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Interactive Comment

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

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In response to the illustrative and useful comments made during a group discussion as presented by Dr. van den Hurk:

1) The nature of the evaporative force

We start with the third comment, which concerns the nature of the evaporative force. It is stated in the comment that *"condensation at higher atmospheric layers does NOT affect the weight of the column of air experienced by water molecules in a turbulent non-static environment."* As a thought experiment, it is suggested to consider a glass filled with a mixture of two fluids with different molar masses, where in the absence of mixing a gravitational stratification occurs separating the two fluids. It is further stated that if by some mechanism fluid of one kind is removed somewhere high in the glass, the gradient of that fluid will be enhanced and *"an extra diffusion will take place, which represents the "evaporative force" claimed by the authors"*.

Briefly, in this interpretation two distinct physical phenomena, diffusion and dynamic force, are confused. An additional source of misunderstanding is the usage of the ambiguous term "fluid", which can stand for either liquid or gas. This problem has been already discussed in the authors' previous comment on the non-equilibrium of moist atmosphere, where the applications of the term equilibrium, *hydrostatic* versus *aerostatic*, to liquids and gases, respectively, were considered.

Indeed, the suggested thought experiment will look principally different if made with a liquid or a gas. Consider first the simplest example — one liquid (e.g. water) occupying some volume in a glass. Since water is practically non-compressible, if some macroscopic volume of water is removed from the upper part of the glass, one will not notice any macroscopic motion, despite the fact that the pressure of the water column will change appreciably. The balance between the inter-molecular forces and macroscopic pressure of the water column will reestablish instantaneously without any motion of the liquid.

Now consider a gas filling the same (closed) glass. If one instantaneously removes the gas from some macroscopic volume in the upper part of the glass, the pressure in this part of the glass drops, and the gas from the lower part of the glass will flow upward. One will observe a macroscopic (dynamic) motion of the gas, i.e. a wind. The time scale of this motion will be determined by the velocity of gas molecules and the linear size of the glass. In this sense gas molecules in the lower part of the glass will immediately **be affected** by the pressure drop in the upper part of the glass. It is important to emphasize that this very rapid dynamic process has nothing to do with molecular diffusion of the gas. The latter occurs *in the absence of pressure gradients* and indeed has a very large time scale.

Now returning to the thought experiment with a mixture of *two* fluids. As is clear from the above example with water, whether one removes one kind of liquid or both liquids from the upper part of the glass, nothing will happen in terms of macroscopic motion. However, even in this case, it is not correct that the water molecules are not affected by what is going on in the upper part of the glass. They are affected in that sense that the higher the weight of the water column, the larger the inter-molecular forces that counteract and balance this gravitational pressure. Liquid pressure, unlike the gas pressure, is **not** a function of concentration and temperature and can take different values depending on external pressure imposed on the liquid.

In sharp contrast with this situation, if one gas is removed from the upper part of the glass (filled with a mixture of two gases) and the pressure in the upper part of the glass drops, there will first appear a vertical wind (not diffusion!) working to restore the equilibrium pressure in the glass. Only after the equilibrium pressure in the glass is restored by this wind and the dynamic motion is extinguished, the molecular diffusion processes can be manifested as more slowly equating the concentrations of the two gases. However, if one is **continuously** removing one gas from the upper part of the glass, the vertical wind will be blowing **continuously** as well, as long as there remains gas in the glass.

Namely this process is occurring in the atmosphere at $\Gamma > 1.2$ K/km. Here the role of gas removal is played by water vapor condensation. As repeatedly noted in the paper and the authors' comments, the **stationary mean** velocity of the dynamical upward movement of moist air (not diffusion!) under the action of the evaporative force is determined by the rate of evaporation, i.e. by the rate of water vapor input into the atmosphere. In the absence of evaporation from the surface the dynamic upward movement of moist air will continue until all water vapor is removed from the atmosphere through condensation. Thus at $\Gamma > 1.2$ K/km and in the absence of evaporation the stationary concentration of water vapor in the atmosphere is zero.

