Adhering to the editorial support team's instructions, this author comment (AC) starts a new discussion thread. It begins with a general statement before addressing each reviewer individually, first with a general reply and then with the author's replies to specific referee comments (RC; underlined) including proposed revisions (with some specific text in italics).

General statement

The WCD Ideas category is intended for "innovative and well-founded scientific ideas that have not been comprehensively explored." Comments from each reviewer generally contribute to the inference that the present manuscript meets this criterion:

- Referee #1: "the fundamental observation is correct"
- Referee #2: "a novel perspective"

while criticizing the lack of quantitative assessment. In response, I propose to include a first-order estimate of the throughflow magnitude by revising the first sentence of the paragraph at line 181 as:

Average throughflow velocity magnitudes can be estimated from Eq. (4) using column-integrated values of meridional mass transport (~10 kg m^{-1} s^{-1}*; Liu et al., 2021) and mass (~*10^4 *kg* m^{-2}*), yielding average meridional velocities of order 1 mm* s^{-1}.

This estimate is intentionally limited in scope. Detecting or constraining large-scale millimetre-per-second mean flows is challenging, as such signals do not exceed uncertainties in observationally constrained datasets. Moreover, existing reanalysis products are derived from models that do not explicitly represent source–sink-driven potential flow associated with atmospheric mass exchange, which complicates their use for diagnosing such throughflows.

A more complete quantification would likely require incorporating hydrologically forced potential flow into general circulation models. This lies beyond the scope of the present study, whose aim is to articulate the conceptual framework and motivate further investigation.

Reference

Liu, Q., Li, T., and Zhou, W., 2021, Impacts of multi-timescale circulations on meridional moisture transport, J. Climate, 34, 8065-8085, https://doi.org/10.1175/JCLI-D-20-0126.1

Anonymous Referee #1

General reply: I thank the reviewer for this considered assessment, which generally agrees with that of Referee #2 and represents constructive criticism of my manuscript. I believe that the guidance for updating the bibliography will allow me to improve the basis of my paper but has not undermined the derivation of meridional throughflows. Further comments have, I hope, enabled me to clarify my message.

<u>The paper notes that there is net (water) mass transport by atmospheric circulation and argues that such a throughflow should be accounted for. While the fundamental observation is correct and the author raises some interesting issues regarding the distribution of trace gases in the atmosphere, the proposal to address it through a 'latent' cell is rather unclear.</u>

This concept is illustrated schematically in Figure 3 but could be clarified with revision. To make this coupling more explicit, I propose the following specific revisions.

Revised Figure 3 caption:

Schematic representation of coupled ocean–atmosphere "Latent cells". Dotted lines denote non-gaseous-phase mass transport associated with the hydrological cycle, terminating at regions of net evaporation in the subtropics. These regions act as sources of atmospheric mass, where water vapour enters the atmosphere. Solid lines denote gaseous source–sink flows that originate at these evaporative sources and terminate at regions of net phase change and precipitation (high latitudes and the equator) that remove atmospheric mass and add freshwater to the ocean. Together, these linked source–sink flows form closed circuits across the ocean–atmosphere system. Tropical branches are shown in red and extratropical branches in blue. Adapted from Dey and Döös (2019).

Two new paragraphs to be inserted at line 156 (i.e., just before Fig. 3):

Figure 3 illustrates the proposed coupling between oceanic and atmospheric branches of the hydrological cycle. In the ocean, net freshwater gain through precipitation and runoff defines sources that are balanced by net evaporation in the subtropics, where freshwater is removed. These evaporative regions act simultaneously as sources of atmospheric mass, as water vapour enters the atmosphere. The resulting atmospheric source–sink flow originates in the subtropics *and terminates in regions of net phase change and precipitation that act as sinks of atmospheric mass and return freshwater to the ocean.*

In this way, oceanic and atmospheric transports are linked through the phase changes of water, forming continuous source–sink circuits across the coupled system. The gas-phase segments of these circuits correspond to the throughflows described here, while the liquid-phase segments provide the return pathway required to close the mass budget.

