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Hydrology and Earth System Sciences Discussions

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6, S311–S317, 2009

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.

A. Makarieva

elba@peterlink.ru

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In this comment, eyeing the approaching closure of the discussion on Friday, 13th March, we respond to the recent comments of the DP authors and to the comments of Referee 2.

1. According to the DP authors, condensation now leads to the RISE of air temperature (!!!) in the air parcel (S307), which causes air pressure to rise instead of dropping as it should due to the removal of water vapor from the gas phase. This, according to the DP authors, "follows from physical laws that have been well established for one and

a half century on the basis of observation". In reality, however, this statement is fundamentally erroneous. Condensation in the atmosphere CANNOT RAISE air temperature of the air parcel where this condensation takes place, because it occurs AFTER the temperature drops when the air parcels move upward in the atmosphere with a negative vertical temperature lapse rate and DUE to this temperature drop. To say the reverse means to speak against the second law of thermodynamics and against the Clausius-Clapeyron law, which dictates higher concentration of water vapor at higher temperature, NOT THE REVERSE (higher temperature at lower water vapor concentration), as the DP authors now propose. To summarize: as it rises, the air parcel COOLS and loses some part of its gas (in the form of water vapor). The statement of the DP authors made under subsection (a) that condensation rises temperature of the air parcel where it occurs renders all the subsequent criticisms present in Section 2 (S307-S310) physically untenable. For the record, we already had one attempt to explain the above physics to Dr. Meesters, the relevant exchange can be traced from http://www.cosis.net/copernicus/EGU/acpd/8/S8923/acpd-8-S8923.pdf. Also in passing, we note that the power of condensation (flux of potential energy released during condensation) remains undiscussed, which is not surprising.

What is indeed well-known and taken into consideration in the conventional meteorological paradigm is the diminishment of the moist adiabatic lapse rate compared to the dry adiabatic lapse rate (9.8 $\mathrm{K\,km^{-1}}$). Obviously, the air column where lapse rate is moist adiabatic is warmer in the middle than the column where the lapse rate is dry adiabatic (provided that air temperatures at the surface are the same). However, in the vertical dimension air the middle of the column remains COLDER than air the surface. HORIZONTAL temperature gradients are conventionally taken into account when modelling atmospheric motions and considered the primary cause of motion, we are well aware of this fact. The BPT proposes that the neglected non-equilibrium drop of air pressure due to condensation ALONG THE VERTICAL is a different and major driver of atmospheric circulation.

HESSD

6, S311–S317, 2009

Interactive Comment

2. In Section 1 the DP authors take the statement of Referee 2 that "The pressure difference generates a "wave of expansion" traveling at the speed of sound (see the Comment of Referee 2) and it is within this wave that the kinetic energy is located."

The examples and references cited by Referee 2 pertain to relaxation of an initial perturbation. One should remember that sound waves are, by definition, small-amplitude fluctuations, for which the second term in Euler equation (written for simplicity for the one-dimensional case) can be neglected compared to the first one:

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} = -\frac{\partial p}{\partial x}\frac{1}{\rho}. \tag{1}$$

Writing this equation approximately in terms of finite difference we have

$$\Delta u\frac{\Delta x}{\Delta t} + u\Delta u = -\frac{\Delta p}{\rho}. \tag{2}$$

One can immediately see that the first term becomes larger than the second one at $\Delta t \to 0$, i.e. when the time during which the perturbation is introduced is small. This condition is obeyed in the papers cited by Referee 2. E.g., in Bennon (1995) $\Delta t = 0$, i.e. the perturbation is introduced instantaneously.

In the real atmosphere the process of condensation that sustains the air pressure imbalance (and the process of evaporation that, on a global average, compensates for the condensation), occur CONTINUOUSLY within the circulation events driven by the evaporative force, so the application of the equations for sound waves is physically incorrect. Again, there is no "relaxation". There is a stationary circulation pattern with the first term of Euler's equation equal to zero. Evaporation and condensation ARE NOT random perturbations of the atmosphere, but common processes continuously at work. For a study of the effect of such processes the sound wave formalism is inapplicable.

3. Through this discussion, the DP authors have made it clear that the evaporative force for them "appears hard to understand" (S304). However, we remind that M&G2007 introduced the evaporative force by explicitly mentioning condensation in the first place

HESSD

6, S311–S317, 2009

Interactive Comment

and emphasizing that it is EVAPORATIVE force inasmuch the evaporation compensates for the condensation (p. 1022): "As far as the ascending water vapor molecules undergo condensation, the stationary existence of force f_E is only possible in the presence of continuous evaporation from the surface, which would compensate for the condensation. It is therefore natural to term force f_E, Eq. (16), as the evaporative force."

When writing the paper we could not know what word could have a better appeal to most readers. For us the evaporative force is better as it implicitly bears a connection to the significant role of forest evaporation for the biotic pump. Seen purely physically, someone might say it might be better termed the condensational force. The essence of the phenomenon will not change because of the word used.

4. On p. S305 the DP authors state: "In S179, first paragraph, it is stated that: "The equations of hydrodynamics do not reveal the nature of pressure gradients and say nothing about it. One imposes an external pressure field ...". This is an unrecognizable rendition of the facts to anyone with some serious expertise in atmospheric modeling. Only at the lateral and upper boundaries (if any) are pressures imposed externally. Within the boundary layer, everything depends on the appropriate internal computation of the pressure field from diagnostic or prognostic equations. The easiest method is to determine the bulk pressure gradients using the well-known hydrostatic pressure equation, whereas more refined methods are applied for work on finer scales (see the literature on dynamic meteorology, e.g. Pielke 1984)."

