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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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This is an answer to S1-S10, and to the accompanying "Checklist" S17-S20.

The Comment S1-S10 is well-ordered, and contains an admirable amount of detail. The comment is usually clear, although we are not entirely sure that we have fully understood every argument (in S4-S5 the purpose of the arguments is not always clear). The checklist, an original idea to our knowledge, appears very helpful. In the following, the questions will be answered point by point. Little will be said however about some of the points in the list which are either no point of disagreement, or too far removed from the subjects dealt with in the Discussion Paper (DP). General readers who do not know where to commence, are advised to look at the 10^{th} point first. Before addressing these points, we have to turn to some points which have been skipped in the Checklist.

Fundamental points which are not in the Checklist

The last full paragraph of S3 is in our opinion the most fundamental of all. First it is repeated (S3, third paragraph) that component equilibrium *is* to be thought of in mechanical terms (such as partial pressures being in balance with the weights of the respective components), which we denied in the DP. This is followed by a reference to Dalton's law.

Why would component equilibrium be a consequence of mechanical principles? It is remarkable that MG (2007) start their theoretical discussion (see beginning of section 3.1) with expressing that *according to Landau and Lifschitz*, aerostatic equilibrium of a gas mixture means that change dp_i of partial pressure p_i of the i-th gas over a vertical distance dz is balanced by the weight of this gas in the atmospheric layer of thickness dz (component-equilibrium). We have never been able to find such a statement in that book, nor in any similar work (but if someone can show us, it is likely that we will re-evaluate our standpoint). In Landau and Lifschitz the equilibrium principle is only applied to the bulk, not to the components, except in the treatment of thermodynamic equilibrium: just as in our DP. Also, equation (15) in the Biotic Pump paper can only be attributed to Landau and Lifschitz for what concerns the first "=" sign.

Dalton's law, as formulated in many books on atmospheric physics, states merely that the total pressure exerted by a mixture of gases equals the sum of the partial pressures of the components (which they would exert when they were alone, with the same concentration and temperature). This is a statement about local dependencies, which should not be generalized to saying that the partial pressure corresponds with the cumulative weight of the component above that height. If we measure for example the full

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air pressure at the surface, then we know the weight of the air column (not by Dalton's Law but by hydrostatics), but if we measure the partial pressure of a component we still know nothing about the contribution of the component to the weight of the column.

We do not know where the interpretation of Dalton's Law as given by MG (2007), and used in S3, comes from. We are aware of wider interpretations given in antique literature (see DP, pages 408-409) but these refer to the striving towards thermodynamic (component) equilibrium, and implicitly treat macroscopic flow as something which should automatically die out by dissipation, and hence can be "after all" neglected. As discussed in the DP, this line of reasoning is irrelevant for the physics of the open air, where there is an unceasing forcing of macroscopic flow, and it is understandable that it is no longer encountered in modern textbooks of meteorology. Below, the DP arguments will be defended against the objections made in S1-S10.

We note that 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".

1. Presence/absence of hydrostatic disequilibrium

It is admitted that "Obviously, on a global (or very large-scale) average the atmosphere is in **exact** hydrostatic equilibrium" (middle of S2). We agree with this statement, but we remark that this point is denied throughout the Biotic Pump paper (MG 2007). It follows from the Evaporative Force Theory that there should be practically everywhere a systematic **upward** vertical flow of considerable strength (see MG 2007, middle of the last paragraph of section 3.1). This would not sound suspect if it was about water vapor alone. The trouble is that the theory predicts vertical updraft of the whole air as such (MG 2007, Eq. 15), and this air consists mainly of dry air molecules. This should cause a problem with the mass balance at the surface.

