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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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This is a Response to remarks made in “Evaporative force as peculiar osmosis” (S11-S16). Points that have been addressed already in our first Response (S33-S41) are not repeated here.

In S11 the central statement of the DP is not well rendered: the DP did not state that disequilibrium of one component cannot bring about a pressure gradient over the mixture as a whole. It was merely stated that any resulting pressure gradients are generally much smaller than the ones predicted by calculations (like those in MG 2007) that neglect the restoring effect of macroscopic flow. The latter tends to strongly reduce

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the pressure gradients, as explained on pages 409-410 of the DP.

Concerning the remarks (S12, first half) about Eq. (11) in the DP: We do not deny that, if there is a pressure gradient acting upon the mixed air as a whole, then molecular diffusion is only a minor process superimposed upon the macroscopic flow. This is a key statement of the DP. As for the last sentence of the same paragraph (S12, first half): the role of Eq. (11) in our argument should not be exaggerated; the central argument of the DP is that disequilibrium for one component of the air mixture generally brings about a much smaller bulk-disequilibrium, if any (see previous paragraph). This argument does not involve Eq. (11).

The reference to osmotic devices (S12, 2^{nd} half, and S13) is confusing. Such devices involve membranes which inhibit macroscopic flow, and create the possibility of major jumps in pressure. The argument of MG (2007) indeed shows some similarity with the description of osmotic processes, with water vapor and dry air in the role of the water and salt component, respectively. However, the difficulty is that in the atmosphere, such membranes (or their equivalent) are missing. Condensation cannot play that role, since it does nothing to prevent restoring motions and these motions involve the dry air component as well. It would seem that in the reasoning of MG (2007) the dubious principle that “components independently come into and out of equilibrium”, has replaced the role of the membrane. By the same principle, the dry air component is treated as immobile. We will not comment further on the analogy used by MG since it seems too far removed from the factors that are at work in the real atmosphere, where flow of the entire mixture is dominant (except for precipitation which does not fit into the osmotic scheme. We note also that in advancing the Biotic Pump Theory, MG (2007) dedicated only one line to the analogy with osmosis (and within brackets at that; p. 1022 bottom left).

The central paragraph of S13 has been answered already in our first Response, S34-S35.

Concerning the remarks in S14, first half: We admit that the title of the section “Traditional theory of evaporation” is not well chosen, as the section only contains an analysis from the viewpoint of the subject of the DP.

As for the conclusion of the first paragraph of S14: “Evaporation is only present in the presence of the evaporative force”: We agree that evaporation requires undersaturation of the air in general and, to some extent, flow, but classical theory is quite capable of explaining the origin of these factors, without the need for recourse to the so-called evaporative force.

Concerning the comment on our thought experiment (S14, 2^{nd} half): it is important to distinguish between the controlled experiment and what happens in open air. In the experiment, the rapid removal of the lid can bring about an audible sound, so strong is the acceleration caused by the jump in air pressure (dozens of mbar if the air was dry originally). This will last only for a fraction of a second, as is known from experience with a bottle whose contents are subject to over-pressure. This is the process of which we say that “mechanical equilibrium will be restored soon”. It is not essential to the description of the release process to consider what happens afterwards to the vapor once it has been released into the open air. The experiment served merely to demonstrate that the straightforward translation of the partial pressure difference of the water vapor, to air pressure difference as such, is only valid as long as the mechanical relaxation (in this case, expansion) is “switched off”. With relaxation, the air pressure difference disappears (common experience!) whereas the difference in vapor concentration and hence partial pressure p_v remains for the moment. Simultaneously, the expansion should cause a decrease in the partial pressure p_d of the dry air component. Since the restoration of mechanical equilibrium implies that the jump in the pressure p disappears, the equation $p = p_d + p_v$ dictates that after expansion, the deviations in p_d and p_v (with respect to the background air) have to compensate each other. This, in turn, implies a violation of MG (2007) their so-called fundamental principle that “components independently come into and out of equilibrium”. Hopefully the present explanation of the thought experiment and its purpose is clearer than the formulation used in the DP.

The statements in the first paragraph of S15 concern several points that have been addressed already in our first Response (S33-S41).

Second paragraph of S15: This is an interesting question! We leave it to others to address this as it is too removed from the subjects discussed by the DP.

Concerning the first conclusion (S15, section 3): The DP states that component disequilibrium does produce air motions, namely to the extent that it does produce mechanic (bulk-) disequilibrium. However, we do maintain that straightforward translation between the two, by assuming that “components independently come into and out of equilibrium”, is impossible. Application of this principle neglects the feedback related to the relaxing motion of the dry air component, and predicts mechanical forces and accelerations which are often too large by several orders of magnitude.

The second conclusion (S15, section 3) has been already commented on in our first Response (S39).

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