

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

**S. Sherman**

sherman@pnpi.spb.ru

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I would like to comment and actually have two questions on the physical mechanism of the biotic pump proposed by the authors.

From the physicist's perspective I am convinced by the authors' reasoning that atmospheric water vapor cannot be in hydrostatic equilibrium above the open water surface or moist soil surface if the vertical temperature lapse rate  $\Gamma$  exceeds the threshold value  $\Gamma_{H_2O} = 1.2 \text{ K km}^{-1}$  (p. 2636, eq. (13)), as is the case in the Earth's atmosphere. (I find it remarkable that, to my knowledge,

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this apparently important phenomenon is never mentioned in the basic physical texts where atmospheric gases are discussed. One can always read about such things as convective instability and adiabatic lapse rate  $\Gamma_a$  (e.g., Landau, LD, Lifshitz, EM, Course of Theoretical Physics, Volume 6, Fluid Mechanics), but nothing about the non-equilibrium water vapor and  $\Gamma_{\text{H}_2\text{O}}$ .)

So at  $\Gamma > \Gamma_{\text{H}_2\text{O}}$  water vapor is saturated and cannot be in hydrostatic equilibrium – when water vapor pressure at any height is balanced by the weight of the water vapor above that height. This is obvious if one considers water vapor alone or a hypothetical case of an atmosphere consisting of water vapor only. Indeed, water vapor concentration cannot exceed the saturated one. Saturated concentration exponentially diminishes with decreasing temperature in accordance with the physical law of Clapeyron-Clausius. When temperature is dropping with height sufficiently quickly, less and less water vapor can be present at a given height. So starting from some threshold lapse rate the total weight of water vapor in the atmospheric column above a given height becomes less than local vapor pressure. This leads to the origin of an upward-directed force termed “evaporative force” by the authors. In my view, this term is indeed justified for the terrestrial conditions which correspond to the existence of liquid hydrosphere (open water surface of the oceans and moist soil surface), where water vapor is always saturated immediately near the surface. In this case evaporation of moisture from the Earth’s surface compensates precipitation of condensed liquid water back – to the surface. The power of evaporation must be maintained by solar energy.

In reality water vapor is mixed with other air constituents. Consider moist air, which is originally in hydrostatic equilibrium, where water vapor starts to condense somewhere in the atmospheric column and is leaving the atmosphere in the form of precipitation. In such a case in that volume where moisture condensed and which, prior to condensation, was in hydrostatic equilibrium, the atmospheric air becomes rarified and its pressure drops. As far as I understand, the appearing surplus of the vertical air

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pressure gradient, uncompensated by the weight of atmospheric column, creates the evaporative force. This force drives moist air into that area, i.e. it leads to formation of atmospheric circulation, as described by the authors.

(1) However, it remains unclear what does hydrostatic equilibrium mean when applied to the real moist air. It is well-known that the equilibrium Boltzmann's distribution for individual gases with different molar masses should be manifested as different scale heights for the vertical distributions of the gases. So in equilibrium concentration of each gas drops exponentially with height with each its own  $H$ -folding length determined by its molar mass. This does not conform with the observations – the relative ratio of different air constituents practically does not change with height in the troposphere, as noted also by the authors (p. 2534). I think it would be productive if the authors explained in greater details how the vertical constancy of relative concentration of air constituents (constant “mixing ratio” in the meteorological literature) enters the new picture that they propose.

(2) The second concern is that when moisture undergoes condensation at a given height, it, as far as I understand, mostly produces cloudiness which remains in the atmosphere and does not precipitate. The weight of this cloudiness is – obviously – exactly equal to the weight of the water vapor that condensed and left the gaseous phase. The gravitational pressure exerted by cloudiness on the atmospheric column below could possibly compensate the excessive water vapor pressure that appears due to the non-equilibrium state of atmospheric water vapor. In other words, will the evaporative force still form and the fluxes of moist air arise in the case of cloud formation in the absence of precipitation?

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