I thank the anonymous referee for the engaging discussion. Although pushing back against suggested changes, I appreciate this on-line forum as a space in which to rationalize decisions regarding wording and composition, and remain open to further suggestions to improve the manuscript. The referee's comments (italic font) are followed by my replies (normal font).

I agree that the model of stomatal flux as exclusively diffusive is oversimplified; at times I've said that one person's diffusion is another's advection, and applying advective flux principles to stomata is likely to lead to new insights.

I appreciate the reviewer's perceptive perspective.

The parentheses aren't needed in equations 1-3, and 'mere trace' should perhaps be quantified more explicitly, perhaps as greater than 0.9% (argon) or 0.4% (water, on average), because many peoples' research careers depend on these key trace gases including CO2!

Mathematically, the parentheses do not change these equations, and so they are not needed. However, they are included to focus attention on the partial pressure of dry air (line 30). I find them helpful as a reminder that, with negligible cavity pressurisation, humidification suppresses the partial pressure of dry-air and thereby its every component.

I worry that an attempt to quantify "mere trace" would open a can of worms. The most abundant trace gas is CO_2 at about 0.042% of the atmosphere. Its neglect would mean describing atmospheric composition with 99.958% accuracy, which seems acceptable. But where should we delineate the acceptable degree of accuracy? Can we confidently say that 99% is acceptable, but 97% is not? I have searched the literature, of not only atmospheric gases but science in general, for some definitive and citable statement regarding what is and is not negligible. I have found nothing concrete, and so hesitate to explicitly quantify "mere trace".

This sentence begins with the words "Dalton's law of partial pressures", which establish its context. As I have argued to the other reviewer, context is very important when determining what can and cannot be neglected. In the context of Dalton's law, CO_2 is negligible. Of course, it is not negligible in the context of atmospheric absorption of infrared radiation, but that is another story altogether.

In equation 4, the delta implies a surplus or a deficit depending on direction (the text only states surplus)

Line 36 defines Δ as denoting a surplus. Mathematically, a negative surplus is a deficit. Thus, $\Delta e>0$ describes a surplus of water vapour, whereas for dry-air component *i*, $\Delta p_i<0$ describes a deficit.

54: This value still seems quite fast, even as an upper bound, but could be justified in more detail by explaining a bit more the contents of Kowalski 2017 as it applies to the present study

An earlier draft of this manuscript included the following text:

> Newtonian physics identifies an air jet escaping stomata (Kowalski, 2017), here summarised. Air components have diverse momenta (kg m s^{-1}) including outward H_2O and O_2, null N_2 and Ar, and inward CO_2. Air's momentum is the sum of its components' momenta. With H_2O dominating stomatal gas exchange by orders of magnitude, air's momentum density equals the H_2O flux density (F_{H_2O}; kg m^{-2} s^{-1}) that quantifies transpiration. The outward airspeed is therefore the momentum-to-mass ratio $\frac{F_{H_2O}}{\rho}$

A colleague who helpfully read the manuscript suggested that, for a paper describing a model of oxygen's partial pressure inside stomata, this text is extraneous and distracting.

The purpose of this section of the paper is to establish that cavity pressurisation is negligible in the context of Dalton's law, **at most** Δp = 0.0011 kPa. The reviewer says that "*This value still seems quite fast"*, implying that I have likely overestimated v and therefore Δp. If anything, this buttresses the argument that pressurisation can be neglected ($\Delta p = 0$). Therefore, I think my colleague was correct, and that there is no need to justify in more detail using v = 6 mm s^{-1} as an upper bound.

I'm having trouble fully following the paragraph on line 57. The difference is about 1 Pa, which isn't much. Is the argument that, even with extreme parameter values there is little reason to believe that delta_p is approximately zero such that positive delta_e implies negative delta_p of the other gases?

I think the reviewer follows perfectly, but also that example values might illustrate this more clearly. Let's consider a "moderate VPD" (Aliniaeifard and van Meeteren, 2014), with Δe = 1.17 kPa, and examine two cases when assessing dry air's partial pressure, between parentheses in Eq. (4):

1. If we use the upper bound of Δp = 0.0011 kPa, then it must be depressed by 1.1689 kPa;
2. If we neglect pressurisation and use Δp = 0, then it must be depressed by 1.17 kPa.

These outcomes are 99.9% the same, which I think justifies assuming Δp = 0 in Eq. (4). I do not wish to add these example values to the manuscript, unless the reviewer and editor so prefer.

Reference

Sasan Aliniaeifard, Uulke van Meeteren, Natural variation in stomatal response to closing stimuli among *Arabidopsis thaliana* accessions after exposure to low VPD as a tool to recognize the mechanism of disturbed stomatal functioning, *Journal of Experimental Botany*, Volume 65, Issue 22, December 2014, Pages 6529–6542, https://doi.org/10.1093/jxb/eru370

I still find the evidence to be by omission: if O2 can't leave stomatal cavities by diffusion, it must be 'jets'. But what do these jets look like, how do they 'burst' and do other mechanisms like Bernoulli pumping cause the transport in practice? To me the major weakness is a lack of description of how the jets work, for lack of a better word, in practice and if fluid mechanical simulations, conceptual models, or studies of 'bursting' when stomata open (or the 'thermostat' model that Joe Berry described) would be most fruitful for better understanding stomatal dynamics going forward.

Rejecting long-embraced ideas can be a major challenge for advancing knowledge (Keynes, 1936), and I believe this applies to the assumption that diffusion alone transports gases through stomata. I would argue that, if O_2 cannot leave stomatal cavities by diffusion, then non-diffusive transport must be responsible. The terminology of "jets" may be appropriate as argued by Kowalski (2017), or it may not. Once it is accepted that non-diffusive transport exists, science can then address its mechanistic functioning as a next step. Does it occur as a steady stream, or in spurts? If the latter, are the spurts triggered by atmospheric turbulence (a Bernoulli effect), or by thermodynamic forcings, or what? Such questions certainly seem relevant to understanding stomatal control of gas exchanges, particularly at high temperatures. I share the referee's curiosity regarding these questions, but feel that they will be best addressed only once the scientific community accepts the relevance of non-diffusive transport.

Reference

Keynes, J. M., The General Theory of Employment, Interest and Money, Macmillan (1936)