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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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We find the commentary made by Dr. Meesters, Dr. Dolman and Dr. Bruijnzeel (hereafter MDB) to be most instructive and illustrative. Although, as we argue below, the critique of our results (Makarieva and Gorshkov 2007, hereafter MG) is based on several key physical misunderstandings of atmospheric processes, we believe that publishing this commentary in HESS would be of substantial methodological value, as such misunderstandings, expressed by well-positioned scientists, can be justifiably characterized as widespread. Publication of this commentary will further contribute to the widening discussion of the biotic pump theory and its implications (Chown and Gaston, 2008; Makarieva, Gorshkov and Li, 2008; Sheil and Muridyaso, 2009), the importance of which is not disputed by anybody, including MDB.

1. "Bulk-equilibrium", mixing and dry air constant composition

The general line of the critique does not appear to be logically verified. This pertains to the central issue of "bulk-equilibrium" discussed by the authors. ("Bulk-equilibrium" of MDB refers to hydrostatic equilibrium of gas mixture as a whole and corresponds to hydrostatic equilibrium of MG (Eq. 1 of MDB, Eq. 8 of MG). "Component-equilibrium" of MDB corresponds to aerostatic equilibrium (Boltzmann's distribution) of the considered mixture component. When all air components are in aerostatic equilibrium, air as a whole is in hydrostatic equilibrium.)

It is useful to note from the very beginning that applying the notion of hydrostatic equilibrium to atmosphere as a whole or to atmosphere on a very large (continental or oceanic) scale is trivial. Obviously, on a global (or very large-scale) average the atmosphere is in **exact** hydrostatic equilibrium, which means that the atmosphere as a whole does not move either upward or downward, but remains where it is. Being in equilibrium on a grand scale, the atmosphere could be still and motionless on a smaller scale, too, for most periods of times (ubiquitous **presence** of hydrostatic equilibrium) or, alternatively, the atmosphere could be in the state of continuous circulation with local updrafts and downdrafts commonly present (ubiquitous **absence** of hydrostatic equilibrium).

The authors do not appear to have presented a clear picture of how, in their opinion, the atmosphere behaves. For example, it is said in the concluding paragraph of Section 2.1 that "it is well known (Wallace and Hobbs (1977); Holton (1979); Dutton (1986); and many others) that air is usually in hydrostatic equilibrium, to very good approximation, **except when local phenomena such as up- and down-drafts occur.**" At the same time, later in Section 2.2 it is stated that "For example, contrary to prediction of Eq. (8), the observed dry-air composition is constant in the troposphere as a consequence **of the strong vertical mixing induced by upward and downward motions.**" The two

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phrases are contradictory, as soon as one recalls that upward and downward motions can only arise when there is an uncompensated pressure gradient force (and, hence, no equilibrium) that makes air move in the vertical direction. As noted above, either air is commonly in hydrostatic equilibrium and then there is no upward motions to impose a "strong vertical mixing" or there **are** upward and downward motions ubiquitous and important to the degree that they "strongly" mix the dry air up to a uniform composition. But then there is no hydrostatic equilibrium for air to commonly find itself in.

The word "approximate" ("approximate equilibrium") lacks a quantitative meaning and none is assigned to it by MDB. For example, even if one assumes dry air to be in equilibrium (Eq. 12 of MDB) and water vapor not, due to the low water vapor content in the atmosphere the deviation of moist air from the "bulk-equilibrium" would still be negligible, not exceeding a few per cent at maximum (see also Section 3 below). Until the authors specify how exact is the presumed "well known hydrostatic equilibrium" where air "usually" finds itself, the discussion of this notion in the DP remains unsubstantiated.

MDB's statement that the aerostatic equilibrium (or "component-equilibrium" in their terms) "is not to be thought of in mechanical terms (such as partial pressures being in balance with the weights of the respective components)" is incorrect. Aerostatic ("component") equilibrium is direct consequence of Dalton's law and static equilibrium of all gases in the gravitational field of Earth, when partial pressure of each gas at height z is equal to the cumulative weight of this gas above that height. If one takes two containers with different gases, each will be distributed according to its Boltzmann's distribution. If one removes the partition between the containers and let the gases mix, the vertical distributions of the two components will be preserved, as Dalton's law prescribes that each gas fills the entire volume and its partial pressure remains such as if this gas were alone.