In a glass with water, turbulent mixing can be ensured if one takes a tea spoon and stirs the liquid. In the atmosphere the role of such a “spoon” is played by the pressure gradient forces that create winds stirring the atmosphere. Irregularities of air pressure gradient and density lead to the partial transformation of the regular large-scale air flows into turbulent eddies mixing the atmosphere. In other words, it is necessary to bear in mind that, unlike molecular diffusion, turbulent diffusion does not occur ‘by itself’ but demands a macroscopic force to be acting on the system. Therefore, in the presence of vertical wind and air circulation caused by water vapor condensation and the evaporative force, the diffusion processes in the atmosphere no longer remain molecular, they are of turbulent nature. Moreover, as shown in the paper, the coefficient of atmospheric turbulent diffusion is directly calculable from the evaporative force parameters (see p. 2642, line 25 in the paper and pp. S1178-1179 in the response to Dr. Dovgaluk).

In the comment an attempt is made to compare the rates of gas transport by turbulence and by the evaporative force (the latter incorrectly interpreted as molecular diffusion). It should be noted that the turbulent fluxes of atmospheric gases have been quantified in detail using the observed values of eddy diffusivity and vertical pressure gradients, see the authors’ response to Dr. Dovgaluk. This analysis has shown that for all gases, except for water vapor, the dynamic flux caused by the evaporative force is much larger than turbulent fluxes; for water vapor the turbulent and dynamic fluxes are of comparable magnitude and of the same direction in the ascending air masses. In other words, the time scale of vertical dynamic movement of most air gases is much shorter than the time scale of their turbulent mixing.

To summarize, an essential point that was not taken into account in the comment is that on average the condensation process occurs continuously and, thus, continuously creates pressure shortage and the non-equilibrium pressure gradient in the atmosphere. Gases react to pressure shortage by dynamic movement along the non-equilibrium pressure gradient (hence general atmospheric circulation), irrespective of whether they are mixed or not, i.e. whether the flow is laminar or turbulent. More-

HESSD
3, S1705–S1712, 2006

over, turbulent mixing itself arises only as a by-product of such dynamic movements, caused by pressure gradients. The statement about the negligible magnitude of the evaporative force in the atmosphere resulted from the confusion between the diffusion processes and the dynamic, force-induced movement of gases and from the neglect of the fundamental differences in the physics of liquids and gases. The latter point lends further support to the conclusion earlier reached in the authors' comment on the non-equilibrium state of moist atmosphere, that namely the traditional treatment of the atmosphere as a **fluid** (or even a liquid, hence the term "hydrostatic equilibrium" and the above examples with fluid in a glass), rather than explicitly as a gas, excluded the possibility of accounting for the evaporative force within the conventional meteorological framework.

2) Choice of precipitation transects and atmospheric moisture transport

In the comment the reference is made to Fig. 12.17 of Peixoto and Oort (1992) ("Physics of climate", AIP, New York), which shows the global map of the total aerial runoff Q as a global mean and means for December, January and February (DJF) and June, July and August (JJA). From the relationship div $Q = E - P$ shown in the legend to Fig. 12.16 it can be inferred that in the notations of our paper Q corresponds to the moisture flux F (Eq. 1, p. 2626). On the basis of Fig. 12.17 it is argued in the comment that the choice of precipitation transects in the paper is made in the opposite, as compared to moisture flow, direction in the West Equatorial Africa, USA and East Asia transects, while the Yenisey transect is referred to as running perpendicular to the vector of Q.

However, if Q indeed stands for moisture flux F, the data of Fig. 12.17 should be interpreted with much caution, perhaps due to their very low resolution. Atmospheric moisture can only arrive to any particular location with wind, so the vector of Q must be in concordance with the patterns of air circulation. Judging from Fig. 12.17 it can be concluded that moisture transport occurs in the direction away from the continent in the West Equatorial Africa both in DJF and in JJA. However, it is well established that precisely the opposite is true for both time periods, see, for example, the detailed picture of African circulation in Fig. 1 of Nicholson (2000).