Main comments:

1. Discussion of the atmospheric circulation:

The description of the atmospheric circulation as consisting of three cells superimposed on an eddy "diffusive" flux is rather dated. Multiple authors have provided insight into the global circulation and, in particular, on the role of the hydrological cycles (see Pauluis et al. Science 2008, and Lalilberte etal Science 2015, among others). There is also extensive literature on 'freshwater transport' by the ocean circulation (see Schanze et al., Journal of Marine Research, 2010).

I thank the reviewer (and also Referee #2) for the assistance in updating the literature review and propose to add the following two paragraphs at the end of the introduction (line 38), as well as corresponding references to the bibliography.

In recent years, substantial progress has been made in describing the global atmospheric circulation using thermodynamic coordinates. In particular, Pauluis et al. (2008) and Laliberté et al. (2015) introduced formulations based on potential temperature and equivalent potential temperature that provide physically insightful descriptions of mass and energy transport. These approaches reveal circulation aspects that are not apparent in conventional Eulerian frameworks and have clarified the role of moist processes in shaping large-scale transport.

However, these formulations define time-mean streamfunctions that vanish at the domain boundaries and therefore describe recirculating flows with zero net mass transport across latitude circles. As such, they do not explicitly represent source–sink-driven throughflow arising from spatially structured mass exchange with the surface. The present work complements these approaches by focusing on this component of the circulation.

Later in the paper, it seems appropriate to refer back to these recent advances in two locations. I therefore propose to insert the following paragraph at line 112:

This perspective differs from recent thermodynamic-coordinate analyses (e.g., Pauluis et al., 2008; Laliberté et al., 2015), which describe atmospheric transport using streamfunctions that vanish at the boundaries and therefore represent closed recirculations. While highly effective for diagnosing energy and mass redistribution, such formulations do not capture throughflow induced by net mass sources and sinks.

Also, I propose to add the following sentence to the end of the conclusions section:

An important direction for future work is to examine how hydrologically driven throughflow might be represented within thermodynamic-coordinate frameworks such as those developed by Pauluis et al. (2008) and Laliberté et al. (2015).

Finally, the Schanze et al. (2010) article is also a welcome addition to the references, and I propose to cite it at line 108 where global distributions of net evaporation and precipitation are mentioned.

2. What exactly is the author's proposal for a throughflow?

While everyone would agree that there is indeed net transport of (water) mass by the atmospheric circulation, this transport is small when compared to overall mass transport. The author proposed addressing this by adding a throughflow. Less clear is how the author would estimate it, or whether it would have a significant impact on circulation itself. (Abeit the author seems to concede that the impact of the throughflow would be minor, "the direct contribution of atmospheric throughflow to the meridional transport of water vapour itself is minor" on line 180.)

The issue here is that there is a simple fix to the problem raised by the author: Given that there is no mass transport of 'dry air', one solution is to compute the circulation in terms of dry-air mass transport. This circulation would be 99% similar to the circulation obtained using the 'moist air transport', and would not involve any throughflow . In this context, the paper would benefit from some modicum of computations. Reanalysis datasets are widely available and should be used to provide preliminary results.

A first-order estimate of throughflow magnitude will be provided in revision (see General Statement above) with mean meridional velocities on the order of $1\ mm\ s^{-1}$.

While small relative to typical winds, this magnitude is consistent with large-scale, time-averaged mass transport and is critical to describe the mechanisms that convey relatively inert constituents like oxygen (O_2). While the suggestion to compute circulation in terms of dry-air mass transport yields flows very similar to those obtained using moist air, such a dry-air framework effectively removes the influence of the hydrological cycle and obscures the dominant transport mechanisms that arise from humidity-driven mass sources and sinks. Dry-air formulations suppose O_2 to vary by just 10 ppm against a background of 210000 ppm, or practically negligibly (e.g., Adcock et al., 2023). True O_2 concentration differences are orders of magnitude greater (Kowalski and García-Valdecasas Ojeda, 2026) and drive massive O_2 diffusion up humidity gradients. This implies substantial compensating transport, which is naturally interpreted within a source–sink (throughflow) framework.

Please see the General Comment above regarding the nature of reanalysis "data".

