Hydrol. Earth Syst. Sci. Discuss., 12, C4945–C4948, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C4945/2015/

Hydrology and Earth System Sciences
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Discussions

HESSD
12, C4945–C4948, 2015

Interactive Comment

Interactive comment on "Experimental evidence of condensation-driven airflow" *by* P. Bunyard et al.

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Received and published: 18 November 2015

This manuscript describes an experiment in which the authors attempt to provide evidence for the biotic pump (BP) hypothesis of Marakieva et al. In this hypothesis, Marakieva et al. argue that the atmospheric circulation is mostly driven by water vapor pressure differences caused by condensation, which is in contrast to well-established concepts in atmospheric science. In principle, I think the authors' attempt is creative and interesting and I feel sorry for my negative review. Yet Marakieva's hypothesis as well as the authors' attempt to test it are so flawed that I recommend rejection as I see no way of how this manuscript can be improved to become acceptable.

1. The (irrelevant) BP hypothesis

First, I want to point out that I am not just skeptical about the BP hypothesis, but convinced that the mechanism they suggest is irrelevant for present-day climate. This can be shown in a simple back-of-the-envelope estimate in which one compares the buoyancy generated by the heat release due to condensation to the buoyancy generated by the reduction in water vapor pressure during condensation (I am happy to share this estimate, but did not want to include it here because it distracts from the main point). The former effect (buoyancy by heating) is much larger by more than an order of magnitude than the latter. This outcome is not surprising. While it is well known and established that water vapor pressure differences can perform physical work and thus generate motion, the conditions that are associated with this are very different to the Earth's atmosphere. One practical example of work done by water vapor is the steam engine. Yet, to perform work, the operating temperature and vapor pressure differences of steam engines are substantial. They operate with temperatures above the boiling point of water, for which the saturation vapor pressure is greater than the mean sea level pressure. Such water vapor pressure differences (and temperature differences) are much greater than what is observed anywhere in the Earth's atmosphere, which at best amount to 3% of the air pressure. Because water vapor pressure differences on Earth are much smaller, the effect on buoyancy can be neglected. Hence, the BP hypothesis is, in principle, not unphysical, but simply focuses on an irrelevant effect and does not explain why convective motion takes place.

But then, this manuscript is not about the BP hypothesis itself, but rather on an experiment to supposedly support the hypothesis. Yet, this flaw in the BP hypothesis already points out that the interpretation of the experiment is most likely to be flawed as well.

2. Setup of the experiment

If I understand the setup of the experiment correctly, it uses refrigeration coils in order to dehumidify the air. In other words, air is cooled to saturation and below to condense the vapor from the air. This will inadvertently cause air to cool. After all, this is the mechanism that brings the water vapor to condensation. Yet, this cooling of the air causes a change in buoyancy as colder air becomes heavier, and this change in buoyancy is most likely the cause for downward motion below the refrigeration coils that the authors observe. The cooling also causes some condensation and thus differences in vapor pressure, so it is not surprising to find correlations between vapor pressure changes and motion. But by no means (!!!) does this prove that vapor pressure changes are the cause of motion. Yet, the authors do not comment on this critical flaw in their analysis or their discussion.

Furthermore, the BP hypothesis argues that the drop in water vapor during condensation causes an updraft (!), and not a downdraft that the authors observe in their experiment. As described above, this downdraft most likely results from the cooling of the air caused by the refrigeration coils, but not by the effects of water vapor changes. So the experiment of the authors contradicts the prediction of the BP hypothesis, rather than to confirm it.

It is also important to point out that the way the authors implemented the generation of motion in their experiment is quite different from the atmospheric environment. In the atmosphere, it is condensational heating at the cloud base and the associated buoyancy generation that causes an updraft within the cloud and ultimately results in precipitation, while the experiment generates motion by an external power source that cools the air at the refrigeration coils, generating a downdraft and condensation, but not an updraft! The link between updrafts and precipitation are very well established in meteorology, so that I think the experiment has a basic flaw in its setup. Hence, the experiment cannot describe how convective motion is commonly generated in the natural, atmospheric environment.

3. Description of the methodology

The description of the methodology is difficult to follow and contains several errors. In Eqn. 3, the units only match if the gas constant of air is used in terms of kg, and not in mol. Eqn. 5 looks like an expression for specific humidity, but is not the common way of how this is expressed (which would be $0.622 \cdot p_{wv}/p_{atmos}$). Eqn. 9 is a conversion of units, but not an equation and it has nothing to do with an expression to calculate the kinetic energy. To obtain an expression for kinetic energy, one needs to describe a momentum balance, but this balance is missing from the description. Eqn. 10 describes a kinetic energy flux, not the kinetic energy of the air flow. It also mixes units on the left hand side with an equation. I cannot judge by how much these errors affect the results and the figures.

4. Summary

In summary, the direction of the observed air flow in the experiment likely contradicts the BP hypothesis and the setup is not suited to test atmospheric convection as the implemented mechanism to cool air by an external power source to reach condensation is very different to the condensational heating that takes place within the atmosphere. Furthermore, the mechanism that causes motion in the experiment is most likely due to air cooling, rather than vapor pressure differences, but the analysis in the manuscript is insufficient to identify the main cause. Overall, the manuscript is far from "proving" the BP hypothesis and there is little to learn from the manuscript about how natural convection works. There are also several minor comments, but I think these mentioned aspects basically point out that this manuscript cannot be published.

To provide at least a little bit of a positive perspective at the end, I can imagine that this manuscript could become publishable by drastically changing its focus and make it a case study to disprove the BP hypothesis.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 10921, 2015.