

Summary

The authors outline current shortcomings in this discussion paper from excluding the impacts of water vapour by the use of dry air equivalents. In certain cases, this may impede understanding of measurements or cause inappropriate model outcomes especially for ecosystem-atmosphere interactions. Using thought-experiments, they discuss this issue, its effects and call to improve such models as well as use mole fractions to include water vapour.

We thank the reviewer for taking the time to assess our preprint and for providing helpful comments. The above paragraph summarizes our paper adequately, and we believe the suggestions below will help us to improve the paper upon revision.

There indeed seem to be cases where the outlined issues occur, which could be further highlighted with specific examples. However, many of these cases are only found in very specific or extreme conditions. Furthermore, the current availability of observational data from such cases are still few, requiring theoretical considerations and some speculation as to the magnitude and frequency of their impacts. The inclusion of further supporting data and concrete examples using measurements would be helpful to illustrate the authors arguments.

We agree that concrete examples are needed. Indeed, in our preprint the sentence at lines 61-63 presaged evidence for hemispheric consequences but the paragraph regarding oxygen (beginning at line 94) came up short in this regard. Therefore, we propose to replace that paragraph with the following two:

Oxygen (O_2) is key example of a Type III gas whose overlooked humidity dependence is biogeochemically significant. Discarding water vapour before chemical analysis (Keeling and Shertz, 1992) confines O_2 's dry-air molar fraction (c_i) within $20.95 \pm 0.01\%$, i.e. varying globally by less than 100 ppm (Machta and Hughes, 1970). In reality, water vapour's molar fraction ($\chi_{\text{H}_2\text{O}}$) can reach 5% in extreme sultriness (Raymond et al., 2020), suppressing χ_i —which better describes the true abundance that determines biochemical and geophysical processes—by up to 10,000 ppm in the case of O_2 (Fig. 1). Thus, WVD makes O_2 variability orders of magnitude greater than previously supposed, with strong latitudinal and seasonal patterns in each hemisphere (Kowalski and García- Valdecasas Ojeda, 2025).

Geoscience studies that relied on dry-air O_2 fractions overlooked this. Meridional, oceanic O_2 transport was assessed (Portela et al., 2024) via a model that ignores latitudinal p_i gradients when calculating dissolved O_2 (Aumont et al., 2015). Global O_2 cycle depictions claimed summer air is more aerobic (Gruber et al., 2001; Petsch, 2003), but humidity's strong seasonality makes the opposite true and much more so. Supposing O_2 to mirror carbon

dioxide (CO₂; Gruber et al., 2001), hemispheric O₂ cycles were assumed to be decoupled (Keeling et al., 1998). But while O₂ reflects CO₂ regarding biochemical stoichiometry, the same is not true about physics. Abundances, gradients, and transport mechanisms of CO₂, which is a Type II gas, are fundamentally shaped by the carbon cycle and only secondarily influenced by WVD. By contrast, O₂ is a Type III gas that moves with dry air, whose cross-equatorial transport must be massive to buffer p against seasonal shifts in e , per Eq. (1). For Type III gases like O₂, it is the hydrological cycle that overwhelmingly determines abundances, gradients, and transport mechanisms.

Additional References:

Aumont, O. et al., PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies, *Geosci. Model Devel.*, **8**, 2465–2513, <https://doi.org/10.5194/gmd-8-2465-2015>, 2015.

Gruber, N., M. et al., Air-sea flux of oxygen estimated from bulk data: Implications For the marine and atmospheric oxygen cycles, *Global Biogeochem. Cycles*, **15**(4), 783–803, <https://doi.org/10.1029/2000GB001302>, 2001.

Keeling, R. F. et al., Seasonal variations in the atmospheric O₂/N₂ ratio in relation to the kinetics of air-sea gas exchange, *Global Biogeochem. Cycles*, **12**, 141-163, <https://doi.org/10.1029/97GB02339>, 1998.

Petsch, S. T., The global oxygen cycle. In Schlesinger, W. et al., *Treatise on Geochemistry* Volume 8, Amsterdam: Elsevier, 2003.

Portela, E. et al., The ocean's meridional oxygen transport. *J. Geophys. Res.: Oceans*, **129**, e2023JC020259 <https://doi.org/10.1029/2023JC020259>, 2024.

Overall, the paper acts as an incentive to increase data collection of such cases, as well as creating awareness for the current model shortcomings, which the community need to consider when evaluating their own data. The paper should be accepted after some revision and linguistic editing.

