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Atmospheric Chemistry and Physics Discussions

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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We thank Dr. Gary Lackmann for his interest in our work, for his comment in the ACPD discussion[1] and his subsequent email discussion with the authors. Our shared correspondence confirms Dr. Lackmann and our team's mutual respect of each other's work. We share many concerns. Notably both parties are keen to estimate the impact of the condensational vapor sink on atmospheric dynamics and welcome the increased attention to this topic. Here we would like to share with readers a few points raised by Dr. Lackmann that initially confused us but were subsequently clarified through direct

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[1] http://www.atmos-chem-phys-discuss.net/10/C10973/2010/

discussions. We then focus on our points of disagreement: (1) Dr. Lackmann considers that our theory should not be called new given previous research on this topic and (2) Dr. Lackmann considers that we have overrated the magnitude of the effect.

1 Clarifying misunderstandings

We were confused when Dr. Lackmann commented that we had ignored his previous work along with that of several other authors. Because of that we wrote to him directly. The following is a verbatim extract:

"We are uncertain why you imply that. We cite your work directly. We call it "important" and quote you, see page 24018 line 16: "Lackmann and Yablonsky (2004) investigated the precipitation mass sink for the case of Hurricane Lili (2002) and made an important observation that "the amount of atmospheric mass removed via precipitation exceeded that needed to explain the model sea level pressure decrease". We also cite five of the six papers you suggest we have omitted. The one we miss Ooyama 1990 is because we cite another paper by the same author, Ooyama 2001."

Dr. Lackmann responded. He agreed that there was a confusion noting that he was referring to the discussion (not the manuscript) and appreciated that the key references were present.

2 Past work

2.1 The existence of a precipitation mass sink

Lackmann and Yablonsky (2004) and Yablonsky (2004)[2] reviewed ideas concerning the precipitation mass sink. One of the earliest papers cited by Lackmann and Yablonsky

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[2] http://gsosun1.gso.uri.edu/∼richard/etd.pdf

(2004) is the paper of Palmén and Riehl (1957), who are credited for a somewhat indirect acknowledgement of the precipitation mass sink (the authors noted that the hurricane must be considered as an open system in terms of its mass balance). Several other authors are credited for recognizing the sink directly. In our work, we definitely *do not* claim to be the first to state that the precipitation mass sink exists and quote several prior ideas and developments.

2.2 Mass sink and air motion: Barycentric velocity

The next idea that appeared in the literature was that the precipitation mass sink should induce some motion: when vapor disappears, to keep the circulation (quasi-)stationary, some air flow must be induced to replenish the local moisture store. Our own readings lead us to think that one of the first scientists to mention this explicitly was not Hansen et al. (1983) (the earliest paper in the review of Yablonsky (2004)) but that earlier accounts exist, e.g. Lorenz (1967, p. 49, Eq. 86).

We note that Lorenz expressed mixed, even contradictory, views concerning motion induced by a condensation induced mass sink/source. On the one hand, he provided the first (ever?) numerical estimate of barycentric velocity associated with the mass sink (see M10, p. 24036). Lorenz wrote (p. 51, our emphasis): "It follows that [due to the mass sink] there is a net flow of air across each latitude, equal in mass to the net flow of atmospheric water. As much mass flows across latitude 40 °S, for example, as would *if there were a uniform north wind of 0.3 cm/sec at all levels*."

On the other hand, Lorenz wrote (p. 51, our emphasis): "It is to be stressed that the evaporation and precipitation require a transport of water only in the sense that if they exist, the transport must exist. They cannot be regarded as *the cause of the transport*. If the circulation were unable to carry water in the equatorial zone, for example, there would simply be less precipitation or more evaporation there."

In later work it was realized that there *can* be a positive feedback between a precipitation mass sink and wind formation (for further discussion see Lackmann and Yablonsky (2004), Yablonsky (2004) and our paper (hereafter M10), p. 24018). However, this research was limited in scope and scale and lacked a coherent quantitative picture of these processes in atmospheric circulation. As is clear from the reactions[3] to our previous work (Makarieva et al., 2008), the broader meteorological community seems largely unaware of these studies.

2.3 The study of Lackmann and Yablonsky (2004)

Lackmann and Yablonsky (2004) moved beyond the mass balance considerations to address dynamics. They realized that a mass sink (MS) must bring about an extra pressure gradient force (PGF) and that the extra motion caused by the mass sink is due to this MS-PGF (Yablonsky, 2004). However, Lackmann and Yablonsky (2004) did not develop a physical theory to describe this force and were therefore unable to develop the analytical tools needed to check the results they obtained from a numerical hurricane model against independent physical considerations. The differences in maximum velocities between a "control" hurricane with maximum wind speed of about 60 m/s (and no mass sink) and a hurricane with a mass sink amounted to up to 7 m/s. This 10% rise in velocity translates to about 30% rise in hurricane intensity (as the power is proportional to the cube of air velocity).

Lackmann and Yablonsky (2004) did not answer the question of where this extra power could come from. Lacking a theory, it is impossible to investigate whether the numerical model correctly conserved this additional energy, whether or not this energy is spurious or, conversely, whether or not one has lost a certain part of it.

We note in passing a disagreement between the results of Lackmann and Yablonsky (2004) and those of Bryan and Rotunno (2009). This disagreement is not discussed by Bryan and Rotunno (2009). In Fig. 10 of Bryan and Rotunno (2009) maximum hurricane velocity is plotted for model runs with and without a mass sink versus terminal drop velocity V_t. For realistic terminal velocities (e.g., 7 m/s) the difference is practically

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[3] http://www.atmos-chem-phys-discuss.net/8/17423/2008/acpd-8-17423-2008-discussion.html

non-existent (while the model of Lackmann and Yablonsky (2004) produces a 10% rise in maximum velocity). At larger V_t the model of Bryan and Rotunno (2009) without a mass sink produces even greater maximum wind speeds.

