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 manuscript presents a study in the vein of Shukla and Mintz (1982) where two GCM simulations are compared: one with land surface evaporation enabled (control) and one with it disabled. In this study, the effects on the distribution, transport and precipitation of water vapor is studied using bulk methods.

General comments:

There is much talk throughout the paper of "intuition" and results seeming "counter-intuitive". 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 co-author with a background in atmospheric science to provide this missing perspective.

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, M_r, defined as "recycled" moisture, is really terrestrial moisture (as opposed to M_o = 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.

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:

Brubaker, K. L., P. A. Dirmeyer, A. Sudradjat, B. Levy, and F. Bernal, 2001: A 36-year climatology of the evaporative sources of warm-season precipitation in the Mississippi

River Basin. J. Hydrometeor., 2, 537-557.

Dirmeyer, P. A., and K. L. Brubaker, 2007: Global characterization of the hydrologic cycle from a quasi-isentropic back-trajectory analysis of atmospheric water vapor. J. Hydrometeor. 8, 20-37.

Specific comments:

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.

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.

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.

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

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 sub-

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stantiate 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.

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.

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"?

P 3521, 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.

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.

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

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scheme in the GCM to determine how this is contributing to the response found in the model.

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).

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.

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.

"...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

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surprising results in light of basic meteorological studies of the governing equations of the atmosphere.

Technical corrections:

P3524, L19: dipol -> dipole

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

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