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Interactive Comment

Interactive comment on “Comment on “Biotic pump of atmospheric moisture as driver of the hydrological cycle on land” by A. M. Makarieva and V. G. Gorshkov, Hydrol. Earth Syst. Sci., 11, 1013–1033, 2007” *by* A. G. C. A. Meesters et al.

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The debate between Meesters, Dolman and Bruijnzeel (MDB) and Makarieva and Gorshkov (MG) concerning the biotic pump theory and evaporative force postulated earlier by the latter authors is not concluded by the publication of the "Comments" by MDB and the "Reply" by MG. As handling editor I can appreciate many comments from both parties in this debate. Although not all points are to my opinion satisfactorily resolved the publication of this discussion embedded in the aforementioned manuscripts is desirable. It may trigger more debates that lead to an enhanced understanding of the physics that drives the planetary hydrological cycle in general, and the role of large evaporating bodies in particular.

To further fuel the ongoing debate, I have listed some personal notes emerging while reading the MDB and MG contributions. In the debate both viewpoints agree with the fact that water removed from the upper atmosphere generates a disturbance of hydrostatic equilibrium, but there is disagreement about the significance of the various compensating mechanisms that restore this equilibrium. According to MDB macroscopic motions other than the evaporative force take care of this, while MG argue that the removal of water molecules must lead to a systematic upward motion. I tend to follow the MDB argumentation that the heat release by condensation and the associated pressure increase in a parcel with condensation do disturb the hydrostatic balance, but that this balance is rapidly restored by macroscopic motions resulting from the expansion of the heated air parcel.

In the argumentation of MG no attention is paid to the role of liquid water in the atmosphere, droplets formed after condensation. The condensation process per se does not change the weight of a parcel, it is the removal of the mass that leads to this weight change. Clouds - being droplets of liquid water - remain in suspense a long time, and the weight of the liquid water maintains the hydrostatic balance, until precipitation removes the water. Saturated air can thus be in hydrostatic equilibrium - until precipitation occurs. And I cannot oversee the consequences of including the role of the vertical mass redistribution by precipitation for the hydrostatic equilibrium.

MG open their reply stating that air as a whole is in bulk hydrostatic equilibrium when all components are in hydrostatic equilibrium. While this is true, it is not a necessary requirement, as explained by MDB. Individual components can be out of hydrostatic equilibrium but as a mixture be in equilibrium by restoring (macroscopic) motions (eq 13 in MDB). It explains the fairly constant mixing ratio of various (dry) air molecules in spite of differences in molecular weight. In that sense the MG phrase that condensation does not affect the non-condensable dry air components is a bit confusing: macroscopic

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motions will lead to dry air being (slightly) out of component hydrostatic equilibrium when condensation changes the air mass overhead. The mass effect of condensation is not ignored by MDB but argued to be (1) very small compared to the heating effects of condensation, and (2) restored by macroscopic motions different from the evaporative force.

MG are right that the disappearance of water molecules high in the atmosphere does not affect the composition or mass of dry air. However, the loss of mass is compensated by a gain of mass by evaporation from the surface. The numerical value of the upward air motion w induced by this evaporation E (0.4 mm/s as claimed by MDB) is not easily derived. MG argue that w should be calculated using the typical water vapour density ρ_v as E/ρ_v, but MDB implicitly state that it is the entire atmosphere and not only the water molecules that is lifted to make place for the water molecules. Additional correspondence with MDB have revealed their calculations: for a supply of $1kg/m^2h = 55.6mol/m^2h$, the volume increase V is given by RT/p times this number, with R the universal gas constant ($8.3143J/molK = 8.3143kgm^2/molKs^2$), T the temperature (288 K) and p the pressure ($105Pa = 105kgm/s^2$). This yields 1.33 m/h or 0.48 mm/s.

Another difficulty in the attribution of the role of forests in the hydrological cycle claimed by MG is that energy limitations on evaporation are nowhere included in the debate. Evaporation requires energy, and it is the combination of the availability of moisture and energy that determines the rate of evaporation. Moisture limitation over the oceans is not an issue, in contrast to land masses including forests. Therefore land uses on average a smaller portion of the available energy for evaporation (the so-called evaporative fraction) than oceans do, the remainder being transported back to the atmosphere as sensible heat. On average, land masses do evaporate less than oceans (see e.g. Peixoto and Oort, 1992). In that environment it is very hard for forests to push up the evaporation rate at levels equivalent to surfaces with unlimited water availability, like oceans. Obviously oceanic temperature gradients, aerodynamic roughness, atmospheric recycling and seasonal cycles of energy storage in ocean water/soils do lead to marked spatial structures in the evaporative fraction, but from this energy consideration (forest) areas having larger evaporation rates than (nearby) ocean masses should form a minority, which seems inconsistent with an evolutionary principle explaining the development of forests and their role in transporting water to the land. MG rightly state that our assessment of this transport over the large Amazon basin is far from complete, which pleads for an active continuation of research in this important area.

Finally, I am glad that despite the strong disagreement between MDB, MG and a range of reviewers involved, this disagreement has not prevented to have the various viewpoints published in the open literature. There are probably many topics in the geophysical research arena that have been published with a lot less in depth discussion about the validity of the arguments. Additionally, the open discussion and the publication of ideas that may eventually prove to be wrong does help the scientific understanding of our complex hydroclimate system. The ideas of MG have triggered the imagination of many scientists, which is a strong point in itself. A physical demonstration or falsification of the evaporative force - for instance by Direct Numerical Simulation or direct observations - would be a welcome contribution to this debate.

Reference

Peixoto, JP, and AH Oort (1992): *Physics of climate*. New York, USA: American Institute of Physics. 520 pp

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 401, 2009.

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