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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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In their recent reply (S167-S175) the DP authors state that they have found no reason to change their standpoint. At the same time they find the texts of R. Feynman to which we referred "unsatisfactory" and "confusing". Moreover, the DP authors explicitly admit that they "do not understand why it would be impossible to analyze the effects (not causes) of partial disequilibrium without invoking condensation." Here we respond to the arguments of the DP authors, hopefully to resolve their confusions and further elucidate the role of condensation for generating air motions.

1. On fundamental principles

Unlike the DP authors, we do not consider the text of R. Feynman "confusing". Yes, the ONLY static state of the atmosphere is the state when all partial pressures of air constituents are in aerostatic (component) equilibrium. We invite the DP authors to explicitly agree or disagree with this statement. If the DP authors believe there is another static state of the atmosphere (with zero wind velocities), of which R. Feynman was unaware, it would be most interesting to see a theoretical description of what it is, as a further contribution to the didactic purposes of this discussion.

Because the ONLY static state of the atmosphere is the state when ALL atmospheric gases are in aerostatic (component) equilibrium, it follows that in the atmosphere where one component (water vapor) is principally out of equilibrium *there must be air motions*. This is one of the major messages of MG (2007). The intensity of these motions is naturally related to the magnitude of the departure of water vapor from the static component equilibrium and to the intensity of all physical processes that might sustain the disequilibrium. This is what Eq. (15) is about, which describes the maximum attainable wind speed.

Now about the confusion of the DP authors and their unsatisfaction with R. Feynman (S169). The DP authors write: "See e.g. the experiment described in section 43-5 of Feynman: When in a container of gas in thermal equilibrium, a small amount of a different gas is introduced, the latter will spread out by diffusion. In that text, partial equilibrium is clearly distorted, but it is assumed that the experiment is done without a distortion of static equilibrium (i.e. no winds or convection). As such, static equilibrium can coincide with a distorted partial equilibrium (as described also in the DP)."

Notably, in the first paragraph of section 43-5 R. Feynman warns that it is important *not to confuse* the diffusion of gas and the transfer of large amounts of matter by convective currents. He further states that usually mixing of two gases occurs as a combination of convection and diffusion. *Now* we are interested, R. Feynman continues, in such

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mixing that *is not accompanied by "wind blows"*.

Therefore, we can see that partial disequilibrium accompanied by static equilibrium is, in the view of R. Feynman, an *unusual* situation. What precisely is unusual and where the DP authors got confused? This unusual situation consists in equlal *mobilities* of different gases, i.e. equal molecular diffusion coefficients, a condition which generally never holds for different gases. Indeed, the diffusion equation says that, in the absence of forces acting on the gas in the container, flux F_i of molecules of kind i is proportional to the concentration gradient dN_i/dx of these molecules:

$$F_i = \nu_i dN_i/dx \tag{1}$$

where ν_i is molecular diffusion coefficient $\nu_i \sim lv$, where l is molecular free path length and v is molecular velocity that at a given temperature is inversely proportional to the square root of molecular mass M_i.

It is easy to see that if there are two gases 1 and 2 with total concentration $N = N_1+N_2$, then, at $dN_i/dx \neq 0$, $dN/dx = 0$ if and only if $\nu_1 = \nu_2$. This is because otherwise the fluxes F_1 and F_2 having the opposite direction will change the original bulk equilibrium, transporting the heavier molecules more slowly in one direction that the lighter molecules in the opposite direction in the container, thus creating a non-equilibrium gradient $dN/dx \neq 0$. If $dN/dx \neq 0$, this means that there is a pressure gradient and dynamic motions of the gas. Therefore, the case considered by R. Feynman is indeed very special as it implies diffusion of *different* gases with *equal* molecular masses. (For example, different isotopes of one and the same heavy gas that have approximately equal molecular masses can approximately conform to this requirement.) This is done to better explain the essence of diffusion to the students and does not stand in any contradiction with the earlier statement of R. Feynman that a static atmosphere is the one where all gases follow their component-equilibria.

On p. S170 the DP authors refer to the phrase of Landau and Lifshitz (1987): "It is stated there that: "The Navier-Stokes equation (15.5) is also unchanged". This means that the dynamics and statics still rely on bulk pressure gradients, not on partial pressure gradients." This quotation summarizes the logical confusion of the DP authors. Yes, air motion is governed by bulk pressure gradients and this is what the equations of hydrodynamics are about. However, the equations of hydrodynamics do not reveal the nature of pressure gradients and say nothing about it. One imposes an external pressure field and solves the equations to obtain the distribution of velocities, this is how the hydrodynamics equations are logically organized. The biotic pump theory explains *what determines bulk pressure gradients.* In the case of the evaporative force, these bulk pressure gradients are related to partial pressure of water vapor. The DP authors used the wording "intimately" or "straightforwardly" related, we do not know what these words scientifically mean. We have been explicit on how they are related, (1) in circulation events driven by the evaporative force, the bulk pressure difference Δp is equal to Δp_v (exactly because *all* air reacts to water vapor pressure shortage caused by condensation!) and (2) bulk pressure difference is distributed along the entire streamline.

