

Consistent with previous rounds of review and revision, I have copied the referee's comments below (underlined text), followed by my point-by-point replies (normal text) including descriptions of my revisions that are included in the marked-up manuscript version showing the changes made.

Referee #1

Prior to addressing the comments from the referee, I want to point out a foundational aspect of classical mechanics that seems to be often overlooked. In the first few paragraphs of his *Principia*, Isaac Newton was specific regarding two points that are fundamental to atmospheric dynamics:

1. Motion is quantified (*quantitas motus*) as the product of mass with velocity, or what we now call momentum; and
2. The motion of the whole is the sum of the motions of all the parts.

Osborne Reynolds (1895) respected these concepts when defining the average fluid velocity as the ratio of average momentum to average mass: $\langle wp \rangle / \langle \rho \rangle$. Such a definition—which amounts to density weighting when averaging velocities—can be found in Eq. (4) on page 134, between Eqs. (8a) and (8b) on page 135, and in Eq. (12) on page 140 of this classic paper. Because this is often misattributed, I point out that it is available here with open access:

<https://royalsocietypublishing.org/rsta/article/doi/10.1098/rsta.1895.0004/108119/IV-On-the-dynamical-theory-of-incompressible>

According to Newton and Reynolds, correlations between temperature and velocity are included in the definition of average (or bulk) fluid motion, precluding the possibility that eddies can move mass. Rather, they merely reorder constituents towards uniform composition.

I remain torn on this contribution. As I said in my first review, it is thought provoking, presenting something new to the atmospheric dynamics and circulation community. I also appreciate the author's changes to the text and efforts to be more quantitative. As detailed below, I still think significant changes are required. First, to moderate the implications, and second, to ensure that the schematic doesn't imply detailed differences in the transport with height that may not be justified.

I thank the referee for the additional review and constructive suggestions. The comments highlighted two areas that required clarification. First, meridional transport by individual *branches* of circulations—with different consequences for specific atmospheric constituents—is distinct from the meridional transport of air itself. Second, the schematic in Fig. 2 is intended only as a conceptual illustration, adapted from Dey and Döös (2019), and not as a detailed representation of vertical transport pathways. I have revised the manuscript accordingly.

Major suggestions

1) My main concern from the first draft was the lack of quantitative analysis. This has improved in this draft, but the implications of the analysis are missing. In particular, the abstract suggests that the water vapor conducts “significant” mass transport and “influences the atmospheric momentum budget”. In terms of mass transport, “Significant” could be interpreted to mean large or to mean statistically distinguishable. The latter may be true, but not the former. The throughflow is 1000 times smaller than the net recirculation of the atmosphere associated with the transport of energy poleward. No evidence is provided to suggest that this 0.1% effect has a substantial impact on the momentum budget of the atmosphere.

The relevant sentence in the abstract has been revised to clarify that the discussion concerns transport of atmospheric constituents rather than bulk transport of air. The term “significant” has also been removed to avoid ambiguity.

Regarding momentum transport, I believe it is important to distinguish between recirculating transports, whose opposing branches cancel in the net mass budget of air at each latitude, and non-recirculating transports associated with throughflow. My intention was not to quantify the magnitude of any momentum-budget effect, but only to note that a net mass transport of air may have dynamical implications. To avoid overstating the case, I have retained cautious wording in the revised manuscript.

To be specific, if the net moisture flux is of order 10 kg/m/s, then the total meridional mass transport is $2 \pi r_0 \cos(\text{lat}) \times 10$, or about 3×10^8 kg/s at 45 degrees. In comparison, the total transport of mass by the atmospheric circulation is approximately $1\text{--}2 \times 10^{11}$ kg/s (See Pauluis et al. Fig 2). Thus the latent cells are nearly 1000 times smaller. This scaling is consistent with the velocities, where I would say that meridional transport is of order 1 m/s, compared to the 1 mm/s for the latent cell.

Thus, to see the unclosed streamlines in a diagram of the meridional overturning, one would need extremely fine scale, accurate data to detect it. Speaking from experience, I think numerical errors in computing the meridional circulation will dwarf the error associated with neglecting the source sink flow, and it's not a major issue that most past studies have assumed that the streamfunction has zero boundary conditions at the top and bottom.