The DP authors should probably realize that the main point is that pressures ARE imposed externally, not retrieved from the equations of hydrodynamics. By imposing pressures at spatial boundaries and knowing the linear dimensions of the considered circulation pattern one immediately sets the average pressure gradient for the considered area, with further finer details to be retrieved from the equations of hydrodynamics. The BPT namely determines what is the pressure difference between spatial boundaries of the considered circulation pattern.

HESSD

6, S311–S317, 2009

Interactive Comment

5. On p. S305 the DP authors state "Ad S177 (bottom)-S178: The argument is ingenuous, but incorrect. ... However, it is stated nowhere that diffusion is the cause of, or cannot exist without, convection! The remainder of the argument in S178 is based on this erroneous interpretation of Feynman's statement."

The DP authors fail to mention that although R. Feynman does not state that "diffusion is the cause of, or cannot exist without, convection", the creation of pressure gradient by diffusion process is immediately obvious from simultaneous consideration of diffusional fluxes of mixture components with different mobilities. This effect is very well-known and even used to separate gas mixtures. The gas mixture is placed within a container which has a porous wall through which molecules of both gases can penetrate but which precludes dynamic motion. Due to higher mobility, the lighter molecules diffuse more rapidly via the wall to the second, empty container, which allows for the separation of two gases (see, e.g., http://en.wikipedia.org/wiki/Isotope_separation for general reading). In the result, there appears pressure difference across the wall. A reverse process of mixing of two gases (air and CO_2) that is accompanied by the appearance of a pressure difference can be found, flash animated, at www.bioticregulation.ru/pump/pump3-4.php.

In the absence of the porous wall such pressure differences in a mixture where diffusion takes place should be created and relaxed in a complex manner, which is not the topic of our study. As we repeatedly noted, in the evaporative force physics the pressure difference is produced by condensation, not by molecular diffusion. However, consideration of diffusional fluxes F_i, Eq. (1) on p. S178 of our comment, makes it clear that diffusion cannot go without the appearance of pressure gradients.

6. On p. S309 the DP authors reproduce a few ideas from the conventional paradigm on how convection is formed due to change in buoyancy, with an addition of some remarkable conclusions made on their own, like this one: "For example, if clouds are sucking in air from all sides as the BPT predicts, they should, according to mass balance considerations, not become lighter (as is observed) but rather denser and heavier

HESSD

6, S311–S317, 2009

Interactive Comment

than dry air at the same altitude. As a consequence, application of Archimedes' Law would predict downward motion of the cloud (weight greater than buoyancy), i.e. the opposite of what is observed."

The DP authors are incorrect when stating that the BPT predicts that clouds should "suck air from all sides". As moist air moves upward, the amount of water vapor within it diminishes and the local power of condensation diminishes as well. Therefore, in the part of atmospheric column above the point of condensation there is a diminishing gradient of the evaporative force that is upward directed. The air accelerated by the evaporative force acquires velocity and does not, so to speak, "end in the cloud", as the DP authors propose. Quite the opposite, having accelerated when reaching the region of condensation by the upward-directed force, the air uses this momentum to pass by the region of condensation (which can be quite extensive in height) to the upper atmosphere (to further participate in horizontal motion as prescribed by the closure of the circulation pattern). So, there are no grounds for the cloud region to become heavier.

Regarding the representation of the conventional paradigm by the DP authors, they fail to mention many crucial cases which remains unexplained by this paradigm (which does not account for the dynamic effect of water vapor condensation). For example, according to the conventional paradigm, the upward movement of air masses should be associated with the regions of positive buoyancy (i.e., where the air is warmer and lighter than the surroundings). In contrast, careful observations show that atmospheric updrafts "exhibit a wide range of positive and negative buoyances (Folkins, 2006)" (p. 1021 of MG2007). This is consistent with the BPT, which predicts that not buyoancy, but the intensity of condensation primarily determines the upward air movement. If condensation is intense, it can "lift" even heavier than average air. The conventional theory based on consideration of thermal gradients is unable to reproduce the observed intensity of Hadley circulation (e.g., Fang & Tung, 1999). As pointed out by Dr. Nobre (S282), the conventional schemes cannot account for the Amazon water budget.

HESSD

6, S311–S317, 2009

Obviously, temperature gradients do play a role in atmospheric circulation. However, the BPT shows that without taking the horizontal temperature differences into account one can predict both horizontal and vertical velocities of observable magnitudes from the dynamic power of condensation alone. (Unlike in the conventional paradigm, this is done without involving the coefficient of atmospheric eddy diffusion phenomenologically.) Logically, this means that either the dynamic effects of temperature and condensation gradients coincide in magnitude (which would be a remarkable coincidence of numerically independent physical effects) or, which we consider more likely, that the effect of condensation is a major one compared to temperature in generating the atmospheric circulation.

This is our tenth and last entrance to this discussion, since the number of short comments is limited to five per each Cosis member except the authors of a DP. As the Editors informed us, we will be given an opportunity to submit a formal response to the revised manuscript of the present DP. We hereby thank all the discussion participants and especially the DP authors for initiating this discussion and giving us this fascinating opportunity to present our findings to an attentive and thoughtful audience. We are looking forward to reading the finalized manuscript and continuing the discussion elsewhere.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 401, 2009.