Hydrol. Earth Syst. Sci. Discuss., 12, C4900–C4909, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C4900/2015/

Hydrology and
Earth System
Sciences
Discussions

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

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Received and published: 17 November 2015

1 General

The paper is clearly of an unusual type. It describes measurements of convection in a more or less closed air circuit, forced by cooling coils. It would seem that such phenomena have been well understood since the 19^{th} century, when flow dynamics and thermodynamics were sufficiently developed. However, the authors criticize the common explanation of the phenomenon, and claim that the role of condensation has been overlooked. Condensation causes gradients in the partial pressure of water vapor, which are considered by the authors to be of highest importance for the flow dynamics. This claim goes back to the HESS paper by Makarieva and Gorshkov (2007) cited in the Discussion Paper (DP).

Meteorologists and physicists usually deny that the partial pressure is of importance

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as a forcing agent. On the other hand, the new theory has attracted considerable interest among environmental scientists, not to speak about many non-scientists who are concerned about the fate of forests etc. (the DP offers more information about the context and the link between forests and the proposed mechanism).

I think that this is a sufficient reason to have a fresh look at the foundations of flow dynamics. The strong point of the experiments described in this paper is that they have been done in a controlled environment, and are hence less elusive than the large scale processes in the free atmosphere. On the other hand the experiment is seriously flawed in that the cooling devices which have been chosen to force convection, seem to generate condensation in all cases, even if relative humidity is as low as 30 %. For the present purpose, it would have been better to use less concentrated cooling (and heating) devices, so that cases with and without condensation could be compared.

My own conviction is, not surprisingly, that convection would occur even if there was no condensation at all. By the way, if heating instead of cooling is used, condensation is unlikely and can be ruled out as a cause. The authors reports that heating by external insolation (10927, 11-13) causes a background convection of almost similar strength as the one caused by the cooling coils. While commenting on this (10937-10938), the authors only emphasize the weakness of this flow, but this may be due just to the design of the experiment to shield the air from external influence. It would have been very interesting to do experiments with controlled internal heating.

In the following, I have tried to work out the data and their interpretation as well as I could. This work was hampered by lack of first-hand data in the paper, as well as lack of clear explanations of how the derived quantities were calculated, and because of this it is possible that some details of the following have to be revised. My conclusions are however that the paper tends to misinterpret physical laws, and that in consequence of this, the mechanical effects of both condensation and of density difference have been wrongly calculated, exaggerating the first and underestimating the second.

Based on this, my judgment is that the present paper is not a genuine scientific contribution: the interpretation of the observations is wrong, and there is no good reason to believe that with a correct interpretation they would yield anything but a confirmation of the traditional theory. However, I invite the authors to point to relevant substantial errors in the analysis below.

2 Specific comments

2.1 About the introduction:

The survey in the introduction about the traditional view of air motion, versus the biotic pump theory, is hard to follow: the biotic pump theory is in our opinion rather incoherent, and not matching traditional physics unlike the authors sometimes say, whereas the traditional view is not well represented. To mention one example: it is stated (10924, 14-19) that heating cannot change the weight of an air column, but that is irrelevant since heating causes the column to expand not only vertically, but also to spread out horizontally at higher levels. This causes the drop in surface pressure; so pressure lowering does not require the disappearance of molecules. On the other hand, unlike what is stated in the text, condensation does not by itself lower the weight of the column (the weight is lowered when the condensate reaches the surface, but this is not what the authors mean).

Other arguments in favor of the condensation/biotic pump theory are very indirect, or seem to be based on reversing cause and effect. I won't comment further on the subjects of the introduction as they are no main themes of the paper.

2.2 An estimate of the kinetic energy

The core of the argument in the paper is that the classical explanation (based on density-difference) yields very small values for the net work done by the gravity on the cooled air, especially in comparison to the mechanical energy produced by the condensation (as calculated by the authors). The calculations of both these quantities will be criticized below. However, before this is done, I think that it would be instructive to have a look at the kinetic energy that has to be explained, which is a missing point in the paper. This is an easy exercise: the equation is

$E_{kin} = 0.5\ M\ v^2$

With a total air volume of 20 m^3 (DP abstract line 13) and estimated air density $\rho = 1.25$ kg m^{-3}, and a typical $v = 0.2$ m s^{-1} (maximum of the values in table 1), we obtain $M =$ 25 kg and

$E_{kin} = 0.5$ J = 0.5 Ws

This is small in some respects, but it will be a maximum for the laboratory experiment described in the paper.

