Atmos. Chem. Phys. Discuss., 10, C15085–C15093, 2011
www.atmos-chem-phys-discuss.net/10/C15085/2011/


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.

**A. M. Makarieva et al.**

ammakarieva@gmail.com

Received and published: 26 April 2011

Aside from our technical response to Dr. Held[1] we also wish to discuss the criteria he uses to assess our manuscript. All theories should be subjected to similar standards of scrutiny regardless of whether they conform to conventional thinking or not. We find many examples to show that much of the argument against our theory and in favour of conventional ideas appears based on misconceptions. We conclude with an appeal concerning the wider practical importance of our ideas.

[1]http://www.atmos-chem-phys-discuss.net/10/C14894/2011/



## 1 High bar for unconventional findings

Dr. Held starts his review with the recommendation to reject our manuscript. He explains that a study that goes against *the standard perspective* or aims *to overturn the conventional wisdom* has to *pass a high bar*.

As science students we are taught about the sins of confirmation bias – that is the need not to allow our preconceptions and judgements to cloud our objectivity. We should not reject ideas, or data, that fail to conform to our expectations any more readily than those we agree with. We all agree with that as an abstract idea though it can be hard to achieve in practice. Biases are often hard to perceive for those who hold them especially if they are pervasive. But we should strive for objectivity – when biases are identified we must do what we can to remove or minimise them.

Dr. Held believes our theory has to pass a high bar because of *the accumulated evidence, implicit as well as explicit, that argues against it.* Dr. Held does not spell out any evidence at all (so it all remains implicit in this case), but it is presumably a statement of his confidence that climate modelers already have a handle on the basic principles on how the World's climate works. Clearly we disagree with this. If there were such evidence the scientific approach requires that it is presented to us so that we can address it explicitly. When it is not specified we can perhaps be forgiven for believing that the bar has been placed infinitely high: a serious case of "confirmation bias". Such biases must be questioned by all of us with a training in science (regardless of views concerning our theory).

Dr. Held concludes his review by recommending that we should avoid appealing to authorities. He is referring to our selected quotes by Brunt and Lorenz. Normally we would agree that such quotes are out of place in a physics paper. However, in this case we made an explicit choice as we felt that we needed them. Our appeals to authority are intended to counter the (familiar) arguments based on implicit evidence: that our theory is not needed, or wrong, or the bar should be raised just because climate scientists already have all the theory they need and there are no major gaps and flaws. Our point is that climate theory is full of *basic* questions and doubts and many respected authorities say so. For us a particular value of the opinions of Brunt and Lorenz lies in the fact that they were formulated prior to the era of computer modelling. Nowadays we find *simulation* is too often allowed to replace physical understanding[2]. Targeted coding of numerical models can successfully simulate numerous patterns, including many prohibited by the laws of nature[3].

## 2 Low bar for "conventional" findings?

A higher bar for unconventional ideas automatically implies a lower bar for conventional ones. Introducing a positive feedback – relating the height of the higher bar to the number of studies that have passed the lower bar – in time if this continues a once vibrating scientific community can be trapped in dogma. An objective culture might be eroded by overconfidence in its own monolithic vision. This may be especially likely in climate sciences due to the considerable effort invested in presenting a confident unified front to the outside world.

Attitudes to vapor sink dynamics illustrate our concerns. Water vapor condensation is a ubiquitous process in day-to-day weather and climate processes. Nonetheless the nature of the associated pressure gradients *has never received a theoretical investigation.* These gradients are seldom mentioned (even to discount them) in the reviews of the general circulation theory. We find that in the microphysical studies of the phase

[2]One cannot expect people to explicitly admit such things too often. Dr. Jonathan Vigh (2006, http://ams.confex.com/ams/pdfpapers/108319.pdf, p. 4), when discussing why the physical causes of hurricane eye formation remain obscure and little studied in theory, mentioned that it was probably because the eye can be easily *simulated* in models. Indeed, in modern modelling practice one does not necessarily need to understand what one can simulate.

