Interactive comment on “The boundary condition for the vertical velocity and its interdependence with surface gas exchange” by Andrew S. Kowalski

Anonymous Referee #3

Received and published: 27 April 2017

General comments: Vertical velocity has a tiny magnitude near surface and is difficult to measure because its magnitude is usually smaller than errors. However, vertical velocity plays a substantial role in mass and energy exchanges between land and atmosphere. For simplicity, they usually assume it is zero at surface. The author argues that it is non-zero by a “thought experiment”. The author is a theoretical thinker. This paper shines light on this knowledge gap. I recommend it to be published with minor revision. Specific comments: (1) 2.1.2 The 0th Law of Thermodynamics – I do believe that this is a case from second law of thermodynamics (Postulate of Clausius, see Thermodynamics by Enrico Fermi, 1936). I don’t think that “The 0th Law of Thermodynamics” is independent from second law of thermodynamics. So I suggest using
the second law of thermodynamics instead of the 0th Law so that your statements no
matter heat transfer and mass diffusion are govern by the same second law of thermo-
dynamics. Fourier’s law and Fick’s law are empirical relationships between fluxes and
gradients. Gradients are drivers for fluxes and consequences of fluxes reduce gradi-
ents, following a single irreversible direction (entropy increasing) –equilibrium (entropy
maximum) –second law of thermodynamics.

(2) Vertical velocity at surface is always positive (upward) predicted by the equation (4).
Based on your thought experiment, this looks true everywhere (leaves, ground, water
surface) including large scale (e.g. synoptic scale). To my knowledge, it is sure that
vertical velocity is negative in high pressure system areas and positive in low pressure
system areas. Therefore, it is difficult for me to understand the positive vertical veloc-
ity predicted by your theory in high pressure system areas or divergent air-flow near
surface at any scale. Please clarify the conflict in your revision.

(3) Page 6 second paragraph,

It is fine to me with “vertical advection” because it is clearly defined by vertical compo-
nent

It does not need to assume horizontal homogeneity.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2017-172/acp-2017-172-RC3-
supplement.pdf

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arguing of the above product \( \mathbf{v} \cdot \nabla \mathbf{v} \), where \( \mathbf{v} \) is the fluid velocity and \( \nabla \) is the gradient operator. To be clear, it can make sense to speak of “upward diffusion”, but certainly not “upward advection”. The analogy, in the context of surface-atmosphere exchange, to speak of “upward diffusion” (e.g. Brando et al., 2009) is historicall related to an assumption of horizontal homogeneity, permitting horizontal scale gradients, particularly in the direction of the mean wind.

It is fine to me with “vertical advection” because it is clearly defined by vertical component

\[
\mathbf{v} \cdot \nabla \mathbf{v} = \frac{\partial v}{\partial x} + \frac{\partial w}{\partial y} + \frac{\partial w}{\partial z} \\
\text{vertical advection } \frac{v}{c} \\
\text{horizontal advection } \frac{s v}{c} + \frac{s w}{c}
\]

It does not need to assume horizontal homogeneity.