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In our statements about approximate equilibrium (criticized in S2), we just follow the textbooks about the atmosphere. The statements concern values which are horizontally averaged to some extent. The reason for neglecting small scale up- and down-motions was that the same was done implicitly in the Biotic Pump paper, which also considers a horizontally-averaged w, while neglecting the implications of details on the finer scales. When defined this way, common vertical velocities of the air are of order 1 cm s^{-1}, upward and downward, and for most weather systems the assumption of hydrostatic equilibrium is a good approximation. Exceptions occur only in specific phenomena such as fronts, cumulonimbi, etc. It is well known that these phenomena are not at all ubiquitous.

2. Degree of deviation from the hydrostatic disequilibrium

Concerning the question what we meant by "approximate equilibrium" (S3), what we meant is that in Eq. (13) of the DP, the left hand side is usually small when compared to the individual terms on the right hand side: the deviation from bulk equilibrium is small when compared to the deviations for the separate components. This definition will suffice for the present purpose.

Very important is the remark (S3, 2nd paragraph) that due to the low water content in the atmosphere, any deviation of the water content from equilibrium will cause a negligible deviation from hydrostatic equilibrium, not exceeding a few percents. We believe that many people would agree with this on first hearing. However, a few percents of one atmosphere is still a few dozens of mbar. This is a big value if the difference applies over a short distance! We will return to this in the remarks about the 10th point.

3. Fluxes produced by the disequilibrium

On S4, last paragraph, and further on, our statements that (1) only molecular diffusion can restore component equilibrium, and (2) that this process is slow, are criticized.

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Here it is important to pay attention to the fact that we speak specifically about restoring component-equilibrium. In S4-S5 a lengthy argument is found that it is eddy diffusion, not molecular diffusion, which mixes the atmosphere. Moreover, this eddy diffusion is a rapid process. Indeed eddy diffusion is the main process for mixing. But the critical point is that mixing does not at all work to restore component-equilibrium. That would imply that light components would get a more vertically-stretched profile than heavy components. On the contrary, eddy diffusion tends to generate constant mixing rates, implying non-equilibrium for the components. This is an illustration of the claim of the DP that macroscopic motion (by which we understood all motion except molecular diffusion) does not care about restoring component-equilibrium.

It is also stated (S5, start of first full paragraph) that the vertical flux of latent heat is calculated from the "component-disequilibrium concentration gradient of water vapor" and the eddy diffusion coefficient. In this statement, the adjective "component-disequilibrium" had better be omitted. This being so, the statement is not relevant for the present discussion. The eddy diffusion is driven by shear and buoyancy, whereas water vapor is a passive scalar in this process (unless condensation occurs). So component-disequilibrium is not a driving force for turbulence.

4. Fluxes produced in relation to condensation

We agreed in the DP (see beginning of section 2.2) that the water vapor profile is vertically compressed, and that this is caused by condensation and precipitation. The remark (S6. Middle paragraph) that "this fact ... (is) entirely neglected by MDB in their critique" is not appropriate. Since we agree on this point, there was no reason to say much about this (the quantitative working out of this is not a topic of the DP). Our disagreement concerns the consequences, not the causes, of the non-equilibrium in the vapor profile.

5. Nature of constant mixing ratio for dry air

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See S4. The subject is beyond the scope of the DP. The constant mixing ratio of dry air components in the troposphere (and even above) is a trivial matter, which needs no new explanation: the (very weak) tendency to separate the density profiles of the components is counteracted by the (intermittent but strong) turnover on all scales. See e.g. Wallace and Hobbs, section 1.3.2.

6. Value of the scale height of atmospheric water vapor

See S6, this is also beyond the scope of the DP. We agree in general with what is said about condensation as a cause of component disequilibrium. However, we have one remark: Unlike what is suggested under point 6 of the Checklist, the predicted value for the scale height does not rely on the Biotic Pump Theory. The scaling height for (saturated) atmospheric water vapor can be found immediately from MG (2007), Equation (11), which is a classical result. Substituting observed values into the equation yields 2.4 km as scaling height, which is the result mentioned at the end of page 1020 of MG (2007).

