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Hydrology and
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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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Upon reading the two responses to our critique, we appreciate the readiness of the DP authors to re-evaluate their statements upon further discussion (S34) and hereby provide several key clarifications to that end.

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1 The magnitude of non-equilibrium pressure difference

We invite the DP authors to pay special attention to the statement that might have been ignored when they wrote (S35) "no answer has yet been given to our objection that when air flow is present, the distribution of the dry air component in space must be changed thereby (DP, end of page 411-beginning of page 412). MG (2007) use throughout the principle that it cannot be changed, "because of Dalton's Law"."

We stress that MG (2007) never stated that **when air flow is present** the hydrostatic equilibrium of dry air "cannot be changed". In our comment S1-S10, as well as in point 2 of the checklist, it is explicitly stated that, according to the BPT, the relative non-equilibrium pressure difference along the vertical is $(p_v/p)h/L$, where h and L are the vertical (smaller) and horizontal (larger) linear dimensions of the circulation pattern induced by the evaporative force. Only in tornadoes with $h \sim L$ the non-equilibrium vertical pressure difference is comparable to relative partial pressure p_v/p of water vapor. We emphasize once again: the distribution of dry air components is changed **when circulation induced by the evaporative force is present**. It is changed such as the resulting relative vertical disequilibrium pressure becomes of the order of $(p_v/p)h/L$.

The statement used instead in MG(2007) and everywhere else in our writings and presumably misinterpreted by the DP authors in the sense of the above quotation, is the following: 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. We refer to another "antique" author, R. F. Feynman (1963), who writes when discussing static equilibrium of the atmosphere (Chapter 40. Principles of static mechanics, Section 1. Exponential atmosphere) that "if there are several sorts of molecules with different masses, their numbers will decline with altitude along different exponential scale heights." In other words, the static atmosphere obeys component-

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equilibrium of each gas **and this is the only static equilibrium possible for the gas mixture**.

The DP authors on pp. S39-S40 were unable to immediately agree with our argument that the pressure difference $\Delta p_v \sim p_v$ is distributed along the entire streamline, so that the above value $(p_v/p)h/L$ for the vertical part of the streamline immediately follows. Indeed, this argument literally kills the critique presented in the DP, especially the original idea that the evaporative force should produce hurricane velocities above any evaporating surface. To resist this critical argument the DP authors simply repeated themselves, stating once again that BPT predicts a very large disequilibrium over the vertical trajectory and stated, without any grounds, that "this paradox cannot be resolved by attributing the two sides of the same equation to different trajectories."

The single argument put forward (S39) was that "these equations are derived straightforwardly by calculating the presumed evaporative force from (roughly) observed profiles, which are real-world profiles and hence should already contain the adaptation to any feedback occurring in the real world." This is incorrect. Besides, here the DP authors contradict themselves, because, according to their main statement, moist air of the real world as a whole is in hydrostatic equilibrium, so if MG had used the observed profiles, they would have never obtained their hurricane velocity produced by the evaporative force. In reality, Eq. (15) of MG(2007), to which the DP authors refer, is not an empirical parameterization, but a purely theoretical derivation quantifying the departure from the **static** equilibrium state of the atmosphere. In this derivation, for dry air components **an exact hydrostatic equilibrium** is assumed, see the last line in the left column on p. 1021. As clarified on p. 1019 of MG(2007), when each of the mixture components is in (aerostatic = component) equilibrium, the mixture as a whole is in equilibrium. This assumption is used for the dry air mixture. Constant mixing ratio of dry air is not used anywhere! Water vapor is principally out of equilibrium as dictated by the Clausius-Clapeyron equation. Thus, the magnitude of the evaporative force, Eq. (15) on p. 1021 of MG(2007), is derived theoretically from the consideration of the properties of static equilibrium atmosphere, namely its component (aerostatic) equilibrium, and the property of condensability for atmospheric water vapor.

