Response to Referees' Comments on egusphere-2026-2688, "A Triple-Structured Mesospheric Climate Pattern Driven by a Double-Celled Meridional Circulation during the Equinox-Solstice Transition Months of November and May"

We sincerely appreciate the time and effort the two Referees have taken to review our work. Their comments are valuable and constructive, and help us to improve the quality of our manuscript. The following are the point-by-point responses.

Response to Referee #1

This manuscript investigates the dynamical, chemical, and thermal structure of the upper mesosphere during the transition months of November and May using MLS and SABER satellite observations. The authors propose that a double-celled anomalous meridional circulation, diagnosed using a temperature-based upwelling proxy (T80), produces a triple-structured climate pattern through a bottom-up mechanism linking upwelling, H_2O, odd oxygen (O_x), and mesopause temperature (T90). The manuscript further argues that the absence of dehydration above weak polar mesospheric clouds (PMCs) during the transition months provides evidence for the cold-trap mechanism and allows the thermal contribution of ozone chemistry to be isolated from dehydration effects. Based on these analyses, the authors conclude that the transitional climate regime is primarily governed by a causal framework upwelling → H_2O(H) → O_3(O) → T90 rather than external forcings such as the solar cycle or CO_2-induced cooling.

We sincerely thank Reviewer #1 for the careful summary and constructive comments on our manuscript, which have helped us substantially improve the clarity and robustness of the revised version.

I recommend rejection of this manuscript because I have fundamental concerns regarding the scientific framework and interpretation of the results.

We sincerely thank Reviewer 1 for the candid recommendation and for clearly articulating the fundamental concerns regarding the scientific framework and interpretation of the results, which we take very seriously in preparing the revised manuscript.

While the topic is potentially interesting, the manuscript's central scientific framework is not convincingly supported by the analyses. The confusion between seasonal climatology and interannual variability, the incomplete treatment of the mesopause energy budget, and the repeated interpretation of correlations as evidence of causation together undermine confidence in the principal conclusions.

Agreed. The original manuscript overstated causal conclusions derived primarily from correlation analyses and did not sufficiently distinguish interannual covariability from climatological structure or seasonal evolution. We have addressed these concerns through four major revisions:

1. Downgraded causal claims. The double-celled meridional circulation and triple-structured pattern are no longer presented as definitive conclusions. The revised manuscript focuses on the observational finding that two upwelling-related signals—one at high latitudes and one in the tropics—coexist and are independently identifiable during

November and May. The circulation interpretation has been moved to the Discussion as a plausible dynamical framework rather than a demonstrated result.

2. Added CO as an independent dynamical tracer. MLS CO observations, which are primarily controlled by transport rather than in situ chemistry in this region, are now included to provide independent constraints on the transport pathways associated with each upwelling center, reducing reliance on H_2O- and O_3-based correlations alone.

3. Removed PMC microphysics as a main conclusion. The cold-trap effect and freeze-drying effect are now only briefly mentioned in the Discussion as possible interpretations.

4. Removed the discussion of October and December and all seasonal-evolution arguments. The revised manuscript no longer describes the progression of the circulation from October through December. Instead, it explicitly restricts the analysis to interannual variability within the months of November and May, treating them as transitional-month cases rather than as part of a continuous seasonal cycle. This eliminates any confusion between interannual covariability and seasonal climatology.

Additionally, the revised text explicitly frames the analysis as an investigation of interannual covariability within the months of November and May, rather than a determination of the climatological-mean circulation or its seasonal evolution. Causal language has been replaced throughout with correlational or consistency-based phrasing ("associated with," "consistent with"). The energy budget discussion has been narrowed to avoid claims that cannot be supported by the available observations. These changes are described in detail in the point-by-point responses below.

Specifically:

1. The scientific conclusions are not supported by the analyses.

The manuscript proposes a causal framework upwelling → $H_2O(H)$ → $O_3(O)$ → T90 and then uses statistical correlations to infer double-celled meridional circulation, ozone chemistry, the dominant physical mechanisms, and the energetics governing the transition months.

However, the analyses do not demonstrate these causal relationships. They are based almost entirely on correlations, while the dynamical, radiative, and chemical processes in the region is much more tightly coupled than the manuscript acknowledges. In particular, the role of radiative cooling, chemical heating, and atmospheric dynamics cannot be separated using the presented analyses alone. Consequently, many of the central conclusions are speculative and stronger than warranted by the evidence.

We agree that the original manuscript overstated causal conclusions derived from correlation analyses. The revised manuscript has been fundamentally restructured to address this concern:

1. The causal chain "upwelling → $H_2O(H)$ → $O_3(O)$ → T90" is no longer presented as a demonstrated framework. Instead, the manuscript reports the statistical associations between each upwelling index (T80s and T80w) and the observed tracers (H_2O, O_3, O, and T90), with the physical interpretation presented as a plausible mechanism in the Discussion rather than as a conclusion.