2. Different disequilibria for condensables and non-condensables

Devoting much time to the discussion of equilibrium and disequilibrium for specific air components, the authors fail to mention that in the atmosphere one observes two fun-

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damentally different disequilibria that apply to condensable and non-condensable air components. Here lies the first of the two critical misunderstandings of the atmospheric processes by MDB. Let us discuss it in greater detail.

Consider the non-condensable components first. MDB agree that the principle when each component of the air is distributed along its own scale height $h_i = RT/(M_i g)$, where M_i is molar mass of the i-th component, works in the absence of macroscopic flows. In "the open-air conditions macroscopic flows are so dominant" (MDB) that the component-equilibrium becomes of marginal importance. This, according to MDB, results in the constant composition of dry air. This means that, for example, both nitrogen and oxygen have a single scale height h corresponding to a weighted mean molecular mass of air $M = 29$ g mol^{-1}, which gives $h = 8.4$ km for the global mean surface temperature of 288 K (15 deg Celsius). In the meantime the component-equilibrium height for nitrogen is $h_N = 8.7$ km and for oxygen it is $h_O = 7.6$ km. Thus, in the atmosphere nitrogen and oxygen are in component-disequilibrium: nitrogen is "compressed" from 8.7 km to 8.4 km, while oxygen is, on the contrary, "stretched" from 7.6 km to 8.4 km. The other dry air components also follow the same common scale height "forgetting" the values of their own molecular masses, hence the constant dry air mixing ratio.

It is pointed out in Section 2.1 of the DP, Eq. (11), that the process that could restore component-equilibrium is molecular diffusion and that this process is slow. The authors are incorrect in that it is **molecular** diffusion. As is well-known, non-equilibrium distributions of air components in the open-air ("where macroscopic flows are so dominant"!) are being restored by eddy, not molecular, diffusion. To illustrate this obvious point, we refer the reader to the well-known studies of carbon dioxide fluxes made in tall towers above the forest canopy (the LBA experiment in the Amazon). This example is also useful as the present authors should have some expertise in that area. When, due to the nocturnal alternation between the photsynthesis and dark respiration processes, CO_2 is accumulated in the forest canopy, scientists use the eddy-covariance method to evaluate the eddy diffusion flux of carbon dioxide which restores the component-disequilibrium distribution of this biologically important air component. No one would

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ever try to apply molecular diffusion framework to estimate diffusional fluxes of air components in the atmosphere.

The authors can be additionally advised to consult meteorological literature on the point of how the vertical flux of latent heat is calculated using the component-disequilibrium concentration gradient of water vapor and the eddy diffusion coefficient. Our prediction is that upon re-evaluating this issue MDB will not further insist on the bizarre statement that "component-disequilibrium has so little effect" that "it is barely considered in atmospheric science as a causative factor", not to come in conflict with their own published studies. Rather, component-disequilibrium brings about eddy diffusion fluxes of air components, the importance of which is widely recognized in atmospheric physics.

Unlike MDB, MG devoted much time to the quantitative consideration of eddy diffusion fluxes that work to restore component-equilibrium for oxygen and nitrogen in the HESSD discussion, see Authors Comment HESSD 3: S1176, Section 1.2. "The evaporative force and constant mixing ratio of dry air"). Indeed, it appears that even eddy diffusion is not intense enough to restore component-equilibrium for dry air constituents. Vertical dynamic flows appear to be the strongest ruling factor, which dictate how dry air components should be distributed along the vertical axis.

This is a major objection of MDB to the biotic pump theory. MDB state: "deviations of component-equilibrium cannot cause restoring motions, only diffusive fluxes which are very weak (Eq. (11)). Because component-disequilibrium has so little effect, it is barely considered in atmospheric science as a causative factor, except in relation to interface-processes which always act on the micro-scale (e.g. evaporation at a surface, cloud microphysics)."