Further on, it can be concluded from Fig. 12.17 that moisture is transported from west to east over the entire Eurasian continent, so that even the Far East receives inputs of Atlantic moisture. In reality, however, the influence of Atlantic air masses cease altogether right before the Yenisey transect. Accordingly, most part of Eastern Siberia is characterized as the area of Arctic atmospheric transport. We cite Shver (1976, p. 105) (our translation): *"The area of Arctic [atmospheric] transport spreads over the territory of East Siberia limited by the Yenisey mountain ridge in the West, Stanovyi mountain ridge in the East and Baikal Mountains in the South. Here Arctic air masses dominate, since the conditions of general circulation are such that neither Atlantic nor Pacific air masses reach here. ... [The meridional climatic] border along the Yenisey river is more important than the one along the Urals. It is here that the dominance of Atlantic air masses ceases, and they give place to the Arctic air masses."*

Due to the general complexity of air circulation patterns, it is sufficient to take the direction of moisture transport into consideration only very approximately, provided the transect is chosen in the direction away from the oceanic coast towards the center of the continent. The results will remain unaffected. For example, even if moisture had been transported to the Yenisey transect along the Eurasian parallels from the Atlantic ocean, as presumed in the comment based on Fig. 12.17, this would not help to explain the phenomenon of non-decreasing precipitation along the transect without involving the biotic pump. This is because the corresponding distance from the Yenisey river mouth to the Atlantic ocean is nearly twice less than the $\sim 6 \times 10^3$ km distance from the Atlantic ocean to the innermost part of the Yenisey transect.

3) Biotic pump and global circulation models

It is stated in the comment that *"current generation General Circulation Models do not seem to need this mechanism [of biotic pump] in order to realistically model the*

HESSD

3, S1705–S1712, 2006

distribution of precipitation over the continents."

This is, however, not true. General circulation models build on two conceptually different components, physical principles and empirical parameterizations. Physical principles represent the well-established physical laws, like, e.g., Newton's laws of motions. Parameterizations represent mathematically formalized decriptions of all empirical phenomena, for which no knowledge is available on how they could be numerically deduced from the fundamental physical principles. These parameterizations concern, among others, the global distributions of atmospheric pressure and temperature and even the fundamental parameter of atmospheric eddy diffusivity. With use of a large number of parameterizations, global circulation models are purposely fitted to describe the observed data. Thus, the biotic pump mechanism, which, as argued in the paper, dictates the horizontal and vertical air pressure gradients, wind speeds and turbulence parameters, **is** included into GCMs as an implicit stationary parameterization. However, as far as the physical undestanding of this mechanism is lacking, modern GCMs cannot in principle **predict** the consequences of the degradation of this mechanism. Since the evaporative force is neglected in GCMs, it is generally assumed that there is no major causal link between the atmospheric circulation patterns and the properties of the continental vegetation cover. The former is considered as a geophysical constraint on the functioning of the latter. It is for this reason that GCMs predict only slight precipitation reductions after a large-scale deforestation, contrary to what is predicted from the consideration of the biotic pump mechanism.

In conclusion, in the interest of the readers it should be noted that most of the issues raised in the present comment have been considered elsewhere, either in the paper or in the preceding comments. In addition to the references given above, one can find materials on the difference between diffusion and dynamic force on p. S1451-S1452 in the response to the first comment of Dr. de Melo Jorge Barbosa; a description of a thought experiment with removal of one gas from a mixture of gases on pp. S1450-S1451 and pp. S1496-S1497 of the responses to the first and second comments of Dr.

de Melo Jorge Barbosa, respectively; a discussion of the principal difference between the physical properties of a gas and a liquid and its critical importance for atmospheric phenomena on pp. S1661-S1666 of the authors' comment on the non-equilibrium of moist atmosphere; the dominance of Arctic air masses in the Yenisey transect and the predictions GCMs with respect to precipitation reduction after deforestation were discussed on p. 2630 and pp. 2631-2632 in the paper, respectively; the difference between the modelling and physical approaches were considered on pp. S1494-S1495 of the response to the second comment of Dr. de Melo Jorge Barbosa.

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HESSD

3, S1705–S1712, 2006

Interactive Comment