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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on “Biotic pump of atmospheric moisture as driver of the hydrological cycle on land” *by* A. M. Makarieva and V. G. Gorshkov

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Panel comments on biotic pump

This week a group discussion was devoted to the “Biotic pump” manuscript. A few points for discussion were raised that have not yet been introduced in the discussion so far.

First, the choice of the transects in fig 1, and the expression of de decline rate of precipitation with increasing distance to the coast in fig 2, should be consistent with the mean direction of water advection in each of the transects, in order to be meaningfully

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interpreted in terms of a moisture transportation process. This is not always obvious. From Peixoto & Oorts' (1992) classical textbook ("Physics of Climate", publ by AIP, New York, 520 pp), one can observe mean, DJF & JJA horizontal water transport (fig 12.17, page 298, 299) to be opposite to the arrows in your fig 1 for the forested transect in Africa (nr 7), and the "arid" transects in the US (nr 5) and Eastern Asia (nr 2). The flow in the Amazon (nr 6) is highly variable and in many occasions moisture intrusions originate from other sources than the Atlantic. The Yenisey transect (nr 8) is nearly perpendicular to the mean flow. The authors should explain this obvious discrepancy.

A second descriptive argument in fact against the importance of the biotic pump mechanism is that current generation General Circulation Models do not seem to need this mechanism in order to realistically model the distribution of precipitation over the continents. Also without a biotic pump mechanisms, not many models turn the central Asian continent into a desert. It would be valuable when the authors would indicate what kind of compensating mechanism apparently causes this resemblance to the true distribution of precipitation over the continents.

The third comment concerns the thermodynamic principle of the biotic pump itself. The authors state that according to Dalton's theorem all gasses in the atmosphere should independently obey the aerostatic equilibrium, as expressed by their eq C2 in their (useful!) commentary posted on 12 Dec 2006. They state that the presence of extra forces acting on particular gases (like the removal of water vapour by condensation at higher atmospheric levels) does not affect the set of forces acting on other gases, following Bolzman's law. The question is whether this is also true in a turbulent and non-static medium like the atmosphere. Water molecules at a given height experience the pressure of ALL gasses in their environment, and also the weight of all gasses above them. This is the very principle of the hydrostatic equilibrium, which applies to a balance of macroscopic forces. Moving water molecules will collapse with all other molecules when turbulent mixing is present. This would imply that condensation at higher atmospheric levels does NOT affect the weight of the column of air experienced

HESSD

3, S1700–S1702, 2006

Interactive Comment

by the water molecules in a turbulent non-static environment. As a thought experiment, one could consider a glass with two different fluids with a different molar weight. When mixing is absent a stratification will occur, separating the two fluids. When by some pumping mechanism fluid of one kind is removed somewhere high in the glass the gradient of that fluid will indeed be enhanced, en extra diffusion will take place, which represents the "evaporative force" claimed by the authors. However, when the glass is stirred and the fluid is well mixed, the time scale of this diffusive restoration of the equilibrium is much longer than the turbulent time scale. In other words: in the true atmosphere the evaporative force is present, but it is not a macroscopic force but a microscopic (diffusive) force, and this can normally safely be ignored. An exception may be a condition where steep vertical moisture gradients are accompanied by a drying force, like the large scale subsidence zones over cloudy atmospheric surface layers over subtropical oceans. But these are regions which are considered to be irrelevant regarding the evaporative force, as claimed by the authors.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2621, 2006.