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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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Recommendation: Reject

The authors make an extraordinary claim that a term that is traditionally considered to be small, to the point that it is sometimes neglected in atmospheric models and, even when not neglected, rarely commented on, is in fact dominant in driving atmospheric circulations. The effect concerned is that of the mass sink associated with condensation. This term is of first-order importance in some planetary atmospheres, such as Mars, where the total mass of the atmosphere has a substantial seasonal cycle, but for Earth the standard perspective is that the heat release associated with condensation

dominates over the effect of the mass loss. A claim of this sort naturally has to pass a high bar to be publishable, given the accumulated evidence, implicit as well as explicit, that argues against it. I am afraid that this paper does not approach the level required. I have done my best to keep an open mind, but do not see any cogent arguments that overturn the conventional wisdom. I do applaud the authors for questioning the foundations of our understanding of the atmosphere and provide some unsolicited advice on how the authors might proceed to clarify some of these issues. There is a need for some clarification.

The paper starts with a discussion of the moist adiabat. The key conclusion is that condensation is necessarily "accompanied by reduced air pressure". My understanding is that these considerations have no direct connection to the main claim of the paper, since they relate to the changes in pressure following an air parcel. (Most of us would turn this relation around, leaving the very familiar result that when a saturated parcel moves to lower pressure there will be condensation.) To make a connection with the pressure gradients that drive horizontal winds, one has to talk about pressure differences at fixed height, or something more or less equivalent. I am afraid that I do not see the connection here.

The main argument in the paper, as some of the online discussion also makes apparent, is that involving Eqs 32-37. Eqs 32 and 33 are simply conservation of mass of dry air and water vapor. From 32 and 33 one can directly derive the result that

$$S = N_D v \cdot \nabla(N_V/N_D) \quad (1)$$

In the steady flow being considered, there are regions in which there is condensation and regions in which there is no condensation. In the latter the mixing ratio, N_V/N_D, is conserved along a streamline. If we further assume that in the condensing region the streamlines are vertical, then one obtains Eq. 34, but with N_D rather than N in the expression for S, which I assume is a detail. There is no other physics in this expression. In particular, there is no thermodynamics, which would come in at the point that one determines which regions are condensing and which are not, or when estimating the mixing ratio gradient by assuming that the atmosphere is on a moist adiabat in the region of upward motion.

But I cannot follow the derivation of (35) or (37). As noted by Nick Stokes in his comments, (35) is strange, since it reduces to $S = u\partial N/\partial x$ when combined with (32) and (33), which makes no obvious sense. Up to this point, there is no reference to pressure, but only to density. Pressure now enters through the equation of state and the assumption that temperatures are uniform in the horizontal. There might be some value in this approximation for the mean tropical circulation, where the *weak temperature gradient approximation* is a common starting point for discussing dynamics. (Googling this expression provides a list of relevant papers). In any case, this is academic since I do not understand the expression (35) leading to this, so I can make no sense of the resulting expression for the pressure gradient.

Following this argument there is a discussion of the efficiency of the atmospheric circulation. The authors cite Pauluis and Held on the entropy budget of the atmosphere, but it is a Pauluis-Balaji-Held J. Atmos. Sci. April 2000 paper that is closest to the discussion here. This paper in turn was motivated by some comments in an earlier paper of Emanuel. The atmosphere does a significant amount of work against gravity in lifting water to balance the condensate that falls through the atmosphere. This is interesting, and it is related to condensation certainly, but I doubt that it is very directly related to the strength of the large-scale circulation. From an entropy budget perspective, this sink of energy does reduce the amount of energy that can flow through the kinetic energy of the circulation to be dissipated, but in practice I think what happens is that it primarily reduces the amount of energy flowing through cloud-scale turbulence rather than the large-scale flow. This is a confusing issue, I admit, and I personally suspect that this kind of reasoning is more promising than others in this paper, but the connection to large-scale flows remains obscure to me.