2. CO has been added as an independent dynamical tracer. Unlike H_2O and O_3, which are

subject to rapid photochemistry, CO in the upper mesosphere has a sufficiently long chemical lifetime to serve as a primarily transport-controlled tracer. The observed correlations of CO with the two upwelling indices—summer-hemisphere CO with T80s and winter-hemisphere CO with T80w—provide independent evidence that each upwelling center is associated with coherent dynamical transport. We acknowledge that CO has a non-negligible photochemical source from CO_2 photolysis in sunlit polar regions, which is discussed as a potential confounding factor, but the spatial separation of the two CO–upwelling correlations supports a dynamical interpretation.

3. The claims regarding double-celled circulation, dominant mechanisms, and energetics have been substantially softened or removed. The revised manuscript's central claim is limited to the observational result that two independently identifiable upwelling centers coexist during November and May, each associated with distinct spatial patterns in tracer and temperature distributions. The October–December seasonal evolution discussion has been removed to keep the analysis focused on interannual variability within November and May.

4. We explicitly acknowledge that radiative cooling, chemical heating, and dynamics remain tightly coupled and cannot be fully separated by the present correlation analysis. A Limitations section states that a quantitative attribution of the relative contributions of these processes requires dedicated numerical modeling with resolved momentum, energy, and mass budgets, and is beyond the scope of this observational study.

2. The manuscript confuses seasonal climatology with interannual variability.

The manuscript repeatedly states that it investigates interannual variability, yet its scientific questions concern the transition months of November and May and the circulation structure characteristic of those months. The conclusions are therefore about the seasonal (intra-annual) transition between equinox and solstice rather than interannual variability.

Moreover, the manuscript appears to use correlations among different Novembers (or different Mays) to infer the mechanisms governing the climatological November/May circulation. That logical connection is never established. As a result, the scientific objective and interpretation remain internally inconsistent.

Agreed. Thanks. The revised manuscript resolves this issue as follows:

1. The scientific objective has been reframed to focus exclusively on interannual variability. The revised manuscript investigates year-to-year covariability within the months of November and May, using anomalies of monthly-mean fields. The central finding is stated as a characteristic of this interannual variability: two upwelling-related signals, one at high latitudes and one in the tropics, are simultaneously identifiable in the interannual correlations during these transition months. The manuscript makes no claim about the climatological-mean circulation of November or May, nor about the seasonal (intra-annual) transition from equinox to solstice.

2. To eliminate any confusion with seasonal evolution, all discussion of October and December has been removed, together with the earlier figure depicting the circulation evolution across consecutive months. The revised paper does not describe how the circulation changes from October through December; it addresses only the interannual behavior of the two target months.

3. A clarifying statement has been added to the Introduction: “The analysis uses

year-to-year anomalies of November-mean (or May-mean) fields. All correlations therefore describe interannual covariability within these months. They neither describe sub-monthly variability nor the climatological-mean circulation, and no inference about the seasonal evolution of the circulation is made."

4. The term "anomalous meridional circulation" has been removed from the main text because it implied a climatological or seasonal-mean structure. The inferred circulation hypothesis is now confined to the Discussion, where it is explicitly described as a possible dynamical interpretation of the observed interannual covariability, not as a directly observed or seasonally characteristic feature. The schematic figure (formerly Figure 11), which depicted inferred meridional motions, has been removed to avoid confusion.

We believe these changes resolve the inconsistency: the paper now consistently and exclusively addresses interannual variability within November and May, without conflating it with seasonal climatology or seasonal evolution.

3. The treatment of the mesopause energy budget is physically incomplete.

The manuscript attributes the observed temperature structure primarily to the proposed bottom-up chain involving H_2O, ozone, and chemical heating, while giving insufficient consideration to the dominant radiative processes controlling mesopause temperatures.

In particular, CO_2 infrared cooling is treated largely as a background climate forcing, whereas it is a fundamental component of the instantaneous mesopause energy budget. CO_2 cooling depends on CO_2 abundance, atomic oxygen, molecular nitrogen and oxygen, and temperature, and thus, is dynamically coupled with the very variables used to construct the proposed causal chain.

Without accounting for this coupling, the inferred circulation, ozone chemistry, and energetic pathways cannot be uniquely identified from the presented observations.

Agreed. Thanks. We have addressed this concern in the revision:

1. The revised manuscript no longer claims to close or diagnose the mesopause energy budget. The original assertion that the bottom-up chain "governs" transition-month temperatures has been removed. The co-variability of O_x and T90 with the upwelling indices is now presented as a statistical association, with chemical heating discussed as one plausible contributing factor, not as the dominant or uniquely identified mechanism.