Consequently, the classical explanation (based on density-difference) is not falsified by noting that the calculated mechanical power is just small, but by proving that is too small to produce a kinetic energy of 0.5 J on the long term. The smallness of the power in comparison to the power involved in the condensation (as calculated by the authors) is by itself not conclusive.

2.3 The condensation approach in the paper

According to the explanation in C4745, the calculation with the condensation approach goes as follows: the rate of change of partial pressure of the water vapor, $\Delta p_{wv}\ /\Delta t$ on passing the cooling coils is multiplied by the effective volume V within the coils, which follows from the section area A =0.96 m^2 and the passing length l_{coil} = 0.05 m, hence $V = Al_{coil}$ = 0.048 m^3. The result is equated to the exerted power P (work on V per time unit), hence

$$P_{condensation} = V\Delta p_{wv} \,/\, \Delta t$$

My understanding of the background of this calculation is as follows: It is assumed that the partial pressure difference Δp_{wv} causes a force over the volume of $A\Delta p_{wv}$; the force is multiplied with the velocity v to find the mechanical power $A\,\Delta p_{wv}\,v$; but since $v = l_{coil}\,/\Delta t$, the result equals $V\,\Delta p_{wv}\,/\,\Delta t$, which completes the derivation.

2.4 Critique of the condensation approach in the paper

As far as I can see, the argument is mainly in agreement with the correct Newtonian approach to calculate the power exerted on the volume V between the coils, if one neglects gravity and friction. There is one dubious aspect, and this critique will not come as a surprise for the authors given the earlier discussions about the first biotic pump paper (Makarieva et al. 2007, see references in DP). According to classical understanding, the pressure force is exerted by the *full* air pressure; the partial pressure of the water vapor plays as such no role. Newtonian mechanics explains in no way how difference in *partial* pressure can drive macroscopic air flow. The measured differences in the full pressure of the air (figure 23 in the DP) do not reflect differences in the partial pressure, and are far smaller (using the common hydrostatic approximation, they would be expected to be even smaller than in the figure, but we skip this point). The authors stick to the partial water vapor pressure for explanation (as usual in the biotic pump theory), but the physical basis for this choice remains unexplained.

There is a second problem with the derivation, which is a bit subtle: the calculated power is exerted on the air volume within the cooling coils; but the agent which exerts

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the power is the remainder of the air in the convector circuit, and according to the law of action and reaction, this remainder experiences from the cooled volume just the same amount of power, but in the opposite direction. The net power which is exerted on the whole of the air is zero ! This is a usual situation with internal forces; to get the air as a whole in motion, an external force (gravity) is needed.

Related to this problem is that the disappearance of vapor from the air, on which the authors lay so much emphasis, is expected to lead to a tiny displacement of the air from all sides, which does not force a convective flow but rather a very weak implosion. The authors then invoke gravity to explain how this energy can cause a one-direction flow, but the argument is unconvincing since gravity works on mass, but mass density plays no role according to the authors.

2.5 The density-difference approach in the paper

The explanation of the density-difference approach in C4746 is still vague. It is stated that “the process is comparable in every respect” to the process driven by the partial pressure difference. But that cannot be a good approach, as the driving by density difference depends on a weight surplus integrated over the height for which it is important, whereas the driving by pressure difference depends on the pressure difference between the ends of the volume. Further, gravity (for which the gravity acceleration g is essential) is mentioned, but does not show up in the working out of the calculations. Taken at face value, the authors determine the density change $\Delta\rho/\Delta t$ along the cooling coils (from measurements of thermodynamic parameters and flow velocity), multiply with the cooled volume $V = 0.048$ m^3 to obtain the change in kg s^{-1}, then multiply with the section area $A = 0.96$ m^2, and then divide by 1 s^2 (without explanation). This interpretation of mine may be false, but it is difficult to find another one from the given information.

2.6 Critique of the density-difference approach in the paper

I find the calculation in the paper unintelligible, and don't understand the ideas on which it is based. It is presented by the authors as a quite usual approach, but on the contrary it does not look like anything I have ever seen in a text about flow dynamics. I will now give my own ideas for such a calculation, and then show that this yields a power which, though small, is many times the value used for the paper (as far as I understand it).