Similarly, I think it's hard to argue that we should primarily think of the circulation as a latent cell, with familiar atmospheric features embedded within it. Rather, these familiar features are 1000 times larger than the latent cell. The latent cell can only be quantified with very careful analysis. The overturning circulation primarily moves mass poleward at upper levels / higher energy, with the return flow at lower levels / lower energy, and this is a 0.1 percent correction.

I agree that the magnitude of the throughflow is small relative to the dominant overturning circulations and may be difficult to diagnose directly. However, the massive meridional transports by individual branches of the atmospheric circulation in Fig. 2 of Pauluis et al. (2008) occur in both directions and offset when averaged vertically. As noted at line 45, the Pauluis et al. formulation defines “time-mean streamfunctions that vanish at the domain boundaries and therefore describe recirculating flows with zero net mass transport across latitude circles.”

My purpose is not to suggest that previous analyses are invalid, but rather to identify a distinct transport component that is not represented by streamfunctions constrained to have zero net transport across latitude circles.

In the earlier draft, it was my understanding that the author argued that the required mass transport of inert species (specifically Ar and O₂) by the throughflow was significant for their distribution. In the revised draft, am I correct to understand the primary influence on the distribution of these gases is dilution by evaporation, and the throughflow balances the diffusion down this gradient? (I include a specific comment about this below.)

Yes. My interpretation is that evaporation both dilutes and displaces dry-air constituents, generating compensating diffusive and non-diffusive transports. This is stated at line 149 with reference to another paper that explains it in detail.

All this is not to say that the latent cell isn't interesting in its own right. It is transporting the same order of magnitude of mass of the upper branch of the Brewer-Dobson Circulation of the stratosphere. I just don't think the latent cells need to be justified by implying there's a significant error in the analysis of the meridional overturning circulation in text books, etc.

I agree that the latent circulation is best viewed as complementary to conventional descriptions of atmospheric circulation. The revised manuscript emphasizes this perspective and avoids implying that established frameworks are fundamentally incorrect.

2) I regret that I didn't pay close attention to the key Figure in the first revision. I would like to ask about a few features implied by the figure that are not discussed or justified in the text.

First, the tropical (red) cell is drawn in a way that appears to counteract the Hadley cell (e.g., counterclockwise in the northern hemisphere). Is there evidence that evaporated water actually moves *poleward* and upward before moving equatorward at height? I would have assumed that the moisture transport is primarily in the bottom branch of the Hadley cell, and where it primarily moves equatorward and slowly upward until it condenses somewhere in the tropical lower stratosphere. The dashed curves in the atmosphere indicate the return of liquid water, right? If so, should it be straight down in the atmosphere?

Second, is there any evidence of an equatorward transport of moisture in the extratropical cell, above the tropical cell that then shifts back poleward, as implied by the "S" shape of the blue cell in the subtropics? Naively, I would have thought that moisture is primarily transported slantwise (poleward and upward). As remarked above, I think the dashed lines shouldn't extend further poleward than the solid (water vapor) lines, as there's no mechanism for source sink flow past the point where the moisture rains out? If the dashed curves indicate rain, shouldn't it fall straight down (at least on this scale)?

Lastly, I'm not an oceanographer, but is it true that the oceanic transport goes deeper in the tropics?

To be constructive, it might help to include a sketch of the tropopause. At first I was puzzled why the tropical cell was shallower than the extratropical cell, but I think this is meant to imply that the tropical cell is primarily shallow (though tropical convection is deep: I suspect some condensation in the tropics occurs higher than in the extratropics). This could be emphasized by sketching the tropopause in the plot, to give a sense of the vertical structure.

In addition, I appreciate that this is meant to be a schematic, so perhaps making it more idealized would be helpful. If there is no evidence of the S-curve of moisture in the blue extratropical cell, perhaps just draw an idealized half cell, e.g., the top half of an oval, which connects with the return flow in the ocean below to form an oval.

In terms of the ocean I would only show changes in depth of the return flow if there is evidence that there is definite vertical structure to the flow.