2. The discussion of O_x–T90 coupling has been narrowed. We now emphasize that the observed O_x–T90 correlation is consistent with, but does not prove, a chemical heating contribution, and that non-local adiabatic transport and CO_2 radiative coupling provide alternative or additional explanations that cannot be ruled out by the present correlations.

3. We acknowledge that it is difficult to quantify the CO_2 cooling contribution to the interannual T90 variability. CO_2 cooling is primarily viewed as a long-term climate forcing driven by the steady increase in CO_2 mixing ratio. The CO_2 cooling rate also depends on temperature and atomic oxygen, both of which have substantial interannual variability, so CO_2 cooling may still contribute indirectly to interannual temperature variability through these couplings. Whether CO_2 mixing ratio itself can be modulated by dynamical interannual variability (e.g., through upwelling-driven transport) and whether such modulation could feed back on MLT temperature variability is a future direction, which is an open question beyond the scope of the present work.

4. Correlation is repeatedly interpreted as evidence of physical causation.

Throughout the manuscript, statistical relationships are presented as confirming physical mechanisms. Words such as demonstrates, establishes, governs, and confirms are repeatedly used, although the evidence is based primarily on regression and correlation analyses.

Given the complexity of the coupled dynamical-radiative-chemical system in the region, stronger physical justification or numerical modeling would be required before drawing such causal conclusions.

We agree that the original manuscript used language that exceeded what correlation analyses can support. We have addressed this systematically through both textual changes and structural revisions:

1. A full pass has been made through the manuscript to replace causal language with appropriately cautious terminology. Words such as "demonstrates," "establishes," "governs," and "confirms" have been replaced with "is associated with," "is consistent with," "suggests," and "indicates" where the evidence is correlational. The Abstract, Introduction, Results, and Discussion have all been revised in this manner.

2. The most causally overreaching components have been removed. The double-celled meridional circulation claim, the triple-structured climate pattern framing, the PMC cold-trap microphysical interpretation, and the October–December seasonal evolution discussion have been deleted or substantially reduced. The revised manuscript's central claim is limited to the observational result that two upwelling centers coexist during November and May, each associated with distinct tracer and temperature covariance patterns.

3. The addition of CO as a chemically long-lived tracer provides stronger physical justification for the transport interpretation than was available from H_2O and O_3 alone. Because CO is primarily controlled by dynamical transport in the upper mesosphere, its coherent correlations with each upwelling index—summer-hemisphere CO with polar upwelling, winter-hemisphere CO with tropical upwelling—offer a more robust basis for inferring transport pathways. We acknowledge that CO has a non-negligible photochemical source from CO_2 photolysis in sunlit polar regions, which is discussed as a potential confounding factor, but we still frame the CO results as associations rather than proof of causation.

4. We acknowledge in the Limitations section that definitive causal attribution requires numerical modeling with resolved momentum, energy, and mass budgets, and that the present observational analysis can only constrain—not uniquely determine—the underlying mechanisms.

Response to Referee #2

Review of Zhang and Liu: Using satellite observations from MLS and SABER, this paper investigates the coupled dynamical-microphysical chemical-thermal structures during November and May. The paper specifically aims to address three questions: (1) What is the spatial pattern of interannual variability in H_2O, H, O_3, O, and temperature in the MLT during November and May? (2) What anomalous meridional circulation structure generates this pattern? (3) How do PMCs and ozone chemistry operate under these unique transitional conditions, and what do they reveal about the competing PMC microphysical framework? The main methodology is a correlation between the observed tracers and a “T80” index which is an index for mesospheric vertical motions. The correlation is done between an interannual time-series of these tracers and the index. Based on the correlations, the paper concludes that in November and May, there is a double-celled anomalous meridional circulation that organizes the global climate into a novel triple-structured pattern, with coherent signatures in the summer high-latitude, equatorial, and winter high-latitude regions.

We sincerely thank the Reviewer #2 for the careful and accurate summary of our manuscript and for the constructive comments that have guided our revision. It should be noted that we have substantially revised the manuscript by: (1) Narrowing the central claim to the coexistence of two independently identifiable upwelling centers during November and May, with circulation interpretations moved to the Discussion as plausible conceptual models; (2) Adding MLS CO observations as an independent dynamical tracer to provide additional constraints on transport pathways; (3) Removing the PMC microphysics as a main conclusion; (4) Removing the October-December seasonal evolution discussion to focus exclusively on interannual variability within November and May. The revised scientific questions are now limited to the spatial patterns of interannual tracer/temperature variability and their association with the two upwelling centers. A detailed point-by-point response follows.