There is also a discussion of the fact that there are regions in the tropics in which the

precipitation is a lot larger than the evaporation, due to convergence of water. This is a familiar fact, discussed in many textbooks. The mean precipitation distribution in the tropics bears little resemblance to the evaporation distribution. How this simple fact is related to the main topic of the paper is not explained.

And there is an attempt to argue that the smallness of the mass flux associated with the precipitation sink/evaporation source, as compared to the total mass flux, in the Hadley cell for example, is not a good measure of its importance. My hunch is that it might actually be a pretty good measure. In some idealized axisymmetic Hadley cell models, such as that of M. Sato (J. Atmos. Sci., 1994) and Fang and Ting, (J. Atmos. Sci, May 1996), my guess is that there would actually be compensation, so the total mass flux would change less than the size of the condensation mass flux. But I don't claim to having thought this through. I don't see how one can argue one way or the other without an explicit or conceptual model of the Hadley cell to inform the discussion.

There is some discussion of this topic in the hurricane literature, as exemplified in Lachmann and Yablonsky, J. Atmos. Sci., July 2004) and as discussed by Lachmann in his interactive comment. The conclusion there is that this is non-negligble but certainly not a dominant effect.

Bannon and Chagnon have written a series of papers on "hydrostatic adjustment" to heat sources, providing another possible starting point for an analysis of this effect. They start with an atmosphere at rest and instantaneously add a heat source of finite duration. In the simplest case the heat source has some vertical structure but no horizontal structure and is a delta-function in time. The first paper in this series, Bannon (J. Atmos. Sci, May 1995), describes in some detail how the atmosphere comes back into hydrostatic balance. In this final balanced state the pressure perturbation is maximum at the top of the heat source, with magnitude $\approx Q(R/c_p)(h/H)$, where Q is the time-integrated heating in J/m^3, h is the depth of the heated region which I am assuming here is small compared to the scale height $H = RT/g$ for simplicity (eq. 4.8 in Bannon). The response vanishes identically below the heat source. Spengler et al

(J. Atmos. Sci., Feb 2011) extend this analysis to include the mass sink that would be present if this heating were due to condensation. (The authors of the present paper could not be expected to be aware of Spengler et al before submitting their paper, of course). Putting aside the details of the adjustment process, the final hydrostatic state is modified in the expected way: superposed on the response to the heating, one now has a pressure response below the level of condensation which is simply the weight of the fluid lost, Qhg/L where L is the latent heat of vaporization. The ratio of the condensation response to the heating response is $c_pT/L \approx 1/10$. The fact that there is no response to the heating below the source in this idealized context can be misinterpreted – if one considered a spatially localized source, as in some of the other papers in this series, a circulation is generated that will induce pressure gradients below the source as well. In any case, careful analysis of some simple initial value problems strikes me as another way to proceed to clarify this issue.

Some other unsolicited advice on writing a paper that would be easier to read (and review).

–If the focus is on atmospheric phenomena rather than fluid dynamics per se, choose one phenomenon to discuss and take models of that phenomenon in the literature seriously. For example, if the phenomenon is hurricanes, one has a choice of working in an idealized framework or with more realistic models. If the idealized perspective is chosen, Emanuel's steady state balanced vortex may be a useful starting point. if realistic simulation is the goal, you may need to construct a model of your own, given your skepticism concerning existing hurricane models as expressed in the interactive discussion. In either case, if one ignores the mass sink associated with condensation one is still left with a mathematically coherent set of equations. Adding the mass sink alters these equations. So you should be able to compare the strengths of circulations with and without the mass sink. This is the kind of estimate readers are looking for.

– Focus on one argument and try to make that as convincing as possible, rather than jumping from one line of argument to another.

– Avoid trying to support your argument by appealing to authority; the quotes from Brunt and Lorenz are, at best, a distraction.

Sincerely, Isaac Held