[3]http://www.atmos-chem-phys-discuss.net/10/C12008/2011/ p. C12011



transitions of water vapor the physical causes of condensation are usually neglected. E.g., in the study of Vesala et al. (1997), characterized as *a clear and accurate theoretical description* of condensation by Kolb et al. (2010), it is emphasized that any global processes that induce the conditions required for condensation to occur are *not* considered and that such conditions are assumed to be *predetermined.* Most existing accounts of the vapor sink, like the study of Lackmann and Yablonsky (2004), are based on empirically fitted numerical models and do not provide or allow for a transparent physical interpretation. These simulation studies differ in their formulations, results and outcomes. But we see that in climate science the outputs of models of varying degree of complexity can contradict each other without triggering any discussion or explanation[4].

The few studies designed to investigate the vapor sink appear from our evaluations to be based on unphysical assumptions, like the gravity defying levitation of liquid droplets in motionless air[5] in the study of Bryan and Fritsch (2002) or atmospheric warming by spontaneous drying in the study of Spengler et al. (2011), as endorsed by Dr. Held in his review[6]. Nevertheless, because these flawed results have been "conventional" enough to pass over the bar, they are considered *benchmarks* or *physicality tests* for getting other ideas over the same bar.

A paradox, considering the common claims of general consensus, is that we find that even basic questions are controversial in climate science circles. This is a result of limited attention and investigation. These are not idle claims. For example, does condensation in a volume of atmosphere (near-) immediately lower pressure at the surface or must one wait until the droplets precipitate out to the surface – views among climate scientists differ on this basic question with a majority apparently believing the latter to

[4]http://www.atmos-chem-phys-discuss.net/10/C13260/2011/ pp. C13263-C12265

[5]http://www.atmos-chem-phys-discuss.net/10/C12008/2011/ pp. C12011-C12013

[6]http://www.atmos-chem-phys-discuss.net/10/C14894/2011/ pp. C14895-C14897

be true[7]. Recently *a modelling study* (Spengler et al., 2011) was required to illustrate that, since gas obeys a different equation of state than liquid, changes of air pressure and fall out of droplets occur on different time scales. Indeed, as condensation occurs the air pressure is lowered at the surface near instantaneously (at the time scale defined by the speed of sound and height of condensation). These points are all obvious enough when viewed from the perspective of basic physics.

There are deeper misunderstandings, like confusing *heat* and *work*. Any spatially non-uniform warming of the atmosphere can be brought back to equilibrium by *heat* transfer (e.g., by radiation to space) without any *work* performed or dynamic flow generated. In contrast, to compensate for gas removal from an atmospheric volume, mass can be only resupplied there by performing *work* on that volume by way of its compression. This fundamental difference between pressure gradients associated with heat versus gas removal is ignored. E.g., Spengler et al. (2011, p. 358) believe that as heating is offset by diabatic cooling *there would be analogous compensation for drying* (=vapor sink). This confusion between heat and work is perhaps a key (pervasive) misconception about vapor sink dynamics.

In summary, despite some claims by climate modelers at GFDL[8] that the vapor sink is comprehensively included in current global circulation models, it is unclear how this could be achieved given the poor state of either theory or empirical understanding.

## 3 Science: One bar for all

In our paper, the pressure gradient due to the inherent spatial inhomogeneity of the condensation/evaporation process is derived for the first time in the scientific literature.

[7] See, for example, comments by Dr. Gavin Schmidt at http://noconsensus.wordpress.com/2010/10/19/momentary-lapse-of-reason/#comment-39079 and http://noconsensus.wordpress.com/2010/10/19/momentary-lapse-of-reason/#comment-39087 and by Dr. Roger Pielke Sr. at http://wattsupwiththat.com/2011/01/21/an-appeal-to-the-climate-science-blogosphere/#comment-588301

[8] http://judithcurry.com/2010/10/23/water-vapor-mischief/#comment-10878



It is further shown that in the atmospheric context this gradient is of sufficient magnitude to be considered as a major driver of atmospheric dynamics.