Reference:

Adcock et al. (2023) https://doi.org/10.5194/essd-15-5183-2023

3. The discussion of trace gas distribution is welcome and could be more detailed.

The more interesting part of the paper is the discussion of trace gas distributions at the end of section 4. The discussion is fairly informal and lacks a clear testable hypothesis that would motivate the use of a thorough flow. At one point, the author claims that "Throughflow therefore dominates the net meridional transport budget of argon in the midlatitudes, even though it is not readily apparent in conventional circulation diagnostics". While this is an interesting claim, it is not backed by any data or supporting evidence.

I propose to revise the sentence for clarity as follows:

Throughflow therefore dominates the net meridional transport mechanisms acting on argon in the midlatitudes, even though it is not readily apparent in conventional circulation diagnostics.

A long-standing, testable hypothesis holds that redistribution processes—molecular, turbulent, or synoptic (e.g., "diffusion" by baroclinic eddies)—transport gases down their concentration gradients, with net fluxes in certain directions proportional to those gradients (i.e., neglecting non-diffusive transport by throughflow). While this framework has been applied successfully to transport in plant stomata and the atmospheric boundary layer—and across a global observation network used for inversion modelling (Bousquet et al., 1999)—it does not adequately describe O_2 dynamics. In this case, gradients are comparable in magnitude to those of water vapor, despite O_2 sharing stoichiometric links with CO_2 (Kowalski and García-Valdecasas Ojeda, 2026).

At microscopic scales, throughflow enables photosynthetically produced O_2 to exit leaf pores, overwhelming counter-current molecular diffusion (Kowalski, 2025). In the boundary layer, throughflow likewise transports O_2 up its gradient, opposed by downward turbulent diffusion that is driven by evaporative dilution (Kowalski et al., 2021). At the global scale, O_2 gradients and diffusive fluxes exceed those of CO_2 by orders of magnitude; neglect of the hydrological cycle's influence on O_2 transport mechanisms—including non-diffusive transport by meridional throughflow—significantly skewed characterisations of the global O_2 cycle (Kowalski and García-Valdecasas Ojeda, 2026).

For argon, the supporting evidence for the existence of throughflow is that both baroclinic eddies and Ferrel-cell overturning drive equatorward transport in the midlatitudes, yet persistent meridional gradients indicate a compensating poleward transport mechanism, consistent with the proposed throughflow.

Consistent with my General Statement above, I propose to add such supporting evidence to the manuscript, revising a large chunk of text that began with the first paragraph on page 8 (lines 181-207) with the following:

Average throughflow velocity magnitudes can be estimated from Eq. (4) using column-integrated values of meridional mass transport (~10 kg m^{-1} s^{-1}; Liu et al., 2021) and mass (~10^4 kg m^{-2}), yielding average meridional velocities of order 1 mm s^{-1}. Although modest in magnitude, their significance becomes apparent when considering transport of inert or nearly inert constituents. As shown for oxygen by Kowalski and García-Valdecasas Ojeda (2026), large-scale concentration gradients of certain atmospheric constituents can arise primarily from dilution by water vapour rather than from their own sources and sinks. Because increases in humidity reduce the partial pressures of dry-air components, regions of high q are systematically depleted in inert or weakly reactive gases, while dry regions are relatively enriched. Humidification produces meridional concentration gradients across the midlatitudes that are largely independent of the intrinsic biogeochemical cycling of the constituent.

Argon provides a particularly clear example because it is effectively inert. In the midlatitudes, both baroclinic eddies and Ferrel-cell overturning transport argon equatorward, driven by humidity-induced concentration gradients. These gradients—far larger than those of any trace gas, including carbon dioxide—drive strong *equatorward "diffusive" transport of argon by synoptic-scale motions, independent of any net meridional mass flow. Ferrel-cell overturning further contributes to equatorward transport, as relatively dry, argon-rich air moves equatorward aloft while more humid, argon-poor air moves poleward near the surface. Despite these tandem mechanisms transporting argon equatorward, argon remains enriched in cold, dry polar air and depleted in warm, humid regions.*

The persistence of this argon gradient implies a compensating poleward transport, which is naturally interpreted as non-diffusive conveyance by the atmospheric branch of the Latent cell that balances—and approximately equals—the combined equatorward transports by eddies and recirculation. Throughflow therefore dominates the net meridional transport mechanisms acting on argon in the midlatitudes, even though it is not readily apparent in conventional circulation diagnostics.