Further on, when this (or any other) force is applied to a continuous medium, it is distributed along the entire object, be that train dragged by the locomotive or atmosphere cycled by the evaporative force. As we explained in detail in our first comment and references therein, this is an immediate consequence of the continuity equation for air. That the non-equilibrium pressure difference Δp_v is distributed along the horizontal streamline is additionally supported by empirical evidence – data on horizontal pressure differences in hurricanes and cyclones, see also p. 1023 in MG(2007) where this horizontal difference is discussed. (A similar phenomenon: if there is a differential heating rate that induces air circulation via temperature gradient, this circulation in turns modifies the temperature gradient depending on particular conditions of the considered problem, so that different temperature gradients are possible with one and the same heating rate.)

The neglect of the continuity equation led the DP authors to presume that the evaporative force should have driven the whole atmosphere upward (S35). In reality, even on a roughly homogeneous surface, due to spontaneous symmetry breaking, vanishingly small inhomogeneities in the evaporative force magnitude lead to formation of closed circulation patterns (cells), with local downdrafts and updrafts, as in any fluid exposed to any approximately homogeneous external forcing (e.g., heating). Here the evaporative force makes no exception to other forcings. Generally, it is **impossible** to apply a force to the atmosphere and observe a unidirectional movement of air, like pushing a book from one side of the desk to the other. The continuous medium is different. Any applied force will create corresponding compensating pressure gradients to form a close streamline in the resulting circulation pattern. In their independent development of the physical bases of the BPT, the DP authors have missed this issue.

It would be valuable if the DP authors provided an exact citation where the "classical result" (S39) about the 2 km distribution of atmospheric water vapor would be derived from an equation similar to Eq. (11) of MG(2007) (or derived theoretically in any other way). The only study we know and cite in our 2007 work, that of Weaver and Ramanathan (1995), characterizes this (fundamental indeed) result as "incidental" rather than "classical", which is a bit suspicious, since these authors are likely to be acquainted with the classical literature.

2 Critical limits to the DP scope: condensation excluded

The discussion offered by the DP authors on pp. S40-S41 looks like the DP authors may have come across an important issue, as the reader is advised to start reading from those pages. The DP authors point out that the large horizontal difference in partial pressures over a wet versus dry surface do not produce hurricane velocities, while according to the DP authors' understanding of the BPT, it should. However, in the absence of condensation sustaining the pressure gradient, the original substantial pressure difference between the moist and wet areas would be instantly relaxed, precisely as the DP authors describe it (S44). The residual processes will be the slow mixing processes equating partial pressures of gases in the mixture. Due to continuous input of water vapor from the evaporative surface to the atmosphere, this mixing will be of dynamic rather than of purely diffusional nature, but it will be very slow due to the small rate of the evaporation process. (BPT does not imply any "straightforward connection between Δp and Δp_v" (MDB S40).)

In contrast, the rate of condensation can be arbitrarily high (MG(2007), S10). Since it is the process of condensation that sustains the pressure gradient responsible for the evaporative force, in atmospheric problems where the rate of this process is known, it dictates numerically the characteristics of the circulation pattern. On this basis vertical velocity for the stationary large scale circulation were calculated by MG(2007), and then the magnitude of horizontal velocity was calculated by equating the powers of the evaporative and friction forces (p. 1026). Conversely, setting horizontal velocity it is possible to calculate other parameters of the problem, e.g., the linear dimension of the area which can be moistened by the evaporative force (Makarieva et al. 2008 ACPD 8: S8904-S8915).

On pp. S40-41, as everywhere else, see point 4 in the checklist (S18), the DP authors ignore the role of condensation for the evaporative force. They insist (S38) that the DP scope is strictly limited to exclude the main physical principle behind the evaporative force, namely the vertical temperature gradient that is responsible for condensation. Further on, they explicitly refrain from discussing condensation in their re-formulation of the thought experiment (S44): "It is not essential to the description of the release process to consider what happens afterwards to the vapor once it has been released into the open air." (Originally we pointed out (S14) that the condensation process will sustain the pressure difference, so that the air motion will continue.) In our opinion, under such severe limitations, excluding the consideration of the role of temperature gradient and condensation, it is hardly possible to form a clear physical picture of the considered phenomenon. As long as the DP authors remain confined within those self-imposed limitations, the various misinterpretations of the BPT by them, outstandingly vividly exemplified by the arguments on pp. S40-S41, will be logically unavoidable.

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