The criteria to be applied in assessing the value of new scientific propositions are reasonably simple. Is our proposition consistent in terms of its form and logic with basic physical principles? We say "yes". Does it make predictions that can be tested? Again we say "yes". Has anyone shown any error in the mathematical or physical reasoning? Here the answer is, as far as we can see, "no" – there are valid differences of opinion, and no shortage of misunderstandings, but no-one has shown an error. We thus see no valid scientific argument for a "higher bar" and none has been presented. A study should not be rejected (i.e. given a "high bar") because its results are surprising or because people find the equations hard to follow or because the reviewer believes it is wrong but can present no evidence – these are not the criteria we should accept.

The validity of any new proposition in science is ultimately tested by empirical evidence. In climate science the situation is peculiar in that the objects of interest often exist in a single number – e.g., the Hadley circulation – and the underlying systems cannot be modified at the discretion of the investigator. Unlike chemists or particle physicists, climate scientists cannot easily set up and perform experiments on many of the phenomena they study (see, e.g., Held (2005) for a discussion). In such a situation the first test a new theory (in our case, the proposition that atmospheric dynamics is driven by the vapor sink) can be expected to pass is to explain the existing evidence better (i.e., with fewer empirical parameterizations and on a unified physical basis) than the old one (in our case, the proposition that winds are driven by heat). In our present work and other recent papers we offer a number of examples indicating that this can be achieved (e.g., Makarieva and Gorshkov, 2010, 2011; Makarieva et al., 2009, 2010, 2011). One cannot expect a few people to fully elaborate a new theory and analyze all available evidence in one paper. This is seldom how science progresses. We believe that publication of our findings in the meteorological literature will encourage other climate students to investigate the problems, test our propositions and reach their own conclusions.

We also see an opportunity to study some aspects of condensation dynamics in the laboratory. Condensing vapor allows for a one-dimensional flow between warm and cold liquid surfaces. Vapor arises by evaporation at the warmer surface, flows towards the colder surface and "disappears" (i.e. condenses) there. Such a one-dimensional motion with zero velocity at both boundaries is by definition impossible for a non-condensable gas in which mass is conserved. There is a number of studies, both empirical and theoretical, where such phenomena have been investigated independent of atmospheric sciences, see a recent review by Kryukov et al. (2010). In the Earth's atmosphere the surfaces where condensation occurs (i.e. on moisture droplets) do not coincide with the upper boundary surface of the circulation but are distributed within the atmospheric column allowing for macroscopic motions in all directions. Once our theoretical propositions are seriously analyzed and studied, this may stimulate other investigators to attempt replicating relevant processes in the laboratory.

Why bother? There are several reasons. Here we highlight one which we consider the most important. If we are correct that it is principally the vapor sink that determines large-scale atmospheric dynamics, then all changes in terrestrial vegetation (and associated changes in terrestrial vapor dynamics) threaten drastic changes in seasonal wind patterns and, associated climates (Sheil and Murdiyarso, 2009). Such modifications are unrelated to other aspects of climate change such as those related to greenhouse gases etc. – but will be inherently regional and threaten many human populations[9]. Those who find these ideas unlikely should pause to consider the costs of being wrong – how certain need they be to discount the risk? We do not have a model South American or North American continent in the laboratory to test empirically what happens if their forests are destroyed or degraded.