Closely analogous behaviour is observed for oxygen, notwithstanding its stoichiometric similarity to carbon dioxide. Although biologically active, oxygen is effectively inert with respect to its atmospheric abundance, and its large-scale distribution is primarily determined by humidity (Kowalski and García-Valdecasas Ojeda, 2026). Relative to its familiar concentration of 20.95% in cold, dry air, oxygen is suppressed by many thousands of parts per million in warm, humid tropical and subtropical environments (Kowalski et al., 2025), regardless of its own surface sources and sinks. These strong gradients drive equatorward "diffusion" through the Ferrel cell that is orders of magnitude greater than that of carbon dioxide.

Oxygen is also transported equatorward by Ferrel-cell overturning, owing to humidity contrasts between its meridional branches, in much the same way as argon. As with argon, these equatorward transports by recirculation and eddy "diffusion" are nearly offset by poleward, non-diffusive conveyance associated with atmospheric throughflow. Unlike argon, however, the small residual transport reflects the influence of oxygen's surface sources and sinks, which slightly perturb the balance among large, nearly compensating transport mechanisms.

References:

Bousquet et al., (1999) Inverse modelling of annual atmospheric CO2 sources and sinks 1. Method and control inversion, J. Geophys. Res., 104, 26161– 26178

Kowalski, A.S. (2025) An elucidatory model of oxygen's partial pressure inside substomatal cavities, Biogeosciences, 22, 785-789, https://doi.org/10.5194/bg-22-785-2025

Kowalski, A. S. and García-Valdecasas Ojeda, M. (2026) Global oxygen distributions at the Earth's surface, Science of the Total Environment, 1031, 181809, https://authors.elsevier.com/sd/article/S0048-9697(26)00473-0

Anonymous Referee #2

General reply: This reviewer offered constructive criticism that generally agreed with that of Referee #1 and furthermore identified conceptual errors by the author that influenced the manuscript's organization and presentation, but not its deductions regarding throughflow.

As I will explain below, I feel extremely torn about the value of this paper. On one hand, the author highlights the intriguing fact that the atmospheric circulation drives a net meridional transport of water. Streamlines of meridional mass transport in the atmosphere do not close, unless one accounts for the return of water vapor by the ocean! This is a novel perspective that fits well with the WCD Ideas goal of raising interesting questions to the weather and climate community.

On the other hand, I am concerned by the near utter lack of quantitative analysis. I feel it is important that the author provide at least a rough estimate of the magnitude of these effects. Lacking something quantitative, the reader is left wondering if this is an important effect missing in our understanding and/and or ability to model the atmosphere, or a miniscule effect overwhelmed by uncertainties in other processes.

As a possible example of what I mean, the Earth is an oblate sphere. Should we be concerned by models that treat it as a sphere, or is this effect safely well below the rounding error/other uncertainties? This was considered in Gates 2004 (https://doi.org/10.1175/1520-0469(2004)061<2478:DOTEOA>2.0.CO;2,), who quantifies the effects in section 7 and table 1. [This is not meant as a request to cite this paper, rather, an example of how to quantify a neglected effect!] Quantitative estimation matters a lot: there are many things one could worry about: deviation of air from the ideal gas law, non-traditional Coriolis terms, Cosmic rays, etc., but some things matter and others don't.

I agree that quantitative context is important. The revised manuscript will include a back-of-the-envelope estimate of mean throughflow velocities (see General Statement), yielding values on the order of millimetres per second.

While small, these velocities are consistent with large-scale, time-averaged transport and are sufficient to influence the distribution of inert or weakly reactive constituents.

The intent here is to establish order-of-magnitude relevance rather than provide a full quantitative characterization, which would require dedicated modelling.

