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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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This is a reply to interactive comment S46-S51 by Makarieva & Gorshkov. Although each of their points will be commented upon separately, the discussion below will not be paragraph-by-paragraph, but rather according to subject, starting with the most important subjects first.

It will be seen that we have found no reason to change our standpoint.

1. About fundamental principles

In S47, 2^{nd} paragraph, it is said that "*MG (2007) never stated that when air flow is present, the hydrostatic equilibrium of dry air cannot be changed*". Unfortunately, this dry-air-equilibrium plays a fundamental role in — and is repeated throughout — the theoretical part of MG (2007). For example, after Eq. (15), which describes atmospheric motion, it is stated that its derivation depends on dry-air-equilibrium: "*it is taken into account that dry air is in hydrostatic equilibrium*". Further, at the start of the bottom left paragraph on page 1022 (in the context of a treatment of atmospheric *circulation*) it is declared 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*". Et cetera, et cetera.

The phrase (central part of S47) "*We emphasize once again: the distribution of dry air components is changed when circulation induced by the evaporative force is present*", simply does not apply: in the new comment (S47, 2^{nd} paragraph; central paragraph of S50) it is explicitly admitted for the first time that a substantial change in the partial equilibrium of the dry air occurs (until then this was rejected with an appeal to "Dalton's Law"). But this implies that the theory of the evaporative force, as introduced in MG (2007), is untenable (see section 2 below).

The reference (S47, last paragraph) to R.F. Feynman (1963) requires closer consideration: "*If there are several sorts of molecules with different masses, their numbers will decline with altitude along different exponential scale heights*". First of all, the title of Feynman's chapter is "*Principles of statistical* (not: static as used by MG, S47) *mechanics*". Statistical mechanics is introduced as being "the laws of mechanics which apply just to thermal equilibrium". The cited sentence comes from a piece of text starting with: "*Let us begin with an example: the distribution of the molecules in an atmosphere like our own, but without the winds and other kinds of disturbance (...) and at ther-*

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mal equilibrium". Here, "thermal equilibrium" means what was called "thermodynamic equilibrium" in the DP. The sentence quoted at the bottom of S47 pertains to such an ideal atmosphere. The conditions for thermal equilibrium to be valid are much more stringent than simple static (mechanical) equilibrium. That is exactly what we said in sections 2.1-2.2 of the DP.

As a consequence, the text of Feynman cannot be used to prove 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 of water vapor to produce bulk equilibrium of the moist atmosphere as a whole*" (S47, last paragraph), or that partial equilibrium "*is the only static equilibrium possible for the gas mixture*" (S48, top); upon closer scrutiny of the context (see above), Feynman stated that partial equilibrium is the only *thermodynamic* equilibrium.

However, Feynman's text must be considered as unsatisfactory on one point: whereas the conditions under which the component equilibrium holds are indicated all right, it is not well explained *why* these conditions (thermal equilibrium) imply component equilibrium. Worse, the statement that component equilibrium should hold is preceded by a calculation of hydrostatic equilibrium for an unmixed gas, and the result is then applied without explanation to the components of the mixture. This is confusing! The result is indeed valid for the components, but this fact can only be explained from statistical mechanics (as done in other texts), not from hydrostatics that permits mixtures which are not at all in partial equilibrium. 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).

For a text dealing specifically with the hydrodynamics of mixtures, see Landau and Lifshitz (1987, see DP) which deals specifically with this case in Section 57 entitled "The

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equations of fluid dynamics for a mixture of fluids" (previous sections do not consider the composition of the fluid). 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. In the remainder of that text, it is described how partial gradients only influence diffusion and thermodynamic properties.

2. Reconsideration of the foundations of the Evaporative Force theory

The various remarks by MG about the redistribution of pressure made in numerous paragraphs of S46-S51, contain a new viewpoint: "*When air flow is present, the distribution of dry air components can be changed, and the partial equilibrium of dry air can be distorted*".