While the climate community strives to impose global regulations on humanity based

[9] We note that Dr. Held has an opposite view on the reasons of precipitation change on land: http://www.gfdl.noaa.gov/sahel-drought



on their understanding of climate, climate scientists should not be afraid of investing in the constructive evaluation of provocative results[10]. Despite considerable scrutiny from large numbers of climate scientists over the last nine months, for which we are grateful, we have not been shown to be wrong. We request that reviewers begin to allow for the fact that we might be right. If true our theory has implications for the lives of many people and for the global environment. For both scientific and moral reasons we believe our ideas urgently need concerted objective examination by climate scientists. To make responsible predictions and elaborate strategies that are compatible with long-term human well-being, we need to ensure that the physical principles underlying our shared understanding of atmospheric circulation are correct. This calls for scrutiny not just of our propositions but of all the models that are currently utilised. We call on climate scientists to ensure good open-minded science striving for objectivity and insight – please place all bars accordingly.

**Acknowledgements.** We thank all people who discussed our work. Our special thanks are due to Drs. Peter Belobrov, Paulo Nobre and Stefan Emeis, whose comments submitted to this discussion did not require a response from the authors but were greatly appreciated. Upon the closure of this discussion, further developments with regard to our paper can be monitored at http://www.bioticregulation.ru/acpd10.php.

## References

Bryan, G. H. and Fritsch, J. M.: A benchmark simulation for moist nonhydrostatic numerical models, Mon. Wea. Rev., 130, 2917–2928, 2002.

Makarieva, A. M. and Gorshkov, V. G.: The Biotic Pump: Condensation, atmospheric dynamics and climate, Int. J. Water, 5, 365–385, 2010.

Makarieva, A. M. and Gorshkov, V. G.: Radial profiles of velocity and pressure for condensation-induced hurricanes, Phys. Lett. A., 375: 1053–1058, 2011.

[10] http://2s3c.wordpress.com/2011/04/26/advice/

Makarieva, A. M., Gorshkov, V. G. and Li, B.-L.: Precipitation on land versus distance from the ocean: Evidence for a forest pump of atmospheric moisture, Ecological Complexity, 6, 302–307, 2009.

Makarieva, A. M., Gorshkov, V. G., Li, B.-L., and Nobre, A. D.: A critique of some modern applications of the Carnot heat engine concept: the dissipative heat engine cannot exist. Proc. Roy. Soc. A, 466, 1893–1902, 2010.

Makarieva, A. M., Gorshkov, V. G., Nefiodov A. V.: Condensational theory of stationary tornadoes, Phys. Lett. A., in press, doi:10.1016/j.physleta.2011.04.023, 2011.

Held, I. M.: The gap between simulation and understanding in climate modeling, Bull. Am. Met. Soc., 86, 1609–1614, 2005.

Kolb, C. E., Cox, R. A., Abbatt, J. P. D., Ammann, M., Davis, E. J., Donaldson, D. J., Garrett, B. C., George, C., Griffiths, P. T., Hanson, D. R., Kulmala, M., McFiggans, G., Pöschl, U., Riipinen, I., Rossi, M. J., Rudich, Y., Wagner, P. E., Winkler, P. M., Worsnop, D. R., and O' Dowd, C. D.: An overview of current issues in the uptake of atmospheric trace gases by aerosols and clouds, Atmos. Chem. Phys., 10, 10561–10605, 2010.

Kryukov, A. P., Levashov, Yu. V., and Pavlyukevich, N. V.: Condensation from a vapor-gas mixture, Journal of Engineering Physics and Thermophysics, 83, 679–687, 2010.

Lackmann, G. M. and Yablonsky, R. M.: The importance of the precipitation mass sink in tropical cyclones and other heavily precipitating systems, J. Atm. Sci., 61, 1674–1692, 2004.

Sheil, D. and Murdiyarso, D.: How forests attract their rain: an examination of a new hypothesis, Bioscience 59, 341–347, 2009.

Spengler, T., Egger, J., and Garner, S. T.: How does rain affect surface pressure in a one-dimensional framework? J. Atm. Sci., 68, 347–360, 2011.

Vesala, T., Kulmala, M., Rudolf, R., Vrtala, A., and Wagner, P. E.: Models for condensational growth and evaporation of binary aerosol particles, J. Aerosol Sci., 28, 565–598, 1997.

---

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