I think it should be straightforward to estimate the magnitudes of the latent cells and compare them to the mass transport by the atmosphere. Based on precipitation less evaporation, P-E, which could be taken from a model or reanalysis (albeit with caution), the net transport of water could be estimated and compared the transport by the mean meridional overturning cells and lagrangian transport by eddies (e.g., the TEM circulation, or isentropic circulation).

Reanalysis products provide valuable constraints on atmospheric transport, and I agree they could offer useful context. However, because they are derived from models that do not explicitly represent source–sink-driven potential flow associated with atmospheric mass exchange, their ability to diagnose such throughflows is limited.

Another way to bolster the quantitative elements of the proposal would be to show more about the argon and oxygen fields. I was intrigued by the gradients in these species created by latent cells, and maps of their concentration relative to the latent overturning would be exciting to see.

Additional quantitative support for the proposed mechanisms is provided in Kowalski and García-Valdecasas Ojeda (2026), which demonstrates that O_2 distributions are primarily controlled by humidity. The resulting spatial patterns—and associated gradients—are representative of all gases whose concentrations are governed by dilution (including inert gases such as argon).

I propose to clarify this connection as indicated in the above reply to the final main comment by Referee #1.

Lacking something quantitative, I am hesitant to recommend publication of this article. To be constructive, I've made several suggestions/comments below for the author to consider.

1. As discussed above, I feel the paper needs at least a back of the envelope estimate of the magnitude of these cells, and/or more quantitative discussion of the argon and oxygen gradients.

Please see the General Comment above for the back-of-the-envelope quantification of average meridional throughflow velocities.

The net transport of water vapor by the atmosphere is also intimately connected to energy transport, which can in turn be linked to mass transport by considering the circulation averaged on dry and moist isentropes (a more Lagrangian perspective). I think it would help to link the latent cells to energy transport, an even more fundamental aspect of the atmospheric circulation, or at least make a connection to this body of work.

The connection between throughflow and energy transport is an interesting and important question. However, a rigorous treatment would require extending the framework beyond the scope of the present manuscript. I have therefore chosen to limit the discussion to mass transport and constituent distributions, while noting this as a direction for future work.

2. For example, there is a series of nice work conducted by Pauluis and coauthors that consider the transport of mass by the circulation in potential temperature and equivalent potential temperature coordinates, in particular Pauluis et al. 2008 (10.1126/science.115964). They show that the amount of mass moved by the atmosphere from the equator to the pole (and back again) effectively doubles when you account for the fact that poleward moving air carries moisture relative to the returning, drier equatorward flow.

Please see the reply to Main Comment 1 of Reviewer #1 (above) regarding the Pauluis et al. (2008) and Laliberté et al. (2015) formulations.

3. I did not find Figure 1 to be helpful; I think all readers will know what convergent/divergent flow looks like. This is just my opinion, but I felt the paper started too slowly, to the point of insulting the reader, and would appreciate jumping more quickly to the latent cells.

I agree that Figure 1 is not essential and am prepared to remove it. A brief textual reference to standard fluid dynamics treatments of source–sink flow can be included instead.

If the paper develops gradually, it is because the prevailing paradigm in studies of surface gas exchange largely neglects non-diffusive transport, making it necessary to first establish several principles related to potential flows. Early work describing hydrologically induced gas transport in the atmosphere (Kowalski, 2017) was rejected by multiple journals and to date has received little attention within atmospheric dynamics. Progress has resumed only more recently, driven by studies of O_2, for which the importance of non-diffusive transport is particularly evident.

I appreciate the goal of Figure 2, but I also found that the schematic didn't help much beyond the description in the text, which was informative. The problem is that the schematic diagram doesn't include the essential sink and source elements, so I don't see exactly how it helps.

This figure helps to illustrate the fact that, with rising fluid in the container as a consequence of the water source at the bottom, any eventual spill will include oil. In panel (a) the initial spill is pure oil, while in panel (b) it is only partly oil. Also, in the case with evaporation compensating the injection of water at the bottom, panel (b) helps to visualize the oil appearing as stationary.

To be constructive, I did like Figure 3 and would encourage the author to move it up in the text. This more quickly gets the reader thinking about the latent cells. Following up this schematic with an rough estimate of the net transport would be very nice, in my opinion.

