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Hydrology and Earth System Sciences Discussions

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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With great interest I read the large number of comments both in this discussion and in the previous discussion in HESSD, when the biotic pump theory was first presented (http://www.cosis.net/members/journals/df/article.php?paper=hessd-3-2621).

In the present discussion paper (DP), Meesters et al. focus on the physical foundations of the biotic pump theory, highlighting, in their opinion, an important misinterpretation of Makarieva and Gorshkov (1997; hereafter referred to as MG) about the consequences of the observed non-equilibrium vertical distribution of atmospheric water vapor. I believe the main point of disagreement can illustrated by equation (13) of the DP, which I

repeat here:

$$-\frac{\partial p}{\partial z} - \rho g = \left(-\frac{\partial p_d}{\partial z} - \rho_d g\right) + \left(-\frac{\partial p_v}{\partial z} - \rho_v g\right)$$

Let the first term in brackets on the right hand side of the equation be called "term (I)" and the second term in brackets be "term (II)". Term (I) represents the thermodynamic equilibrium (or aerostatic equilibrium as termed in MG) of dry air components, and term (II) the thermodynamic equilibrium of water vapor. All authors agree that term (II) is not in balance (substantially different than zero) and that is caused by the large lapse rate of air temperature and condensation of water vapor. They disagree, however, on what is the consequence of this imbalance to the bulk (hydrostatic) equilibrium of moist air (the left hand side of the equation).

According to MG, the so-called evaporative force, which can be viewed as a peculiar case of osmotic force, is created by this imbalance of (II). I suppose small imbalances of the dry air components (term (I)) may also create other types of osmotic forces, but in MG's view are probably much smaller than the evaporative force.

On the other hand, Meesters et al. defend in this DP that imbalances in the bulk (hydrostatic) equilibrium - that is, the left hand side of equation R1 - are efficiently restored by macroscopic motions, so imbalances (II) will, indeed, initially create some imbalance in the bulk equilibrium, which will cause a dynamic flow. This dynamic flow, however, will disturb the equilibrium for the dry-air component (term (I)) and efficiently restore the hydrostatic balance of the mixture, and therefore, will not be sustained by the imbalance of term (II) for long.

I must confess I was enthusiastic with the reasoning presented by MG, and I'm convinced that the natural biota critically influences the environment, perhaps with mechanisms such as the proposed biotic pump of moisture. However, as educated within the classical school, I tend to agree with the DP authors that it is unlikely that the

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imbalance of one component (water vapor), even under high rates of condensation, causes a sustained dynamical imbalance of the whole mixture.

For that reason, I recommend that the paper is published with a minor revision in the following: the authors presented, in the discussions (S44), new comments on the thought experiment proposed in the DP better clarified the effect of an imbalance of water vapor pressure (p_v) causing also a compensating imbalance in the partial pressure of the dry air components (p_d) while the whole mixture restores mechanical equilibrium. I recommend that these new comments are incorporated in the published version of the paper.

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