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Interactive Comment

Interactive comment on "Where do winds come from? A new theory on how water vapor condensation influences atmospheric pressure and dynamics" *by* A. M. Makarieva et al.

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Reply to Dr. George Bryan: Pinpointing where the potential energy of condensation was lost

We agree with Dr. George Bryan that considering other people's arguments takes time and we very much welcome his contribution to this discussion. In the context of his comment[1] (hereafter BC) and our work (M10), we shall here discuss our continued concerns with the physical fundamentals of the model of Bryan and Fritsch (2002) (BF02).

[1] http://www.atmos-chem-phys-discuss.net/10/C11194/2010/

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Interactive Discussion

Discussion Paper

1. Eq. 27 of BF02, which calculates condensation rate $\dot{r}_{cond}$, operates with the oversaturated mixing ratio $r_v > r_{vs}$. It thus contradicts the Clausius-Clapeyron law, according to which saturated concentration is the maximum possible one for any given status of the atmosphere (with the number and properties of condensation nuclei fixed). In our comment[2] (hereafter M10-C2) we indicated that Eq. 27 also violates the first law of thermodynamics. Dr. Bryan does not dispute this statement. He argues that the physics of Eq. 27 does not matter, as the equation is only used to generate a *guess* for $\dot{r}_{cond}$. However, in the presence of multiple solutions, the particular solution to which the system converges when numerically solved is not independent of the initial guess, so generally this argument is not valid.

If one accepts that Eq. 27 does not have physical meaning, but is just an equation that causes Eqs. (21)-(24) of BF02 to converge to some solution, one must note a further inconsistency. As pointed out by BF02, the iteration scheme involving Eq. 27 ends at some n-th step ($n = 4 - 5$) when the new value of potential temperature θ does not differ from the one obtained at the previous step (temperature T in Fig. 1 of M10-C2 tends to T'). This means that the new mixing ratio of vapor r_{vn} obtained using the n-th guess $\dot{r}_{cond\,n}$ is no longer oversaturated, $r_{vn} = r_{vsn}$. But at $r_{vn} \to r_{vsn}$ Eq. 27 of BF02 dictates that $\dot{r}_{cond\,n} \to 0$, because $\dot{r}_{cond\,n}$ is proportional to $r_{v\,n} - r_{vs\,n}$. This would mean that the iteration scheme of BF02 invariably yields a zero condensation rate, which does not make sense. Moreover, at $r_v \to r_{vs}$ we have $\dot{r}_{cond} \equiv dr_{vs}/dt \to (r_v - r_{vs})/dt$. This means that Eq. 27 has the form $\dot{r}_{cond} \equiv a\dot{r}_{cond}$ with $a \neq 1$. This again yields $\dot{r}_{cond} = 0$ both in Eq. 27 of BF02 and in Eq. 4 of M10-C2, where the first law of thermodynamics is respected. It follows from these considerations that Eq. 27 of BF02 is neither mathematically nor physically sound.

2. Erroneous formulation of hydrostatic equilibrium. In M10 (Section 4.2) as well as in our first comment[3] we showed that the dynamics of gas in the presence of con-

[2]http://www.atmos-chem-phys-discuss.net/10/C10926/2010/
[3]http://www.atmos-chem-phys-discuss.net/10/C10922/2010/

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densation is sensitive to the correct formulation of condensation rate. In our second comment (M10-C2), to which Dr. Bryan responds, we further elaborated that expressing condensation rate in terms of water vapor mixing ratio $\gamma_d \equiv r_v \equiv p_v/(p - p_v)$ rather than relative partial pressure $\gamma \equiv p_v/p$ (where p and p_v are air pressure and partial pressure of water vapor) corresponds to the case when dry air is in component equilibrium, $\partial p_d/\partial z = -\rho_d g$, where p_d and ρ_d are the partial pressure and mass density of dry air. With water vapor being out of component equilibrium, $\partial p_v/\partial z \gg -\rho_v g$, in this state the condensation induced pressure drop is located in the vertical dimension.

At $\gamma < 1$ the difference $\gamma - \gamma_d = \gamma_d^2/(1 + \gamma_d)$ is a higher order smallness. Therefore, replacing γ for γ_d in Eqs. (22), (23) (M10, p. 24025) yields a condensation rate S_d that only slightly differs from S (34) and a minor (of the order of γ) deviation from hydrostatic equilibrium. However, as shown in M10, this deviation when distributed in the horizontal dimension (this corresponds to using γ and not γ_d in Eqs. (22), (23) and (37) of M10) produces horizontal pressure gradients of observed and significant magnitude.

This sensitivity has never been discussed in the meteorological literature. Neither has the model of BF02 (or any other model) been tested against the corresponding sensitivity constraints. Our statement in M10-C2 (p. 10930) that *"These expressions were adopted for the BF02 and BR09 models without evaluation of their suitability for the studies in question"*, which Dr. Bryan characterises as *"untrue"* (BC, p. C11196) taking it out of context, refers to this fact. This is clear from the last but one paragraph on p. C10929 of M10-C2 that precedes the paragraph to which the quoted statement belongs.

The use of γ_d as the basis for the determination of condensation rate (which is the case in BF02) is not compatible with the hydrostatic equilibrium assumption. But if the hydrostatic equilibrium is represented incorrectly, this second error may mask the error in condensation rate formulation. Lacking a theory on condensation rate, one is unable to check the results of numerical simulations against robust physical estimates. Simulations are checked by comparison to other simulations, such that the errors have the

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opportunity to perpetuate from model to model. However, in less complicated (compared, e.g., to GCMs) models like that of BF02 the inconsistencies can be explicitly traced relatively easily.