This figure has been moved up in the text.

Finally, here are a few points in the text where I believe I misunderstood the author. I point them out not to be critical, but to encourage the author to be more precise.

I have tried to adopt the reviewer's suggestions (below) and improve the text as a consequence of these points raised by the reviewer.

21-22 Baroclinic eddies are by no means the "smallest scale" of the atmosphere, nor of the transport of water vapor. Convective systems transport water, from the scale of an individual cloud to a tropical cyclone.

The context here, "meridional transport processes of global relevance", was established in the previous paragraph. Nonetheless, this sentence has been modified to begin: "At the smallest scales considered here".

The notion of a “least degree of organization” is also vague. Baroclinic systems are very effective at transporting sensible and latent energy poleward. To argue that this transport/precess is “less organized” than the Ferrell or Polar Cells seems a matter of semantics. It is know that baroclinic systems transport far more mass than the Ferrel or Polar Cells. The polar cell is very weak, to the point that I believe one needs multi-year averages to see it. I was in general surprised by the amount of time discussing the polar cell throughout the paper given how small and insignificant it is relative to the eddy transport.

23 Baroclinic eddies transport angular momentum. This understanding dates back to Rossby, I think, and is textbook material, e.g., Vallis (2017) Chapter 15. I believe I misunderstand what the author meant by this statement.

The referee has identified a conceptual error made by the author, which influenced the organization and presentation of the manuscript—and thereby its clarity and accuracy—but not its primary deductions regarding throughflow. The concept of “degree of organization” was based on entropy considerations and arose from the spontaneous and irreversible nature of diffusion, which transports fluid properties via essentially random redistribution from areas of abundance towards those of scarcity. The author erroneously attributed this to “diffusion” by baroclinic eddies considered as closed circulations.

To remedy this misinterpretation, I propose to recast the opening paragraphs of section 1 to describe a spectrum that increases in spatial scale and delineates dynamically distinct transport mechanisms but no longer mentions “degree of organization” or transport of angular momentum.

References to the Polar cell will be limited to those where all three types of cells are mentioned.

28 Baroclinic eddies transport momentum upgradient, to generate the westerly surface winds.

Please see the reply to the previous comment.

29 The Hadley cell is a better example of a circulation which transports humidity upgradient.

The Hadley cell will be mentioned here instead of the Polar cell.

30 I don't understand what the author means by no net momentum? The Hadley cell advects angular momentum poleward, manifested the subtropical jets. I think I misunderstand what kind of momentum the author means?

I propose to add the adjective "linear" here.

35 I appreciate that the latent cells are hemispheric in scale, but in the midlatitudes I believe that the transport of water vapor is dominated by baroclinic eddies. I think it would be a mistake to think of these latent cells in a zonal mean sense.

This sentence refers to the existence and scale of throughflow, not its relevance to transporting water vapor. Indeed, the manuscript makes clear at line 179 that its contribution to "the meridional transport of water vapour itself is minor". This is easy to misconstrue because the throughflow exists due to water vapour dynamics, while throughflow transport of water vapour itself is minor.

I believe the latent cells exist in the zonal mean, but not that they determine—or even strongly constrain—the flow at every longitude along a latitude circle. Even in the time average, flow at a given longitude may be either poleward or equatorward, depending strongly on semi-permanent pressure systems; for example, flow is predominantly poleward west of the Azores High and equatorward to its east. Persistent meridional throughflow emerges in midlatitudes only after zonal integration. I believe the same is true of overturning circulations such as the Ferrel cell, and it would therefore be misleading to regard latent cells as zonally uniform. The key distinction being made here concerns mechanisms of transport, a point I intend to clarify in the revised opening paragraphs.

152-155 Again, I think this description implies that the meridional transport in the atmosphere is dominated by the zonal mean cells. This is somewhat true for the Hadley cell, but demonstrably incorrect for the Ferrel and Polar Cells. I strongly encourage the author to emphasize that this meridional transport is not effected by the zonal mean circulation, but rather dominated by eddy transport.