Without a theory on how vapor removal influences dynamics, the results of and discrepancy between various numerical models are ambiguous and difficult to interpret. Our approach, by focusing on explicit and tractable physical principles and the relationships we derive from these, is able to avoid such pitfalls.

3 What is new in M10

Our analyses indicate that Lackmann and Yablonsky (2004) are incorrect to suggest that the pressure gradient force is a relatively minor aspect of atmospheric dynamics. In our paper (Section 4.2) and subsequent comments[4] we showed that formulation of condensation rate using mixing ratio $q \equiv p/(p - p_v)$ (as is the case in the work of Lackmann and Yablonsky (2004) and others) corresponds to a strong deviation of the vertical distribution of moist air from hydrostatic equilibrium. In numerical models where the vapor sink is fully neglected the mathematical treatment of condensation is such that condensation of any amount of vapor is formally accompanied by an instantaneous addition of an equal amount of dry air (Thuburn, 2008), to satisfy hydrostatic equilibrium in spite of condensation. Hydrostatic equilibrium in such models was correctly represented as

$$\frac{\partial p}{\partial z} = -\rho g, \tag{1}$$

where p and ρ are the pressure and density of the air, $p = p_d + p_v$, $\rho = \rho_d + \rho_v$ (d and v stand for dry air and vapor, respectively).

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[4] http://www.atmos-chem-phys-discuss.net/10/C10922/2010/ http://www.atmos-chem-phys-discuss.net/10/C12836/2011/

Earlier we discussed[5] that under real conditions such an addition of dry air cannot occur without work being performed on the considered air volume. By neglecting this work these models overlooked the importance of condensation-induced dynamics.

Another error occurs in some models that explicitly attempt to account for the mass sink, e.g., Lackmann and Yablonsky (2004), Rotunno and Emanuel (1987), Bryan and Fritsch (2002), Bryan and Rotunno (2009). Here the correct hydrostatic equilibrium equation (1) was replaced with a different one:

$$\frac{\partial p}{\partial z} = -\rho_t g, \tag{2}$$

where ρ_t is the total density of gas plus liquid, $\rho_t \equiv \rho_d + \rho_v + \rho_l$. From (2) we have

$$p(z) = \int_0^\infty \rho_t(z) g dz - \int_0^z \rho_t(z) g dz = p_s - \int_0^z \rho_t(z) g dz, \tag{3}$$

where $p_s \equiv p(0)$ is surface pressure.

This is a fundamental error equivalent to violation of the ideal gas law. From Eq. (2) it follows that under the modified "hydrostatic equilibrium" condition, the surface pressure p_s remains unchanged as long as the total amount of matter in the column is unchanged, $p_s = \int_0^\infty \rho_t g dz$. Suppose we initially have an atmosphere in hydrostatic equilibrium with $\rho_l = 0$ (no liquid): $\rho_{t1}(z) = \rho_d + \rho_{v1}$. Then we replace an air volume at height z by an air volume of the same total density but containing liquid instead of vapor: $\rho_{t2}(z) = \rho_{t1}(z) = \rho_d + \rho_{l2}$, $\rho_{l2} = \rho_{v1}$.

The initial distribution $\rho_t(z)$ has not changed, which, according to (2), means that surface pressure has not changed either. From this and (3) we conclude that pressure at height z has not changed upon the replacement of vapor by liquid: $p_1(z) = p_2(z)$.

However, the ideal gas law dictates that gas pressure is proportional to temperature and the number of *particles* (molecules) N per unit volume, $p = NRT$. Substitution

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

of vapor by liquid droplets has reduced the total number of particles by the number of vapor molecules. (Liquid does not make a contribution to the ideal gas pressure!)

In the result, pressure $p(z)$ in the air column has reduced by p_v from $p_1(z) = p_d + p_v$ to $p_2(z) = p_d < p_1(z)$. The hydrostatic equilibrium has been disturbed *immediately* upon the replacement of vapor by liquid (which happens throughout the column during condensation in the moist ascending air), $p_2(z) \neq p_1(z)$. The resulting pressure gradient (which is key to condensation-induced dynamics) and its associated potential energy have been neglected in all models (hydrostatic as well as non-hydrostatic) where the hydrostatic equilibrium condition was formulated as in Eq. (2). Eq. (2) is equivalent to the statement that the conversion of vapor to liquid does not change the gas pressure. This is physically incorrect.

We also draw attention to the fact p in hydrodynamics is a scalar, not a vector, it does not have a direction. Expressions like "accounting for the *downward pressure* of liquid", though frequently encountered, are unphysical.

The fundamental error of Eq. (2) interpreted as hydrostatic equilibrium makes it clear why the term "mass sink" should be avoided. The physical process behind the origin of the condensational pressure gradient force is the loss of molar density N that occurs *immediately* upon condensation (vapor loss) rather than when the liquid precipitates (mass loss). In the context of condensation-induced dynamics it is natural therefore to speak of a *vapor* sink.

In our work we consider the basic physical principles that underlie the existence of the vapor sink in the atmosphere. We discuss in detail the implications of the hydrostatic equilibrium assumption for the formulation of condensation rate. We estimate the global mean power of the potential energy release associated with the vapor sink. We derive a theoretical expression for the pressure gradient force associated with this vapor sink. Using the observed fundamental atmospheric constants we show that our expressions and the pressure gradient force they describe are relevant in atmospheric context, both in large-scale circulation patterns like Hadley cells and in hurricanes. To our knowledge, none of these efforts have been previously undertaken. We hope that our analyses will stimulate other researchers to examine and test our conclusions.

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