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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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1. Evaporative force as a peculiar case of osmosis

In this commentary we would like to demonstrate in further detail that the central statement of the discussion paper (DP) is incorrect, namely that a disequilibrium distribution of partial pressure of one of the mixture components cannot lead to a mechanical disequilibrium of the mixture as a whole.

To prove that statement, the DP authors use their equation (11) with a reference to

Einstein, Landau and Lifshitz and others. The DP authors appear to have ignored that Eq. (11) is derived and only valid for the case when, from the very beginning, there is no pressure gradient force applied to the mixture as a whole, i.e. for a static medium. For example, an equation similar to Eq. (11) would describe diffusion of ions in a liquid solution to which an electric field is applied. The resulting non-uniform concentration gradient of ions will appear as an interplay between molecular diffusion and the electric force. That this equation is not valid for mixtures under external pressure gradient becomes immediately clear when writing it for a "mixture" consisting of one gas only. In the absence of equality between pressure gradient and weight, dynamic (not diffusional!) fluxes will appear restoring the mechanical equilibrium. Therefore, the logic of using Eq. (11) by the DP authors is corrupt: the equation is **originally** written for the case of mechanical equilibrium (when there is no external pressure gradient); in this case, says the equation, the only process counteracting the force acting on ***some*** components of the mixture will be molecular diffusion. Derived as such, the equation **cannot in principle be used** to prove the statement that a non-equilibrium concentration gradient of one of the components cannot bring about a pressure gradient acting on the mixture as a whole.

There is a huge domain of empirical evidence ignored by the DP authors that documents how component-disequilibrium creates pressure gradient and dynamic flow in the mixture as a whole. We mean the phenomenon of osmosis. It consists in the fact that, in agreement with Dalton's law, partial pressures of particular components of gas mixtures or liquid solutions tend to spatial homogeneity independently of each other. Consider two mixtures with different concentrations of various components that are separated by a semipermeable membrane, which impedes spatial propagation of one of the components and prevents it from reaching the equilibrium distribution. The resulting equilibrium distribution of partial pressures of all other components will be associated with a pressure gradient across the membrane. The trans-membrane pressure difference will be equal to the magnitude of deviation of the partial pressure of the considered non-equilibrium component from equilibrum. If now the membrane is removed, the **dynamic** fluxes of liquid or gas will follow governed by this pressure gradient until the mixture pressures and concentrations of all constituents in the two areas equate. Osmosis penetrates all aspects of life's existence, being responsible for the maintenance of intracellular pressure, etc. Since the spatial pressure difference induced by the osmotic processes can be enormous (e.g., contact of seawater and freshwater results in a pressure difference of about 30 atmospheres, e.g., Makarieva et al., 2008), there are even plans to use this effect for power generation (e.g., Gerstandt et al., 2008) using the natural fresh water – seawater interface in the mouths of large rivers. All these effects are solely possible due to the fact that the spatial homogeneity of different mixture components restores **independently of each other** so that partial pressures of particular components equate.

The statement of the DP authors that component-disequilibrium "is not to be thought of in mechanical terms (such as partial pressures being in balance with the weights of the respective components)" reveals a fundamental misunderstanding by the DP authors of the physics of gas mixtures and liquid solutions.

The physics of the evaporative-condensational force can be interpreted and understood as a peculiar, previously undescribed case of osmosis. In the atmosphere, the role of semipermeable membrane of a unique nature is played by the vertical temperature gradient – it selectively removes, via condensation, one of the gases from the mixture (water vapor) and does not allow it to propagate to the upper colder atmosphere in quantities sufficient for the restoration of component equilibrium of water vapor in the gravitational field. At the same time, lacking material essence, this unusual "membrane", unlike the conventional osmotic membrane, is penetrable to the dynamic flow of mixture as a whole, sustaining continuous air circulation. In the ordinary osmosis, the dynamic flow should be intermitted by periods of molecular diffusion via the semipermeable membrane, when the osmoitc pressure difference is restored. In the case of the evaporative force, the dynamic flow itself sustains the "osmotic" pressure difference by bringig water vapor to the area of condensation. All these processes were ignored

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by the DP authors.

Below we make a few more comments on the DP.

2. "Traditional" theory of evaporation

In our opinion, this section of the discussion paper should be substantially re-written towards a greater clarity and specificity. As presented, the traditional theory of evaporation comes as undeservingly too modest, i.e. completely lacking any quantitative theoretical background. Not a single equation is written, not a single work on the subject is cited. The only estimate of vertical velocity presumably related by MDB to evaporation comes without any explanation. The authors could have at least mentioned the Penman-Monteith equation for evaporation, which explicitly relates evaporation to the presence of large-scale atmospheric motions (winds). Generally, in a still atmosphere in hydrostatic equilibrium there will be no evaporation at all. Surface air would become saturated with water vapor, that's all. In order that evaporation continues, the process of condensation that occurs in the upper atmosphere and initiates the upwelling motion of surface moisture-rich air is necessary. Hence, evaporation is only present in the presence of the evaporative force.

We would also like to dwell on the thought experiment proposed by the DP authors in Section 2.2. Closed container with dry air is filled with "a realistic distribution of water vapor", so that pressure in the container becomes higher than the pressure above the container. Then the lid is removed and moist air from the container will initially start accelerating upward with a maximum acceleration described by Eq. (16) of the DP. The DP authors conclude that "this will be a transient phenomenon as mechanical equilibrium is soon restored." This is incorrect. Again, as anywhere else in the discussion paper, the DP authors have been successful in completely avoiding to mention the vertical temperature gradient, despite the critical importance of the latter for the biotic pump theory. The upper atmosphere above the container is significantly colder than within the container. So, as soon as moist air parcels from the container reach the

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upper colder layers, water vapor will condense. This condensation will counteract the anticipated "restoration of mechanical equilibrium".

In the real atmosphere, where "strong vertical mixing" to which the DP authors repeatedly referred, is normally present, air is mixed not by molecular, but by the much more intense eddy diffusion. Effectively, eddy diffusion takes macroscopic parcels of moist air from the surface and throws them upward to the colder atmospheric layers. The associated condensation of water vapor as the air parcel cools maintains the pressure shortage that sustains dynamic air flow and the eddy diffusion itself. In the result, mechanical equilibrium is never restored in the atmosphere. Importantly, as already noted in our previous comment, namely the absence of this mechanical equilibrium is responsible for the observed constancy of composition of dry (but not moist!) air.

An interesting thought experiment might be to consider an atmosphere consisting of water vapor only. In such an atmosphere water vapor, compressed by the temperature gradient sixfold compared to its equilibrium distribution, would be always very far from equilibrium. This should, on the one hand, initiate severe circulation events like hurricanes and tornadoes. At the same time, the cumulative power of such events cannot be larger than the (relatively small) power of solar radiation spent on evaporation of water vapor. How would the problem resolve? This would be a good thought-provoking question to fresh-minded students, the one inviting to think creatively of the physical processes behind the evaporative force and atmospheric circulation on Earth.

3. Conclusion

In conclusion, as we argued in the two short comments that opened this discussion, the two main objections of MDB against the biotic pump theory, namely that (1) component-specific disequilibrium (for water vapor) does not produce air motions and (2) the evaporative force physics should produce unrealistically high velocities, are incorrect. The first one is based on the unjustified neglect of the fundamental difference in atmospheric physics of condensable and non-condensable air components. The second

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one is (presumably) based on the neglect of the continuity equation for atmospheric circulation which distributes the driving pressure difference along the entire streamline and on the neglect of friction forces that are responsible for low air velocities in the stationary large-scale circulation.

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