I think it is important to be specific here about what mass is being transported, and to distinguish between the mass of air and the masses of its components. I agree that eddy transport dominates the latter for some components, such as water vapour. To address the reviewer's comment, I propose to revise this paragraph as follows.

From this perspective, the meridional component of the atmospheric general circulation is not purely cellular. Instead, it may be viewed as a hemispheric-scale throughflow (Fig. 3) within which familiar recirculating structures—the Hadley, Ferrel, and Polar cells—appear as embedded, quasi-stationary eddies. Likewise, the baroclinic eddies—whose stirring is so important for meridional transport of some constituents—are embedded in flow that is not purely zonal. These closed-streamline circulations remain dynamically important, but they operate within, rather than define, the background transport of mass established by hydrologically driven potential flow.

Section 4 discusses the hydrological cycle. It's begins a very basic level, but never seems to connect with the large body of work in the atmospheric literature on how to define mass transport. Equations 3 and 4 take baby steps towards the idea of a residual mean circulation, but never get past the idea of a single parcel.

Equations 3 and 4 do not refer to a single parcel. They derive from Equations 1 and 2, which were introduced to describe the atmosphere's meridional momentum, and are applied to a control volume whose scale is left indeterminate with intent.

180 I acknowledge that the author is getting to the point that eddies dominate the transport of water in the atmosphere, but it's a rather oblique way of getting to this important fact.

The first sentence of this paragraph does not seem oblique: "For water vapour itself, meridional transport associated with throughflow is readily quantified and is relatively modest."

197-203 I found this part of the manuscript intriguing – and getting near the point of being quantitative – but feel that I don't fully understand it. (I point this out with humility, that this is a section where the author could help a reader like me.) I don't fully understand why O2 is transported much more than CO2. Is this because there is vastly more O2, and thus the photosynthesis and cellular respiration thus have a stronger relative impact on CO2? Or is it because there are fossil fuel sources of CO2 (whose burning would affect O2, but again, the relative abundance is very different.) Quantifying the effect of biological vs. latent cells on O2 and CO2 would have helped me.

The reviewer has understood correctly that the same throughflow transports different gases as a function of their abundances (with O_2 about 500 times more abundant than CO_2). Similarly, dilution by water vapour is much greater for O_2—which can be reduced by thousands of ppm—than for CO_2, and thereby more importantly determines its diffusive transport. Thus, the hydrological cycle's actuation of opposing diffusive and non-diffusive transport mechanisms is very different for different gases.

Kowalski and García-Valdecasas Ojeda (2026) address this issue in detail and Kowalski et al. (2025) summarise it (publication chronology does not reflect that of submission). Rather than repeating it again, I propose in revision to cite the key conclusions of Kowalski and García-Valdecasas Ojeda (2026) regarding the relevance of throughflow transport to different gases as indicated in the reply to the third "Main comment" of Referee #1.

209 Why is the author talking about linear momentum when it is angular momentum that is conserved? I again refer to text book explanations of the momentum transport by the eddies and mean flow, mentioned above. I think I'm a bit confused, because the first part of the paragraph talks about weak zonal winds in the subtropic (implying less momentum) and then switches to a discussion of the abundant angular momentum. I think the switch from linear to angular momentum makes this overly confusing.

Conservation equations can be written for both linear and angular momentum (Holton, 2004). To avoid their confusion, I propose revising the paragraph in question as follows:

The implications of hemispheric-scale meridional throughflow extend beyond atmospheric composition and can be illustrated by contrasting linear and angular momentum advection in the midlatitudes. Poleward mass transport through the Ferrel cell carries linear momentum from subtropical regions of relatively weak zonal winds toward higher latitudes dominated by strong westerlies, corresponding to negative advection. Angular momentum behaves differently: poleward throughflow transports it from the subtropics, where it is relatively large, toward the planetary rotation axis, where it vanishes, implying positive advection.

Classical treatments of meridional momentum transport generally exclude such behaviour by assuming zero net mass flux across latitude circles (e.g., Holopainen, *1967; Lindzen and Hou, 1988). While appropriate for recirculating frameworks, these assumptions may require reconsideration in the presence of a persistent, hydrologically driven background throughflow.*