2. Condensation

We have stated in our first comment and later as well that the DP authors misunderstand the role of condensation for the evaporative force. Their last reply provides compelling evidence for that. The DP authors are explicit: "Finally, we do not understand why it would be impossible to analyze the effects (not causes) of partial disequilibrium without invoking condensation (S51, full paragraph). Partial disequilibrium exists very often without simultaneous condensation. We understand even less why the analysis of our thought experiment would have to be modified by considerations about condensation, as the released water vapor will condensate only long after the end of the experiment."

Partial disequilibrium of water vapor indicates how much vapor undergoes condensation when the surface air moves upward. Condensation leads to disappearance of gaseous water and lowers *bulk air pressure* by an amount of partial pressure of con-

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densed vapor. This sustains the pressure gradient force *acting on moist air as a whole* as repeatedly emphasized in MG(2007) and beyond. Condensation provides potential energy from which the kinetic energy of air is formed. It is therefore possible to analyze the effects of partial disequilibrium without condensation, but such analyses will be irrelevant for the physics of the evaporative force and the air motions induced by it.

Regarding the thought experiment – the statement that "the released water vapor will condensate only long after the end of the experiment" – is incorrect. The DP authors appear to think that after "relaxation" of the initial pressure difference, the atmosphere will remain still. They forgot about the energy conservation law: potential energy associated with their initial pressure gradient will turn into the kinetic energy of ascending air masses. If the original pressure difference is $\Delta p \sim \Delta p_v$, then they will observe a tornado-like upwelling of air masses (around 50 m/s). In less than a minute these air masses will reach the level of 2 km, after which most water in the atmospheric column will undergo condensation, thus creating exactly the same pressure difference Δp_v that initiated the motion. This process has nothing to do with molecular diffusion, to which the DP authors repeatedly try to reduce the evaporative force.

The resulting stationary pattern of the circulation will depend on the rate at which water vapor is imported to the condensation area by those air currents that are induced by the condensation-related pressure drop. It is impossible to solve this problem without considering the spatial dimensions of the circulation and the intensity of friction. Again, we sress that the "relaxation" of pressure difference, to which the DP authors many times refer, does not result in a motionless atmosphere. This is the difference with air in a container, where the container walls can promptly absorb gas motion.

The DP authors further state: "Therefore, the remark (S50, last paragraph) that condensation should be treated along other lines than evaporation because it is a much faster process, is not relevant. The condensation rate will remain limited since (at least in the lowest few kilometers of the air column where sufficient condensation nuclei are present), upon condensation, the vapor pressure equals its saturation value, which is a

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function of temperature alone. This implies that condensation is a gradual process and there is no reason to think that condensation is too fast to be followed by the (small) vertical displacements required to restore hydrostatic equilibrium."

Again, we have to remind the DP authors about the energy conservation law. Restoration of hydrostatic equilibrium by "small vertical displacements" produces a non-zero air velocity (conversion of potential energy to kinetic energy). Initial partial pressure difference produced by condensation initiates air upwelling. This upwelling brings water vapor to the area of condensation. Water vapor condenses and sustains pressure difference.

Condensation is NOT a gradial process. It can occur at any arbitrarily high rate bounded from above only by w_{max} of Eq. (18) of MG(2007). For example, in hurricanes condensation occurs hundreds of times more rapidly than the average condensation rate that is equal to the rate of evaporation. Condensation rate is proportional to the vertical velocity of air motion.

Here lies the fundamental difference between mixing of two different gases without condensation (partial disequilibrium) and condensation-driven air circulation. If a small amount of gas is added to the container with another gas, the resulting dynamic air flows will depend on the difference in molecular masses of the two gases, see discussion of Eq. (1) above. These flows enhance mixing and contribute to the disapperance of its cause: the original partial pressure disequilibrium. In contrast, air flow initiated by condensation sustains and can enhance condensation rate via water vapor import.

3. On pressure difference

On p. S168 the DP authors state that the fact that "a substantial change in the partial equilibrium of the dry air occurs" implies that "that the theory of the evaporative force, as introduced in MG (2007), is untenable *(see section 2 below)*." In section 2 the reader is referred even further, as the DP authors say: "Now we find ourselves compelled to pose the critical question: "What else does the evaporative force express then?" We expect that the answer will be: the same Δp_v should be applied over a different trajectory. *This viewpoint will be considered below in Section 4.*" In Section 2 it is merely stated that "the air has locally relaxed almost to bulk-equilibrium (DP). With this relaxation, the effect of Δp_v on atmospheric motions on a larger scale is already reduced by several orders of magnitude."