I appreciate these detailed observations regarding Fig. 2. The figure was intended only as a schematic illustration of the conceptual framework and was adapted directly from Dey and Döös (2019). The specific vertical structures, including the subtropical S-shape and oceanic depth variations, were not intended as new interpretations or claims. To avoid over-interpretation, I have added text at line 189 clarifying that these details should not be interpreted quantitatively.

3) A final suggestion. It might be good to note that while the latent cell does show the net transport of water vapor by the atmosphere, it may not indicate what actually happens to a given water molecule. I suspect that moisture is continually being evaporated, rained out, and re-evaporated along the march poleward. The majority of this transport is effected by the baroclinic eddies (the diffusive transport), and so-called atmospheric rivers (the moist, poleward transport on the flank of a low pressure systems). These streamlines are thus not well captured by the Eulerian mean circulation, but by the TEM circulation (or the circulation in isentropic coordinates, as explored by Pauluis et al.).

I agree. The latent-cell framework describes the net transport associated with the hydrological cycle and does not imply that individual water molecules follow the depicted pathways. A clarifying sentence, quite similar to that of Dey and Döös (2019), has been added to the manuscript at line 190.

I think my problem might be confusion over what is meant by diffusion vs. through flow. I think that water vapor is moved by eddies e.g., “diffusive” transport at line 26. Other inert gases (Ar, O₂) but with net transport up the gradient created by dilution, which is balanced by diffusion of the dry constituents backwards. Don’t both directions involve diffusive transport by the nomenclature introduced in the paper,. Please pardon me if this reflects my confusion, and this comment is meant to help the author explain what is meant.

I appreciate the reviewer’s observation. The intended distinction is between diffusive transport, which acts down concentration gradients, and non-diffusive transport associated with throughflow. Both processes contribute to constituent transport, and I have added a phrase at line 27 to try to make this distinction clearer.

Small comments by line number

12 I do not feel that evidence has been shown that latent cells alter the momentum budget of the atmosphere. The meaning of “significant” mass transport is unclear, but given that it’s 1000 times smaller than the transport by the atmosphere, I’m unsure this is a fair characterization.

As noted above, the word “significant” has been removed and the type of mass transport has been clarified.

23 I am not sure what is meant by “bulk flow” here. Given correlations between temperature and velocity, the eddies move mass, as characterized by the TEM circulation. The key is that they are not random in the sense that poleward flow is associated with more energetic (warmer/moister) air, while equatorward flow is associated with colder/drier air. It is this correlation that allows them to transport mass and energy.

“Bulk flow” is used here in the fluid-dynamics sense of collective fluid motion that transports all constituents together. To reduce ambiguity, I have added the term “bulk” at line 36 in the description of throughflow.

29-30 Could you emphasize that these flows possess no net *meridional* momentum when averaged vertically? I think this is what you mean.

Note that the Hadley cell does transport zonal momentum because the air in the poleward flow aloft moves eastward relative to the low level equatorward flow. I know the author didn't mean this, but in the theory for the Hadley cell, the field normally focuses on zonal momentum.

This part of Section 1 has been modified as requested (line 32)

36 I don't mean to dwell on semantics, but while the latent cells are hemispheric in total scale, I suspect a lot of this mass flux is driven by baroclinic eddies. As I suggested in major comment 3, water is likely evaporated and condensed, and reevaporated a few times on the way from the subtropics to the tropics.

The point being made at line 36 concerns the transport of air rather than the detailed phase history of individual water molecules. In the flow decomposition by Reynolds (1895) that respects Newton's definition of motion, eddies do not transport air. Rather, their streamlines close because while they transport water vapour in one direction, and an equal mass of dry air in the other.

63 I am not sure if this is a good analogy for the atmosphere, where the other species (N₂, O₂, etc.) cannot spill out. Only water can be injected and removed. As the atmosphere doesn't have a lid to spill, in this case it would just increase in mass until there came a pathway for the water to get out.

I agree that the analogy is imperfect. Its purpose is simply to illustrate how sources and sinks of one constituent can induce transport of other constituents. This point is made in the opening sentence of the paragraph.

