The author thanks the referee for the evaluation and especially for the recommendations to improve the manuscript. In the replies that follow, the referee comments are repeated (bold font) followed by the responses from the author (normal font).

Replies to Referee #2 (W. Eugster)

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 ms^{-1}, and also mean vertical wind speed $w|_0$ should be 0 ms^{-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

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

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 advectiondiffusion concept that should better represent reality.

The author thanks Dr. Eugster for this assessment, which shows that the manuscript has managed to communicate the essence of the theory being developed.

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 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 it's 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.

The author disagrees here, and points to his previous publications that address this very issue.

Certainly, it is traditional in micrometeorology to infer a mean velocity in the direction of the sensible heat flux (Webb et al., 1980; "WPL"), as Dr. Eugster asserts. However, Kowalski (2012) showed that this inference is an artefact of imprecise averaging procedures. Perhaps the simplest illustration of the WPL error is given by the scenario visualized in the artless drawing in Figure X below, where turbulent air convection is enclosed within a stationary chamber with an upward heat flux under steady-state conditions.

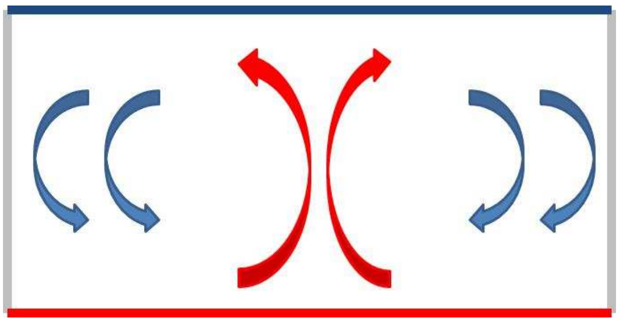

Figure X. A chamber with a heated floor (red), chilled ceiling (blue), and insulated side walls (grey). The inside air is in steady state and turbulent (convective) due to an unstable lapse rate.

The most fundamental definition of the mean velocity of a system is the ratio of its displacement to the time elapsed. For steady-state air confined to a stationary chamber, its displacement over long time periods is clearly zero, and therefore so is the average velocity, notwithstanding WPL predictions of an upward velocity associated with the upward heat flux.

For non-steady-state conditions, average velocities can occur due to expansion/compression of the air layer near a surface boundary. For example, in Figure X, assuming that temperature effects dominate those of pressure in determining air density, there is more air near the cool ceiling than at the hot floor. Thus, eliminating floor heating and ceiling cooling would result in a net downward transport of air. Such effects are treated by Kowalski and Serrano-Ortiz (2008) for a simple scenario, neglecting evaporation's influence on the average velocity, and demonstrating conceptual errors in the WPL velocity. A more general derivation, combining the effects of compressibility and vapor exchanges, is beyond the scope of the current discussion.

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

The author agrees that the concept and analysis could be expanded to the issue of isotope transport, but is ignorant of the phenomenon of "counter-gradient isotope fluxes". Perhaps Dr. Eugster could provide references for these observations (?). Otherwise, it seems that this issue is beyond the scope of the present manuscript, but could be worthy of future investigation.

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).

The author is aware that some readers find his writing style to be offensive, has worked hard to try to correct this problem, and is not surprised to find that it persists. There can be little doubt regarding the benefits of exchanging ideas between disciplines, and any specific recommendations would be very welcome regarding how to reword passages so as to be more neutral.

However, having said that, the author is unwilling to refrain from criticizing theories or procedures that are incorrect. The objective here is not to consider anyone as superior, but rather to discover and defend the truth, and the author has taken great care to do this based solidly on fundamental physical laws. From the author's point of view, Jarman (1974) neglected momentum conservation, erroneously classified the description of Parkinson and Penman (1970) as "incorrect" (twice) or "substantially incorrect" (twice more), and misled an entire community of scientists along a mistaken path for several decades. The key question then is how correct this error and prevent its further propagation.

In an attempt to comply with Dr. Eugster's suggestion and avoid defaming a particular discipline, the author proposes the following changes

- Lines 394-394: delete ",as has been neglected by the discipline of plant physiology, or ecophysiology";
- Line 410: delete ", but broadly neglected in the field of ecophysiology"
- Line 417: delete "among plant physiologists"

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 pik up, understand, and implement in our own calculations.

Unfortunately, the author has not been able to develop a "quick fix": an algorithm or equation that would immediately correct WUE or c_i by accounting for non-diffusive transport. This therefore falls under the category of future research, and is open to any scientist who may have better ideas about solving these tricky issues.

"Minor technical issues":
L. 86: add "vertical" before velocity

The author agrees to make this change when revising the manuscript.

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

The author agrees to make this change when revising the manuscript.