There is no logic behind this conclusion: We state that if $\Delta p = \Delta p_v$ is spread along the entire trajectory, only a small part of it will fall on the vertical dimension of the streamline. (It is rewarding that the DP authors appear to have ultimately realized that.) However, the fact that it is indeed so in the observed large-scale circulation patterns (which are characterized by a horizontal pressure difference $\Delta p \sim \Delta p_v$ and intense condensation), does not in any way mean that "the effect of Δp_v on atmospheric motions on a larger scale is already reduced by several orders of magnitude". This just means that $\Delta p = \Delta p_v$ is indeed distributed along the entire streamline. Note that it is namely *bulk air pressure difference* Δp *equal to* Δp_v and not partial pressure of water vapor itself that is distributed along the streamline.

In Section 4 the DP authors make a few statements about the presumed *local* nature of the evaporative force, e.g. "To obtain the observed small upward and downward vertical accelerations, the compensating pressure gradients should be located in the same place as the "Evaporative Force" (S173) or "It is obvious that the contradiction cannot be removed by making an appeal to the continuity equation ... that will not influence the local prediction of the dynamic equation for vertical acceleration, which is based in MG (2007) on the water vapor profile." These verbal speculations lack a physical ground. If one has a ventilator or a vacuum-cleaner working in the room, which locally sucks air in, the resulting air circulation and the associated air pressure gradients are NOT localized within the vacuum-cleaner but may impact the entire room. Yet no one will dispute that this circulation IS driven by the vacuum-cleaner or the ventilator. This non-locality of pressure differences does not give any grounds to accept "that the pressure differences which drive atmospheric motion, have no intimate relation with" the vacuum-cleaner or, in the case of the BPT, with the evaporative force.

See also on p. S173: "To obtain the observed small upward and downward vertical accelerations, the compensating pressure gradients should be located in the same place as the "Evaporative Force". Furthermore, they can only be identified as gradients in the dry-air component, as was done in the DP. This local compensation means that no "Evaporative Force" remains, since the net resulting force does not at all reflect an "Evaporative Force" as it is very small and of variable direction." – the fact that air around the vacuum-cleaner flows in quite different directions does not cancel the fact that it is the vacuum-cleaner that provides the driving force for the circulation.

Having wandered all the route from Section 1 to Section 2 and then to Section 4 a thoughtful reader should be unable to find any proof for the many times announced untenability of the evaporative force.

4. Other comments

We would like to stress once again that MG (2007) never stated that when air flow is present, the hydrostatic equilibrium of dry air cannot be changed. We wrote on p. 1022 that "In agreement with Dalton's law, partial pressures of different gases in a mixture independently come in or out of the equilibrium. The non-equilibrium state of atmospheric water vapor cannot bring about a compensating deviation from the equilibrium of the other air gases" – this is absolutely true and, as we clarified in our response (S47), implies that "There is no such a static state of the atmosphere where the vertical distribution of dry air constituents would, to any degree, compensate the component disequilibrium distribution of water vapor to produce bulk equilibrium of the moist atmosphere as a whole." That is, other gases cannot compensate for the disequilibrium of water vapor *to make the atmosphere motionless = static*. In Eq. (15) the component equilibrium for dry air is assumed (and hydrostatic equilibrium of dry air as a whole), producing a vertical pressure difference $\Delta p = \Delta p_v$, but on p. 1023, left column, it is described that such a situation is only possible under particular conditions

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of very intense circulation events. In stationary circulation, due to the re-distribution of pressure difference Δp along the horizontal streamline (see p. 1023 of MG2007), the vertical distribution of pressure for dry air constituents does change.

The DP authors are incorrect in that this is "explicitly admitted for the first time that a substantial change in the partial equilibrium of the dry air occurs." It is explicitly stated on p. 1023 of MG(2007) that pressure difference $\Delta p \sim \Delta p_v$ is distributed in the horizontal dimension. We understand that the DP authors were unable to retrieve from this that the pressure difference along the vertical dimension *is also affected* and *how it is affected*, but at the same time we think that there can be quite a few people for whom that was not a problem. In any case, an explicit treatment of pressure differences and their distribution was given by Makarieva, Gorshkov and Li (2008) ACPD 8, S8904-S8915, 2008 (http://www.cosis.net/copernicus/EGU/acpd/8/S8904/acpd-8-S8904.pdf) on November 10, 2008, i.e. over two months earlier than the publication of the present DP.

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