P-E approximates runoff only in the annual mean (or integer multiples of years) and not on shorter time scales like seasons because terrestrial storage (and less importantly, atmospheric storage) have significant seasonal cycles too.

AR11: We must contradict the statement that we have assumed "runoff" (i.e. "terrestrial runoff") and "P-E" to be identical on sub-annual time scales. Rather, we have assumed "aerial runoff" (i.e. moisture flux divergence) and "P-E" (accounting for the sign "E-P") to be identical on the monthly time scale. E-P approximates aerial runoff much closer than P-E approximates terrestrial runoff because the monthly change in terrestrial storage is typically considerably larger than the monthly change in precipitable water. We are not considering the terrestrial side of the water balance, so the (larger) change in terrestrial storage plays no role. However, in the new manuscript we do not use the term aerial runoff anymore.

RC12: L14-15, 20-21: This supposition assumes a time scale and spatial behavior that has not been justified in the text. Consideration of mean flows seems to be leading the authors to make some poorly substantiated conclusions about the behavior of the atmosphere. The effect of moisture on atmospheric (convective) stability cannot be ignored. P3523, L13-14 is a partial recognition of this, but in fact "condensation-induced heating" and the irrotational components of the flow that drive it are important anywhere precipitation is occurring, including the subtropics, mid-latitudes and in baroclinic systems that dominate the winter hemisphere.

AR12: To facilitate the discussion we cite the passage that contains the sentences in question (the latter highlighted with italic letters): P3521, L11-21: "Within the traditional moisture recycling framework some precipitation decrease is expected following the RMF pattern, which would alleviate the amplification of the continental moisture sink. This alleviating (compensating) effect would occur in continental downstream regions (with low RMF) more than in upstream regions (with high RMF). However, since precipitation responds strongly not only in continental downstream regions, but also in upstream regions, the precipitation decrease compensates for most of the evaporation decrease in large parts of the continents (Fig. 5, left). Paradoxically, the suppressed continental evaporation flux is often fully or even over-compensated, the latter meaning that the suppression of evaporation does not strengthen but weaken the land moisture sink." Prior to the criticised sentences we indicate explicitly that in the following the "recycling perspective" is described: "Within the traditional recycling framework . . .". We thus believe that the criticism results from a misunderstanding. However, we think that in the new manuscript we make clear that we do not ignore the effect of moisture on atmospheric stability (Sec. 2.2).

RC13: P3523, L23: "...relative humidity": this may be a clue - many moist convective parameterizations use a trigger for convection based on an arbitrary threshold value of relative humidity, or some other "IF statement" in the code. Please investigate the convective scheme in the GCM to determine how this is contributing to the response found in the model.

AR13: Please see AR4.

RC14: P3524, L15-16: This unique behavior of the Indian monsoon seems to be very common in GCMs run in a dry land mode - going all the way back to Shukla and Mintz (1982).

AR14: Yes, and in the new manuscript we are elaborating on this aspect (Sec. 2.3, Sec. 5.2).

RC15: P3525, L14-15: "VIM and the surface fluxes decrease continuously (exponentially) from the upstream coast to the downstream coast" - this conceptualization is not generally observed in mid-latitudes where baroclinic instability determines storm tracks, nor in the deep tropics where the Hadley and Walker circulations locate precipitation patterns through their connections to moisture flux convergence. It seems that

only in monsoonal subtropical regimes do we see something resembling the reference situation in the schematic in Fig 8. It seems not to be widely applicable.

AR15: We agree that the reference situation is not widely observable in reality. In general we are now more careful regarding the "explanatory power" of the simplistic picture illustrated by Fig. 8. Yet we keep the discussion of the simplistic picture because, even if it is not realistic, it demonstrates the necessity to investigate the predictive utility of recycling estimates.

RC16: Conclusions: "...moisture recycling estimates are of limited use to deduce hydrological impacts of land-cover change activities." - This experiment hits the atmosphere with a very big hammer (globally dry continents). In terms of the range of natural variability of the water cycle, recycling estimates are quite useful and there is a large body of literature on the subject. The usefulness is based on its goodness of fit (e.g., regression) to various elements of the hydrologic cycle in many parts of the world, as found in reanalyses, and other observationally-based studies. With such large perturbations as in this study, the GCM's moist dynamics and thermodynamics are adversely affected, making a rather non-Earth-like planet where the regressions based on small (natural) perturbations no longer hold.

AR16: The scale-issue is now a central aspect of our manuscript (e.g. Sec. 2.4). Yet we do not fully understand what the statement "recycling estimates are quite useful" is based on. The explanation that "The usefulness is based on its goodness of fit (e.g., regression) to various elements of the hydrologic cycle in many parts of the world, as found in reanalyses, and other observationally-based studies" does not convince us. We would really appreciate it if the referee could be more precise about studies that have shown that "recycling estimates are quite useful" (to deduce hydrological impacts of land-cover change activities), because we believe this to be a plausible but unverified conjecture. 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 (in the sense of Schär et al. (1999)). We think that it remains an open question if there is a scale (an intermediate scale?) of the perturbation at which moisture recycling estimates might be useful indicators (Sec. 2.4, 6, and 8).

RC17: "...our results question the relevance of traditional moisture recycling estimates even for continental scales – an admittedly counter-intuitive conclusion." - Again, I think with a better background in moist atmospheric dynamics and physics, the authors would come to a different conclusion. This would inform a more thorough diagnosis of the results, such as the direct effect on moisture flux convergence, the surface energy budget, and diabatic processes that are affecting the water cycle. These are not such surprising results in light of basic meteorological studies of the governing equations of the atmosphere.

AR17: Please see AR1 and the general response.

References (apart from those already contained in the (new) manuscript):

de Groen, M. M. and Savenije, H. H. G.: Do land use induced changes of evaporation affect rainfall in Southeastern Africa?, Physics and Chemistry of the Earth, 1995

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