We thank the reviewer for this encouraging assessment.

Specific points

- 1. Generally, a well-conceived idea to raise awareness of such issues. That is why basing arguments on concepts of “air is water vapour” detracts from the overall discussion and aim. Because water vapour can in some cases reach bulk concentrations, does not warrant it to assume parity with the main constituents in***

all cases. Naturally, the addition of a gas will change the relative distribution among the other constituents. The discussion could be dedicated less to this effect and more to its impacts.

Two reviewers have objected to the phrase “air is water vapor,” indicating the need to reword our narrative in this regard. We hope that the following revisions express the underlying idea with greater clarity:

- a. In the abstract, we propose change the offending sentence to: “Here it is shown that water vapour is an air component of paramount importance because its sources and sinks dominate those of air.”; and
- b. We propose to substitute the paragraph that begins at line 46 with the following:

By dominating air’s sources and sinks, water vapour uniquely exerts influence on atmospheric dynamics. Source–sink flows (Owen et al., 1985) are a type of fluid motion whose streamlines begin at sources and end at sinks. Though subtle in the atmosphere, such flows are shown below to contribute to transport, and they are largely governed by WVD. Since evapotranspiration exceeds the combined surface fluxes of all dry-air components by orders of magnitude (Kowalski, 2017), to a very close approximation, air’s sources and sinks are those of water vapor. This is reflected in the above example where the Mediterranean’s higher sea-level p is due to its greater humidity (Table 1), which in turn is forced by its superior evaporation rate (Lu, 2007). Thus, the substantial Atlantic–Mediterranean pressure gradient in Table 1 arose from WVD, and such gradients drive air motion (Sun et al., 2013). No other gas has comparable dynamical significance.

2. ***The authors omit any discussion considering humidity corrections in observational data to account for the impacts of water vapour in the aforementioned cases and references to relevant method papers or examples where such effects were observed could be added.***

We think that the paragraph proposed above for insertion into the manuscript regarding oxygen provides several examples that show the benefits of correctly accounting for humidity. Our equation (2) makes the relevant humidity corrections, and as suggested by the first chronological reviewer (Dr. Yakir) we propose to add a comment following that equation to note that a quantitative example of this is depicted in Figure 1.

3. ***Replacing dry air measures simply with molar fractions that include water vapour would likely cause more issues than it would solve, except in the few circumstances outlined in the manuscript in which cases molar fractions could be***

used. Also, this method would remove this useful measure for independent comparisons. If decoupling of CO₂ and H₂O at stomatal interfaces has been observed due to this effect, then there should be data available to model these processes with the improved approach suggested by the authors to illustrate the importance of such effects.

We do not propose abandoning dry-air measures, but we do call for ending the systematic neglect of humidity. Constant inclusion of humidity data when monitoring and reporting gas concentrations would allow researchers to account for water vapour fluctuations where relevant. The paragraph proposed above for insertion into the manuscript regarding oxygen provides several examples where reliance on dry-air fractions has restricted knowledge. Regarding the stomatal gas exchanges, as noted in our reply to Dr. Yakir, we prefer to keep the focus of this paper on the underlying physics and not make it largely about ecophysiology. We agree that a new modeling framework is needed for high-temperature stomatal gas exchanges, and hope to address this in future work.

4. In Fig. 4. the CO₂/H₂O decoupling graph is not very clear. Please improve the illustration/graph/labelling to highlight the argument and link it with the examples given.

We agree that Fig. 4 was not adequately explained, and propose to add the following text at the end of the legend: “Line sinuosity depicts the percentage of transport that is diffusive.” Also, to link this figure with its justification, we propose:

- a. to note in the legend that its justification is provided in the Appendix; and
- b. to add a final sentence to the Appendix stating that the consequences of Equation A5 are visualized in Figure 4, motivated also by the comments by Dr. Yakir.

This Viewpoint paper offers a provocative perspective of the role of water vapor in atmospheric and leaf-scale gas exchange. It argues that water vapor dynamics (WVD) can be a driver of gas transport phenomena, using first-principles reasoning and thought experiments. The paper challenges the conventional (and useful) practice that expresses gas concentrations relative to dry air, especially under very humid conditions. It raises valid conceptual challenges to current modeling frameworks in both atmospheric chemistry and plant ecophysiology.

Again, we thank the reviewer for this considered assessment of our preprint and for the suggestions that we believe will improve its clarity.