We have already pointed out that the diffusion fluxes are not weak. Further on, the "diffusion" logic, while being (generally) true for non-condensable air components, flaws radically when applied to the condensable water vapor. Indeed, water vapor has a component-equilibrium height of $h_v = 13.4$ km. MDB "agree with M&G that observed p_v-profiles are usually compressed vertically with respect to the equilibrium profile", but they do not inform the reader on **how** the water vapor profiles are compressed. Given the strong vertical updrafts and downdrafts that rule in the atmosphere and, treating all gases equally, make them follow the single common scale height of 8.4 km, one could expect water vapor to follow the same distribution and to get compressed from 13.5 km to 8.4 km, similar to nitrogen that is compressed from 8.7 km to 8.4 km.

Instead, one finds that the characteristic scale height of water vapor is **only two km!** How so? How can vertical mixing, which treats all gases equally, or (molecular or eddy) diffusion that was judged by MDB to be negligible at all, make water vapor to follow its own, highly non-equilibrium distribution, which is strikingly different from both its own component-equilibrium distribution as well as from the common distribution of dry air components? This fact (fundamental for the biotic pump theory and entirely neglected by MDB in their critique) indicates that there is a physical process, which (1) applies to water vapor only and **(2) is much stronger than vertical mixing**, as it creates a distinct distribution for water vapor despite the atmospheric mixing tries to mix all gases equally. We note in passing that, not discussing this fact, MDB equally failed to note that the biotic pump theory quantitatively predicts the observed scale height of water vapor distribution.

Now, what is this process? This process is the process of water vapor condensation, which implies mass non-conservation of one of the gaseous components of the mixture. Unlike the non-condensable gases, which relatively slowly tend (diffuse) in the atmosphere to their component-equilibrium without changing their phase volume, propagation of water vapor as it tends to component-equilibrium and rises towards 13.4 km is accompanied by phase transition, because the upper atmosphere is significantly colder. As water vapor condenses, their appears a local pressure shortage which creates dynamic air flows. (Namely these flows are ultimately responsible for the vertical mixing of the atmosphere that creates a uniform dry air composition.) Therefore, in the presence of a sufficiently large vertical lapse rate of air temperature, water vapor does not diffuse. It undergoes phase transitions, initiates dynamic air flows and propagates with the dynamic air flows initiated by condensation. Not making the distinction

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between the molecular and eddy diffusion, condensable and non-condensable air components in their critique and neglecting the different nature of component-disequilibria for condensable and non-condensable components led MDB to state that "component-disequilibrium has so little effect". This is incorrect. Component-disequilibrium of water vapor brings about dynamic air flows, which further serves to sustain eddy diffusion fluxes of all non-equilibrium air components.

3. Velocities produced by the evaporative force

The second conspicuous point of MDB's critique appears to be the statement that the physical mechanism behind the biotic pump predicts very high velocities in the order of $50\ \mathrm{m\,s^{-1}}$ "above any evaporating surface! Naturally, this raises serious questions about the physical realism of the M&G analysis." (Section 3.2 in the DP).

We do not know what precisely led MDB to conclude that such velocities should be produced by the evaporative force above any evaporating surface. In MG (2007) it was clearly stated that for a large-scale stationary circulation (as the one in the forest-covered large river basins like Amazon or Congo) the resulting vertical velocities will be in the order of several mm per second. From the statement of MDB (last paragraph in Section 3.2) that "it would seem that when the estimated vertical velocity is translated to the speed of the horizontal converging currents, wind speeds far in excess of observed values would be obtained" (the second misunderstanding of MDB) it appears that how such a translation is routinely made in the equations of hydrodynamics is not appreciated by MDB in sufficient detail. Let us dwell on this in greater detail.

Any closed circulation of air obeys the continuity equation, which in its simplest integral form reads as $wL = uh$. Here w and u are the vertical and horizontal velocities, respectively, h and L are the vertical and horizontal linear dimensions of the circulation (atmospheric height and circulation length). MG (2007) calculated the magnitude of the evaporative force and of the corresponding non-equilibrium pressure deficit $\Delta p \sim p_v$, where p_v is partial pressure of atmospheric water vapor. They then estimated ***maximum velocities*** that can be produced by this force when it accelerates an air volume along the entire atmospheric height h. To make a hopeful comparison, they calculated the drag force of a steam engine and calculated the maximum speed with which the engine can drag ***one carriage***.