We understand that these points were already *implicitly* included in the working out of the Biotic Pump Theory in MG (2007). On the other hand, these points contradict the very assumptions upon which the theory was founded (MG 2007 section 3.1 and first part of section 3.2), see also the beginning of the first section of the present comment. Hence, a re-evaluation of the Evaporative Force theory is required once again.

In S48, 2^{nd} paragraph, it is stated that Eq. (15):

$$\frac{1}{2}\rho\frac{dw^2}{dz} = -\frac{dp}{dz} - \rho g = -\frac{dp_v}{dz} - \rho_v g = f_E$$

is purely theoretical, and only valid to the extent that the dry air is in partial equilibrium. At the same time, it is agreed that this latter condition is not fulfilled (as we emphasized in the DP). Hence, the right-hand side of Eq. (15), which was termed the "evaporative force" does not express the vertical forcing (same remark).

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

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should be applied over a different trajectory. This viewpoint will be considered below in Section 4; let us first consider the consequences for the vertical column. To obtain the (very small) observed vertical accelerations, one has to add in Eq. (15) a pressure disequilibrium for the dry air which approximately compensates the pressure disequilibrium for the water vapor, as in the DP. This implies 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. This follows without having to invoke the spreading out of pressure difference over a longer trajectory.

The unfortunate consequence of this is that the Evaporative Force has lost all its foundation, since it was ultimately based on Eq. (15): remember that the evaporative force f_E was defined in MG (2007) as the right-hand side of Eq. (15). Also, everywhere in the application, Δp_v is regarded as the quantity that is forcing atmospheric motion, without considering the compensating disequilibrium of the dry-air components, which largely eliminates its effect on the dynamics of the bulk air.

3. Further comments on S48, 2nd paragraph

Some of the statements made in the 2nd paragraph of S48, have been replied to already in the preceding sections. Further, in the beginning it is stated that we misinterpreted the status of the profiles. However, our remarks about "observed" profiles concerned only the profiles for water vapor. In the derivation of their "Evaporative Force", MG (2007) combined real-world water vapor profiles with a highly non-empirical profile for dry air which upon application led to hurricane velocities (see the DP). In our opinion, one should work with real-world profiles also for dry air (DP).

The paragraph contains some obscurities. E.g. we do not understand the purpose of the statement "*constant mixing ratio of dry air is not used anywhere!*".

4. Effects of feedback ("continuity equation", "distribution along streamline" etc.)

Several paragraphs in S47-S49 have been devoted to the role of the redistribution of the pressure over a longer trajectory. Our criticism was not based on the "neglect of the continuity equation" (S49, 2^{nd} full paragraph) since the idea that a distortion of the pressure field can be rapidly redistributed over large distances by horizontal and vertical motions is correct in itself. Rather, the problem is that the theory of MG (2007) attributes the moving force to the *partial* pressure of water vapor. The latter field does not quite change so fast as the air pressure field: MG (2007) placed much emphasis on the systematic disequilibrium of the water vapor profile which on the larger scale is practically always preserved (which we admit). However, the same must hold then for the evaporative force (as also implied in the founding part of MG (2007)). Furthermore, this force is localized where the gradients in water vapor pressure are the most pronounced, and by itself should cause an upward acceleration that is much stronger than the observed upward movement (DP).

It is obvious that the contradiction cannot be removed by making an appeal to the continuity equation (as is still done in S48 1^{st} paragraph, and S49, both paragraphs): 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. The only way to remove the contradiction, is by accepting that the pressure differences which drive atmospheric motion, have no intimate relation with the partial pressure gradients of water vapor (as already admitted more or less in S48, 2^{nd} full paragraph; see also Section 2 of this comment): in practice, they are already balanced by the gradient in the partial pressure of the dry air (DP). This, in turn, implies that the idea of the "evaporative force" as the force driving the atmosphere, is untenable.

