

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

Anonymous Referee #1

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Many comments have been uploaded during this month, though I note that 4 author's comments of 8 December seem to be identical. In general, the author's comments consist of (1) the presentation of new experiments, with new calculations for the derived quantities (but in spite of earlier complaints it remains as difficult as ever to find out what quantities are actually depicted in the figures); (2) a repetition of former arguments, which mostly have already been refuted. I skip the discussion between the authors and Reviewer 2 about the equations 4 to 6 in the paper.

To start with the repetition of former arguments: I agree with the argument of Axel Kleidon that the results for the direction of the flow make the whole “proof” for the Biotic Pump Theory untenable: The condensation in the experiments causes a locally downward flow, whereas the Biotic Pump Theory which the authors are trying to prove, requires an upward flow. The counterarguments of the authors are indeed “not at all convincing” (the new author's comment of 8 December again repeats the old argu-

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ments). I also agree with Axel's point that "correlation does not imply causation".

I also have a remark about C5331- (author's comment of 2 December). There the authors state that "As the reviewer has pointed out, the total energy involved in condensation far exceeds that required to explain the airflow in the experiments". But that statement of mine (from C4900-, section 2.8) pertained to the energy as calculated by the authors, and it was explained earlier in section 2.4 of the comment that the calculation was wrong for two reasons, and that an improved calculation along these lines would have produced a *zero result* (top of page C4905). I apologize for not mentioning this again in section 2.8, which may have caused confusion.

Concerning the new experiments and new calculations, it is very laudable that they have been carried out. But unfortunately, the confusion about the interpretation of the results may even have got worse now. See the legend of figure 2 in C5474- (identical to three other comments of the same day). It is stated that a positive density difference means that the air in the right-side column is denser than in the left-side column, etc. ; since the depicted difference is on the other hand negative during the cooling phase, the right column is less dense, which would imply counterclockwise circulation, opposite to the what is observed (also stated in the main text). But *how can cooling of the right side column cause the air to become less dense ?* An error must have been made here. This implies that the argument made against the density-explanation breaks down.

Besides, it is hard to deduce from the text what quantities are depicted in figure 2 in C5474-: The text (second page) talks about "kg change per second" but it is not the change per second but just the difference in weight between the columns which theoretically is driving the circulation. Since the observations are sparse in space, this quantity is difficult to monitor accurately. Further, the derived mechanical power has to be integrated in time to find its contribution to the kinetic energy of the flow (but there are also counter-contributions of leakage and friction), yielding in the end much larger numbers than for the change per second.

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12, C5500–C5502, 2015

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In conclusion, about the new experiments: I don't understand all the details, but I am not convinced that the classical calculations of the mechanical power (if they can be done with such sparse observations) yield too small results to explain the circulation.

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

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