

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.

J. Curry (Referee)

judith.curry@eas.gatech.edu

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This paper (M10) has been discussed extensively in the blogosphere, with participation of the paper co-authors (including Makarieva):

<http://noconsensus.wordpress.com/2010/10/15/where-do-winds-come-from/>

<http://noconsensus.wordpress.com/2010/10/19/momentary-lapse-of-reason/>

<http://noconsensus.wordpress.com/2010/10/21/456345/>

<http://noconsensus.wordpress.com/2010/10/26/weight-of-water-and-wind-hurricane-pros-weigh-in/>

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<http://judithcurry.com/2010/10/23/water-vapor-mischief/>

<http://rankexploits.com/musings/2010/flow-system-to-for-further-discussion-of-eqn-34/>

<http://rankexploits.com/musings/2010/equation-34-control-volume-symmetry/>

The authors are to be commended for their willingness to engage in extensive public discussion of their paper.

The paper is interesting and provocative, and these ideas should be developed. That said, I note that it is typically difficult for an “outsider” to get a paper published if previous research isn’t adequately referenced, non-standard notation is used, and the importance and significance of the research is overstated. M10 suffers from all of these problems, each of which should be easily fixed (some specific suggestions in this regard are included in the short comments of Stokes and Lackmann.) By engaging with some atmospheric scientists in the blogosphere, the authors are making progress in addressing these issues.

There are four major issues that need to be fixed before the paper is accepted for publication:

1. Address the issues surrounding the derivation and meaning of eq (34), as per Stokes’ comments. As I understand it, this equation is valid only for saturated air. If this is correct, this assumption needs to be made explicit. It seems that if this is correct, there are some inconsistencies with the interpretation of subsequent equations. To address this problem (which was not adequately addressed in Makarieva’s replies to Stokes), the authors should use standard notation and variables (e.g. Curry and Webster Thermodynamics of Atmospheres and Oceans), clearly state their assumptions, and show the derivation in some detail (perhaps in an appendix if it is lengthy). Exactly what is Nv and what is the basis for assuming that S is linear over Nv ?
2. While I think that the mechanism described by M10 is correct and real, the issue

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is its significance in the atmosphere. It is not clear to what extent this mechanism “matters;” their thermodynamic analysis is insufficient to demonstrate the relative magnitude of this effect. This needs to be demonstrated either in the context of a more comprehensive scale analysis that includes the Navier Stokes equations or numerical model simulations using mesoscale or weather or climate models. The time and space scales of the adjustment to the small mass disturbances engendered by condensation, and how the adjustments occur in the vertical or horizontal direction, can only be determined in the context of a simulation.

3. The Hadley circulation argument is unconvincing, and it needs to be placed in context of other ideas about the Hadley circulation.
4. I disagree with the authors regarding evaporation vs. condensation. They identify “salient differences” between them which in fact do not exist. Evaporation is not a surface specific process. When a cloud forms in the atmosphere, the condensed water has one of two fates: fallout in the form of precipitation or evaporation. The precipitation efficiency of clouds is rather low, much less than 10%. So most of the condensed water in the atmosphere eventually evaporates in the atmosphere. But I don’t see that this has much impact on their overall argument.

With attention to each of the above issues, a revised paper may be acceptable for publication.

Judith Curry

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

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