The mixed case is a better analogy, but again it's imperfect in that only water is allowed to fall out (condense) at the top.

No analogy is perfect. The key point to appreciate here is the influence of one components sources/sinks on the transport mechanisms of other components.

138-9 I agree that assuming the streamfunction vanishes at the top and bottom is incorrect, but as discussed in point 1, the error is 0.1 percent, and thus it's a very good

assumption relative to other uncertainties (e.g., how well we know the circulation in a reanalysis product).

My interpretation differs regarding the appropriate reference scale for this comparison (i.e., the distinction between air transport by a circulation versus its branches). To avoid focusing on a specific percentage estimate, I have clarified the distinction between recirculating and non-recirculating transports in the manuscript.

196 Does the author mean the “net transport” of water vapour transport? This poleward transport of water vapor could be effected by baroclinic eddies, which the author has termed a diffusive transport. See my confusion as expressed in major comment 3.

I may have misunderstood this comment. My interpretation is that it concerns the distinction between diffusive and throughflow transport mechanisms. I hope the revisions made in response to Major Comment 3 clarify this point.

212-222 Do I understand correctly that the concentration of Argon is largely determined by the dilution: when water evaporates into dry air, it must displace some of the dry air, so that the concentration of Ar (in kg Argon/kg air) is reduced. This creates a gradient in Argon, which will diffuse from cold/dry air to warm/moist air. Am I correct to say that this diffusion is balanced by the net return of Argon associated with the latent circulation through flow?

Exactly.

If this is the case, I don't understand how one can say that the throughflow dominates the net meridional transport. In the climatological mean, there is no net transport of Argon: the down gradient diffusion must be balanced by the through flow.

There are three transport mechanisms described in this paragraph. As stated at lines 217-218, “both baroclinic eddies and Ferrel-cell overturning transport argon equatorward”. However, these tandem processes are offset by the lone mechanism, throughflow, that transports argon poleward. For this to be the case, throughflow transport must be greater than each of the other two mechanisms, and is therefore the dominant transport mechanism.

214 Consider omitting “strong”, as the strength of this diffusion is never quantified. Strong relative to what?

The sentence in question compares argon gradients and “diffusive” transport to those of trace gases like carbon dioxide. In that specific context and sentence, I think the word “strong” can be understood as relative to carbon dioxide. Argon is diluted by hundreds of ppm in tropical versus arctic airmasses, while carbon dioxide rarely varies by more than dozens of ppm.

225-8 Oxygen is diluted on the order of 0.1% (i.e., thousands of parts per million) by evaporation. This causes a diffusion downgradient, which must be balanced by the throughflow back.

I understand that the gradient of O₂ in kg/kg is larger than the gradient of CO₂ because there is more O₂, but the relative effect of dilution on CO₂ is the same, right? I suspect CO₂ is likely more influenced by biology (and human activity, burning fossil fuels), which induce sinks and sources that are large relative to the dilution effect of water vapor.

I agree with the reviewer’s interpretation. Because this issue is peripheral to the main focus of the manuscript, I have not added further discussion.

234-242 Please eliminate these paragraphs. As the transport associated with the latent cells is a thousand times less than that associated with the TEM / isentropic circulation, I think its contribution to the momentum budget is insignificant. Uncertainties in surface drag and convective momentum transport would dwarf any errors associated with throughflow.

I respectfully differ from the reviewer’s assessment and therefore prefer to retain these paragraphs. However, the wording remains deliberately cautious, emphasizing only that the implications may warrant further consideration.

Conclusions section: Please see my first comment. I think the idea of latent cells is quite interesting and this could be a useful contribution to the literature. But I don’t think it’s fair to say that the “conventional” view of the atmospheric circulation (the Eulerian mean and Lagrangian mean features) are embedded in these latent cells. They are moving one thousand times the mass on the same hemispheric scales.

I appreciate this perspective. The revised manuscript emphasizes that the latent-cell framework is intended to complement, rather than replace, conventional descriptions of atmospheric circulation. The distinction being drawn is between net transport and the larger recirculating transports that characterize the traditional overturning circulation.