At Mach numbers $u/c < 1$, where c is sound velocity, atmospheric air has the property of continuity. Hence, if one "pulls" air upward somewhere, there appears pressure shortage near the surface and there will originate a horizontal inflow of air into the region of ascent. To continue the analogy with the steam engine which now carries a long train of many carriages, when the steam engine starts ascending a hill and some carriages move already in the upward direction following the engine, the rest of the train can still follow the horizontal streamline and move horizontally towards the hill. This illustrates the obvious point that in the continuous medium like air the uncompensated pressure gradient force is distributed along the whole streamline (like the steam engine drags ***all carriages*** and not only the one immediately attached to it). For this reason, when the horizontal dimension of circulation is much larger than the vertical dimension, the highest velocity will be observed along the horizontal part of the streamline, $u \gg w$. Thus, maximum velocities calculated from the evaporative force magnitude are observed in hurricanes with $L \sim 10^2\text{km} \gg h$ in the horizontal plane. In tornadoes where the two dimensions are of one and the same order of magnitude, vertical velocities reach their absolute maxima.

In the stationary large-scale circulation the evaporative force has to drag air along a very large distance of the order of several thousand kilometers. The cumulative friction force (which, at relatively low velocities, is simply proportional to distance) becomes comparable to the evaporative force itself. In the result, initiated by the same evaporative force, air motions are much slower than those within the hurricanes, both in the vertical and horizontal dimensions. Again continuing the analogy with the steam engine, the same engine drags a very, very long train at a much smaller velocity than it would drag one carriage. For a detailed quantitative estimates of these basic circulation principles the reader can be referred to a recent discussion of (Makarieva, Gorshkov, Li, 2008), see Authors Comment (S8904) "Condensation as Air Circulation

Driver". One of the present authors (Dr. Meesters) took part in that discussion, but apparently did not pay attention to that comment, although it had answered the present concerns of MDB well before they were expressed in the DP.

Importantly, when most part of the pressure difference responsible for the evaporative force is distributed along the horizontal part of the streamline, only a small part of this difference falls on to the vertical dimension. In the result, the observed vertical distribution of moist air within the large-scale circulation pattern will be indistinguishingly close to "bulk-equilibrium". The degree of disequilibrium will be in the order of $(p_v/p)h/L < 10^{-4}$, where p is atmospheric pressure. Only in tornadoes with $h \sim L$ the deviation from hydrostatic equilibrium will be maximum, in the order of a few per cent as noted earlier in this comment. Thus, the biotic pump theory can quantitatively specify the degree of local disequilibrium for circulation events of different spatial scale.

It is also relevant to note here that the magnitude of the disequilibrium pressure difference associated with the evaporative force, $\Delta p \sim p_v$, which is of the order of a few dozens millibar, is indeed the characteristic observed pressure difference featured by both large scale circulations like huge cyclones as well as by spatially more concentrated circulation events like hurricanes and tornadoes. From his/her own daily weather observations everybody knows that the magnitude of atmospheric pressure oscillations (linked to atmospheric highs and lows) is, depending on latitude, in the order of several dozens millibars. This is precisely the characteristic magnitude of atmospheric water vapor partial pressure p_v and the magnitude of the non-equilibrium pressure shortage $\Delta p \sim p_v$, which arises when a significant part of water vapor in the atmospheric column undergoes condensation. The biotic pump theory signals that this is not a random coincidence, but a direct manifestation of the pervasive importance of the evaporative force for all atmospheric circulation phenomena.

The non-trivial nature of the stationary large-scale circulation, such as the biotic pump of large forested river-basins, becomes evident from the following consideration. Condensation of water vapor accumulated in the atmosphere can occur at an arbitrarily

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high rate. In contrast, the rate of water vapor accumulation in the atmosphere is limited by the incoming flux of solar radiation. To give an analogy, biomass in the biosphere can be destroyed (e.g., deforestation) at an arbitrarily high rate depending on the number of humans or other heterotrophs. In contrast, accumulation (synthesis) of biomass is, again, limited by the incoming flux of solar radiation and photosynthesis efficiency. How to correlate the two physically distinct processes, to achieve a stable state without extreme fluctuations (either floods or droughts)? This complex task was achieved by natural forests that went through millions of years of biological evolution to invent regulatory facilities to protect their own ecological community against water supply extremes via control of evaporation and condensation above the forest canopy. The landmasses ceased to be a lifeless desert only after natural forests and the biotic pump evolved.

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