My own ideas for power calculation would be as follows. The gravity force (=weight) on a column is given by

$$gA\int_{bottom}^{top}\rho(z)\,\mathrm{d}z$$

Power is calculated as the inner product of force and velocity (this principle is also used in the paper). Hence the total power exerted by gravity on the left and right columns is

$$P_{density-difference} = gA\int_{bottom}^{top}(\rho_{right}(z) - \rho_{left}(z))\,\mathrm{d}z\; v$$

The minus sign occurs because the flow has opposite directions in the columns. This equation expresses the total power: The horizontal pieces do not contribute because force and flow are perpendicular there. Further, powers exerted by pressure differences sum up to zero (because it concerns internal forces, see the second last paragraph of 2.4 above, or to put it differently because a closed integral over the pressure gradient is always zero). Friction is neglected.

As a first approximation, we may assume that ρ_{left} has a "background value" which does not depend on height; ρ_{right} has the same value above the cooling coils, but below them we approximate

$$\rho_{right} = \rho_{left} + \Delta\rho$$

with $\Delta\rho$ the density jump at the coils. This is a crude assumption: it is likely that the density difference between the columns becomes weaker when the surface is approached, because of air mixing and leaking of heat from outside, so the result of the calculation will be somewhat exaggerated. This result becomes

$P_{density-difference} = gA\Delta\rho h_{coil} v$

with h_{coil} the height of the cooling coils above the surface.

This can be translated further by using $v = l_{coil}/\Delta t$, with l_{coil} and Δt the passing length and passing time along the coils. Using further that $A\ l_{coil} = V = 0.048\ \mathrm{m}^3$ in the usual notation, we find

$P_{density-difference} = (\Delta\rho/\Delta t)\ V\ g\ h_{coil}$

(we neglect for convenience the difference between the A-values inside and outside the cooling region). In this form, the calculation somewhat resembles the one indicated by the authors in C4746, but it is not finished by the mysterious multiplying by *A*-per-second-per-second, but by $g\ h_{coil}$. This would make the result about 40 times as large as calculated for the paper. Because of the nature of the approximations mentioned above, the real difference will be not as large as this, but still large.

2.7 The classical (density) approach can explain the strength of the convection

It is difficult to fill in the values needed to calculate the power, because information in the paper about changes in density (or temperature) is usually “second-hand” (but the authors are invited to provide first-hand data of $\Delta\rho$ or more preferable ΔT which one would expect to be more accurate, as it is measured more directly; $\Delta\rho$ can be approximated as $-\rho\Delta T/T$).

Let us take figure 15 (for 6 August 2015). This shows a typical mechanical power (by the density change) of 8 x 10^{-4} W (brown curve in figure 15; note that the curves represent *power* in W, the captions are very confusing). According to the preceding, this will be too small by some factor. Let us assume that the factor is 20 (one half of the maximum estimate), then the correct power would be 4 x 10^{-3} W. With this power it would take only 2 minutes to reach a kinetic energy of 0.5 J (corresponding to 0.2 m s^{-1}, see 2.2 above), neglecting effects of friction and leakage. Figure 5, which shows the flow speed for the same day (disturbed by background convection) shows a building up in time which is like what one would expect from this estimate.

2.8 The condensation approach predicts an exaggerated strength of the convection

The power calculated by the authors from condensation on the other hand, is typically 10 W (figure 15, grey), or 2500 times our estimate for the density-induced flow. If this is true, one would expect stronger flows than the observed ones. For, the development of kinetic energy E_{kin} is expected to follow an equation like $dE_{kin}/dt = P - E_{kin}/\tau$ in which P is the mechanical power (10 W) and τ is the characteristic stopping time caused by the friction in the circuit. In equilibrium ($dE_{kin}/dt = 0$), E_{kin} is about 0.5 J (maximum, see under 2.2 above) which would yield as an estimate for the stopping time: $\tau = E_{kin}/P = 0.05$ s. This is unrealistically short for air which can move freely through a circuit with about 1 m^2 section area. A stopping time of many seconds is more realistic for such a circuit. That is also in accordance with the result which one would get with the classical estimate of the mechanical power: $P = 4 \times 10^{-3}$ W, see under 2.7 above, hence $\tau = E_{kin}/P = 125$ s.

On the other hand, assuming such a high τ, the equilibrium kinetic energy for a condensation-derived power of 10 W (as claimed by the authors) would become $P\tau = 1250$ J, which would correspond (according to the equations under 2.2 above) to a far too high flow speed of $\sqrt{(2P\tau/M)} = 10$ m s^{-1} (though the calculated value may be exaggerated as τ could become lower for higher flow speeds).

2.9 About the discussion and conclusion

Discussion and conclusion depend on the results (mainly: that density change cannot drive the convection, which I find untenable), and will not be commented on here.

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