

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

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The author is known for his accurate and meticulous assessment of very fundamental aspects of atmospheric physics. In his present paper he addresses an issue that has led to many discussions before and which has not been convincingly solved so far: the magnitude of the vertical motion in the planetary boundary layer near the Earth's surface, a motion that is too small to be accurately measured with present-day state-of-the-art ultrasonic anemometers, but which is still large enough to affect (eddy covariance) flux measurements of trace gases.

So far most scientists would agree that at a certain small height above the solid ground surface, the roughness height z_0 (in Kowalski's notation) the mean horizontal wind

speed must be 0 m s^{-1} , and also mean vertical wind speed $w|_0$ should be 0 m s^{-1} , a boundary condition that Kowalski questions on good grounds. He links $w|_0$ directly to the moisture flux density (E). He develops his theory based on the one-dimensional equation

$$w\rho = \sum_{i=1}^N w_i \rho_i , \quad (1)$$

with w_i and ρ_i being the vertical velocity and partial density of gas component i in a gas mixture with N components. Conceptually this is a hydrostatic approach that only allows for expansion in the vertical direction, which may exaggerate the magnitude of vertical velocity w . Hence, in Section 2.3 Kowalski expands to the full 3-d advection-diffusion concept that should better represent reality.

My main critique – although I must admit that my own understanding of atmospheric physics is not nearly up to the level of that of Kowalski's – is the following:

1. Kowalski primarily associates the vertical velocity at roughness height $z|_0$ with the evaporative flux E but not with the vertical sensible heat flux H . I would assume that this is only correct for $H = 0 \text{ W m}^{-2}$, but not for any other magnitude of sensible heat flux. In my view a partial gas density expressed as ρ_i in kg m^{-3} has its volume component affected by both sensible and latent heat fluxes – and all other gas component fluxes (which however can be neglected, I agree on this aspect). An explicit treatment of the effect of H would be essential in my view to help the average reader (like myself) better understand the concept and argumentation.
2. In principle the concept and analysis could be expanded to the different isotopes (stable or unstable, but the treatment of unstable isotopes would probably add yet another layer of complexity) of each gas component. At least the coverage of



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stable isotopes might be helpful in context with “counter-gradient isotope fluxes” that tend to be brought up occasionally.

3. It would be appreciated to reword some passages where plant physiologists and plant ecologists are non-neutrally qualified as partially ignorant scientists. I must admit that I had a private discussion with Graham Farquhar at a conference in Interlaken more than 10 years ago about “counter-gradient isotope fluxes” and actually had the feeling that it is fruitful in interdisciplinary work to exchange ideas between disciplines, but should not consider ourselves superior to those who start to dig into new terrain (from their perspective) – we tend to leave a similarly bleak trace if we dare to lean outside of our own territory. I think it is the strength of interdisciplinary researchers that they take the risk to be considered a non-savant outside their area of profound expertise, and we should restrain from spreading bad marks to others from other disciplines (this relates mostly to lines 394–395, 410, 415–424).
4. The conclusions end with a very general take-home message, but since the author puts so much emphasis in his text to educate plant ecologists, it would be beneficial to have a more specific recommendation set for what plant ecologists finally are supposed to do with this new-gained knowledge. This does not explicitly become clear and the paper would benefit by having such explicit, specific recommendations that I and other could easily pick up, understand, and implement in our own calculations.

Some minor technical issues that should also be corrected:

L. 86: add “vertical” before velocity

L. 403: this appears to be the old notation of the previous (internal) version and should now read $(w|_0)$