7. Role of vertical temperature lapse rate for generation of dynamic air flows

This is also beyond the scope of the DP, see under point 4.

8. Continuity equation for atmospheric circulation

We assume that this is about the discussion starting at the last paragraph of S7. Our answer is combined with the answer to the following point.

9. Friction forces

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Concerning the discussion (starting in S7, first paragraph of section 3) about the statement in the DP that very high vertical velocities (50 m s^{-1}) are predicted over evaporating surfaces:

MG (2007) are somewhat incoherent on this point. It is true that a vertical velocity of only a few mm per second is reported on page 1026, first column, middle paragraph. However, the derivation of this result is based on hydrological balance considerations, and does not involve the evaporative force (as far as we can see, maybe we missed something here). The value of 50 m s^{-1} (DP) was taken from Equation (18) in MG (2007), and we concentrate here on that result.

The argument which leads to Eq. (18) in MG (2007) starts from Equations (15), (16) which describe the evaporative force as a function of height, based on theoretical profiles that are said to correspond well to observed mean profiles (see end of page 1020). Vertical integration of the force leads to a prediction of the vertical velocity at the end of the trajectory.

Our criticism concerns the prediction from the equations as such. It is obvious that vertical motion should be accompanied by horizontal motion along the surface, and that the latter motion will undergo substantial friction. However, the forcing predicted by Equations (15), (16) is not divided over the horizontal + vertical trajectory, but over the vertical trajectory alone, and it describes the acceleration of a parcel over the vertical trajectory. The argument in S8 amounts to saying that the vertical pressure disequilibrium should drop substantially due to the feedback from the horizontal friction (the remainder of the pressure difference is distributed over the horizontal part of the trajectory, like in the analogy of the train). But how can this be reconciled with the equations? 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.

In our opinion it should be clear that the smallness of the observed vertical acceler-

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ation requires a small disequilibrium over the vertical part of the trajectory, whereas the Biotic Pump Theory predicts a very large disequilibrium over the same trajectory. This paradox cannot be resolved by attributing the two sides of the same equation to different trajectories.

10. Nature of pressure differences observed (e.g.) in cyclones

In S9, the Comment points to the alleged fact that observed pressure differences for specific weather phenomena are comparable to observed differences of p_v (partial pressure of water vapor), and argues that this is proof of the evaporative-force-theory, as was also done in MG (2007). To this we answer that for a dynamic analysis, one should not just consider the differences but also the distances over which they occur (so that gradients can be estimated). When this is done, the agreement with observations to which S9 and MG (2007) point, appears to apply only for special cases (with either a large Δx or an exceptionally strong wind), whereas complete disagreement may be found for other cases (with strong variation of Δp_v over small Δx). For instance, over a heterogeneous landscape, a difference in water vapor content of Δq = 1 g kg^{-1} over Δx = 1 km is common. In measurements on both sides of a dry-wet transition, an increase of Δq = 1 g kg^{-1} is already common over Δx = 100 m. Knowing that 1 g kg^{-1} corresponds (at sea level) to Δp_v = 1.6 mbar, it follows by applying the relation $\Delta p = \Delta p_v$ (which should hold according to MG 2007), that the dry-wet transition should locally involve a horizontal pressure gradient of 16 mbar per km. This is much stronger than hurricane force, and entirely beyond what is observed under common conditions.

In summary, large horizontal Δp-differences are only observed over large distances, except for very violent flows; whereas large horizontal Δp_v-differences are commonly observed even over small distances. Thus, contrary to the Biotic Pump Theory, there is no straightforward connection between Δp and Δp_v . The reason for this is (in our opinion) that atmospheric dynamics work to reduce horizontal Δp-gradients to zero ("bulk

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equilibrium”), whereas horizontal Δp_v-gradients as such do not cause much immediate mechanical reaction (no “component equilibrium” at work). This is also the essence of the argument in the DP.

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