In the BF02 model to compare the moist and dry simulations, a convectively neutral temperature gradient is specified in the initial state of the dry simulation. In this state the dry atmosphere should be in stable static equilibrium: all velocities are zero. This is intended to be compared to a moist simulation. The initial static equilibrium state of the moist atmosphere is chosen such that the liquid drops levitate in the motionless air (BF02, p. 2921). This corresponds to the hydrostatic equilibrium equation

$$dp/dz = -(\rho_v + \rho_l + \rho_d)g, \tag{1}$$

where ρ_v, ρ_l and ρ_d are densities of vapor, liquid and dry air, respectively. It is stated that the model *"has been carefully coded so that no vertical motions develop"*. The same assumption of droplets hanging in static air is present in the model of Rotunno and Emanuel (1987), Bryan and Rotunno (2009, p. 1772) (BR09), Lackmann and Yablonsky (2004) and presumably many others, where the hydrostatic equilibrium is similarly formulated with use of condensate density ρ_l. We emphasize that the problem is not about whether the precipitation fallout is or is not partially allowed, but with the incorrect formulation of hydrostatic equilibrium *per se*. In BR09, for example, the precipitation fallout is allowed when ρ_l exceeds 1 g m^{-3}, while all liquid at smaller density is allowed to levitate in the static atmosphere.

Eq. (1) represents a gross violation and fundamental misunderstanding of the physics of gaseous state. According to the equation of state of ideal gas, gas pressure p depends on the *number* of gas particles, but not on their size or mass; $p = NRT$, where $N \equiv \rho/M$, M is molar mass of the gas, R is the molar (universal) gas constant that is independent of particle mass (including the mass of hydrometeors–droplets). The presence in the gas of "Brownian" (macroscopic) particles can be included into (1), but molar density N_l of such particles is thousands of times smaller than the molar

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density N_d of dry air and N_v of water vapor: $p = (N_d + N_v)RT$, $N_l \ll N_v$. Therefore, any distribution of gas pressure, including the hydrostatic one, does not depend on N_l and $\rho_l = N_l M_l$, although mass density of liquid can theoretically be (as is the case in the BF02 model) of the order of mass density of vapor, $\rho_l \sim \rho_v$. Partial pressure p_l of macroscopic Brownian particles is practically zero, $p_l = 0$, and its spatial gradient is zero as well, $\partial p_l/\partial z = 0 \neq -\rho_l g$.

Considering the initial state of the moist simulation of BR02, it is possible to exactly pinpoint where the potential energy of condensation has been ignored. The correct equilbrium condition for a static atmosphere is

$$dp/dz = -(\rho_v + \rho_d)g = \rho g, \quad (2)$$

where ρ is the density of the *gas*. The imaginary initial state of the atmosphere which is static but has a certain amount of liquid hanging in the air, such that $dp/dz = -(\rho_v + \rho_l + \rho_d)g$, is physically meaningless and does not correspond to either stable or unstable equilibrium. It is not an equilibrium at all. In this state the gas pressure is highly non-hydrostatic and thus a non-equilibrium pressure gradient and an associated store of potential energy exist. Also, the droplets levitating in the air possess potential energy in the gravity field of Earth. The dynamic processes associated with the release of this energy (with drops falling and air expanding upwards) are ignored and artificially eliminated from the model of BF02.

In contrast, comparing the dry A and moist B atmospheric columns without any unphysical assumptions (Section 3.3 in M10) we estimate the amount of this energy and show that it is significant in the atmospheric context.

3. Unphysical assumptions yield unphysical conclusions. The fact that a numerical model which accurately writes out the equations of hydrodynamics can be coded such that it violates Newton's laws and the physics of gaseous state and makes liquid drops levitate in motionless air, illustrates the obvious point that numerical simulations do not possess any independent value and cannot be used to estimate the significance

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of a new physical effect. Simulations should be at best viewed as illustrative tools to teach physics that has been understood *a priori* and independently tested, either from observations or by being deduced from fundamental laws of physics, or both.

Dr. Bryan comments that the simulations of BF02 should be correct because the moist simulation is similar to the dry simulation: *"The dynamical similarity of the dry (without condensation) and moist (with condensation) simulations, under the constraints explained therein, demonstrates the accuracy of the method to determine condensation rate."* This links with another statement in BF02 (p. 2927) that *"one might wonder whether these results only come about due to the unphysical initial environment that must be used to obtain the benchmark solution. Despite the unphysical aspects, this design is required in order for a benchmark solution to be obtained – without this setup, a "correct" solution would not be known, and it would be impossible to objectively evaluate the various model configurations."* These considerations lead one to conclude that the *unphysical aspects* of the BF02 simulations are necessary to obtain the *correct* result, with the criterion of correctness being that the moist simulation is equivalent to the dry one. Obviously, such a problem setting ignores the potential energy of condensation (the focus of M10) *by formulation.*

We agree with Bryan and Fritsch (2002, p. 2921) that *"a moist atmosphere is not as simple"*. But, in contrast to Bryan and Fritsch (2002), we are convinced that no correct conclusions on the magnitude of the condensation effects could have been obtained from *unphysical assumptions*, in particular, from those underlying the numerical simulations of BF02, Bryan and Rotunno (2009) and Rotunno and Emanuel (1987). Before setting out to simulate things, the misunderstandings surrounding the representation of moist processes in the existing models must be eliminated. This can only be done in the context of a basic physical analysis of the key dynamical processes and parameters associated with condensation. In this context we consider our contribution and the resulting discussion useful and timely.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24015, 2010.