For example, let us consider the last full paragraph of S49. It is stated there that even over a homogeneous evaporating area, continuity requires that updrafts are compensated by downdrafts. This is obvious, but how is this reflected in the dynamic equation for the vertical acceleration? The quoted paragraph ends with saying that “compensating pressure gradients” are involved. Again, this is correct, but the critical question is: “Where are these compensating pressure gradients located?”. 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.

5. Role of condensation (section 2, S50-S51)

Concerning the role of condensation in the evaporative force theory, it is obvious that condensation enters the theory of MG (2007) because it prevents the water vapor profile to reach partial equilibrium. This latter fact is fully acknowledged at the beginning of section 2.2 in the DP. But the lack of partial equilibrium is in no way a hindrance to reaching bulk- (hydrostatic) equilibrium, as explained in the DP (e.g. section 3.1 analyses how this happens over an evaporating surface): restoration of bulk-equilibrium occurs without restoration of partial equilibrium (see e.g. the Figure in the DP). Hence, one cannot say, as is done in Section 2 of S50-51, that everything would change when condensation is included. The effect of condensation on bulk-equilibrium can be treated along the same lines as the effect of evaporation: partial equilibrium is probably never restored, but hydrostatic equilibrium is approximately restored everywhere. 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 function of temperature alone.

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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.

These remarks pertain to an ideal horizontally homogeneous atmosphere. In reality, an air column in which condensation occurs, will relax to hydrostatic (bulk) equilibrium, but (mainly because of the release of latent heat) the corresponding pressure profile will differ from the profiles of surrounding areas where condensation does not take place. This causes horizontal pressure gradients that drive a circulation in which the moist air will generally be accelerated upward. All this conforms to classical theory as found in textbooks. In sum, condensation is indeed a well-known cause of distortion of mechanical equilibrium in the atmosphere. However, the theory of MG (2007) does not add to our understanding of the mechanism as it does not even work for a horizontally homogeneous atmosphere.

Concerning the statement made in S50 (end of the middle paragraph) that the "*BPT does not imply any straightforward connection between* Δp *and* Δp_v": the two *are* equated in MG (2007) their Eq. (15) — reproduced above in Section 2 — which is central to the Biotic Pump theory. Even the next paragraph (S50-S51) refers to a computation in MG (2007) in which an expression for the evaporative force was used that was ultimately derived from Eq. (15). Although such a direct connection is given up at other places in MG (2007), this does not refute our statements, but only demonstrates the paper's internal inconsistency.

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.

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6. Clausius-Clapeyron theory

The suggestion made in point 6 of the Checklist (S19) that a height of approximately 2 km is “**predicted**” by the Biotic Pump Theory is apparently based on confusion. Taking the expression for h_{H2O} given by Eq. (11) in MG (2007), which is copied from the literature mentioned there, and inserting commonly used parameter values (e.g. for -dT/dz the observed mean Γ_{ob} of 6.5 K km^{-1} (see above Eq. 13)), one immediately obtains h_{H2O} = 2.4 km. Therefore, the derivation of this height does not depend on the lengthy considerations in the text between Eqs. (11) and (13) in MG (2007), nor on the Biotic Pump Theory.

In S49-S50 we were asked to provide an exact citation as to where the result was derived from an equation similar to Eq. (11) in MG (2007). Such a reference (with the Clausius-Clapeyron equation replaced by the Magnus equation — a modification to account for the dependency of Q_{H2O} on temperature) can be found in Von Hann (1915, see the reference list in DP) on page 233, where it is attributed to a still older source: an article by C.W. Trabert in the *Encyclopaedie der mathematischen Wissenschaften* . The result given by Von Hann is a tenfold decrease in water vapor concentration over a height of 5250 m, which upon conversion yields an e-fold decrease over h_{H2O} = 2.3 km, practically the same result as in MG (2007). We have not consulted any more recent texts, where such speculations (combining the assumption of saturation with a globally-averaged profile) tend to be relatively scarce.

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