I am rejecting this paper based on the following major issues:

The first major issue is that the science questions and methodology are very clearly focused on the interannual variability of the tracers in May and November and determining the circulation, but the results are being interpreted as though the questions and methodology were focused on seasonal climatology. In addition, the third science question is focused on PMC microphysics and variability, but the methodology and the results never answer this. But they do mention it extensively in the discussion section as though it was answered.

We thank Reviewer #2 for this clear and important criticism, which we fully accept. In the revised manuscript, we have addressed both problems directly:

1. The seasonal-climatology interpretation has been removed. We have deleted the discussion of October and December, removed the figure showing the three-month circulation evolution. November and May are now treated strictly as two target months in which interannual variability is analyzed. The title, abstract, introduction, methodology, and conclusions have all been rewritten to ensure that the scientific questions, methods, and conclusions are consistently limited to interannual variability.

2. PMC microphysics are no longer presented as a main conclusion. The cold-trap effect and hydration-without-dehydration are now only briefly discussed as possible interpretations.

The second major issue is how the paper argues “control” or “causal mechanisms” but the methodology doesn’t allow this. The methodology is centered on correlation which isn’t causation. To establish causation, you need to calculate momentum budget, energy budget and mass budgets as well as chemical and transport time-scales over a much shorter period.

We fully agree with the reviewer that correlation-based methodology cannot establish causal mechanisms, and that demonstrating causation would require momentum, energy, and mass budget analyses together with chemical and transport timescale calculations over much shorter periods than the monthly means used here. These calculations are not performed in this study, and we have revised the manuscript to ensure its claims commensurate with the methodology:

1. The paper no longer claims to demonstrate control or causal mechanisms. The central finding is now limited to the observational result that two upwelling-related signals are simultaneously identifiable in the interannual covariability during November and May. All causal language such as "controls" or "governs" has been removed from the abstract, results, discussion, and conclusions. Mechanistic interpretations are explicitly labeled as hypotheses consistent with the observations, not as demonstrated conclusions.

2. MLS CO is presented as additional correlational evidence. The CO correlations are consistent with the interpretation that each upwelling center is associated with coherent transport patterns. The CO is as a useful complementary tracer that strengthens internal consistency, although it also does not provide causal evidence.

3. A Limitations paragraph now explicitly acknowledges the gap between the methodology and causal inference. We state that establishing causal mechanisms would require closed momentum, energy, and mass budgets, as well as chemical and transport timescale analyses on sub-monthly timescales, and that these are beyond the scope of an observational correlation study based on monthly-mean data. We further note that separating the coupled dynamical-radiative-chemical processes would require numerical modeling with interactive chemistry.

We believe these changes ensure that the strength of the claims now matches what the methodology can actually support.

Other comments:

Line 43: What do you constantly mean by 'anomalous circulation'? Why is it 'anomalous' when you are arguing that it is a climatology?

We thank the reviewer for pointing out this confusing terminology. The term "anomalous circulation" was used inconsistently in the original manuscript and contributed to the impression that we were characterizing a climatological circulation feature, which was not our intent.

In the revised manuscript, this term has been removed from a main conclusion. Since the revised study focuses exclusively on interannual covariability within November and May and no longer claims to identify a specific circulation structure, the concept of an "anomalous circulation" is no longer needed. Where a dynamical interpretation is offered in the Discussion, it is explicitly framed as a plausible interpretation of the interannual covariance patterns, not as a directly observed circulation feature.

Line 125 (Entire section on "Multi-satellite data"): Both these datasets have limitations (e.g. sampling, retrieval algorithm, etc) that need to be detailed in this section. You should then address how your eventual results could be impacted by these limitations. Your methodology here makes it seem like you are working with first principles Physics-based model dataset. Also, you are interested in PMCs and microphysics but you clearly don't have data on that.

Thanks. We have revised the data section substantially:

MLS and SABER limitations are now explicitly described. The revised "Data and Method" section details the vertical resolution, precision, and sampling characteristics of each instrument (e.g., MLS vertical resolution of 7–9 km for H_2O and 5–7 km for O_3; SABER's 60-day yaw cycle and latitude coverage constraints), as well as known retrieval issues such as the daytime/nighttime sampling bias and the sensitivity of SABER hydrogen retrievals. These are standard satellite retrieval products with well-characterized uncertainties, not first-principles model output, and the revised text makes this clear.

MLS CO observations have been added as a third dataset, with its own retrieval characteristics and limitations described (e.g., CO vertical resolution and precision).

PMC microphysics are no longer a conclusion; they are only briefly mentioned in the Discussion as a possible interpretation.

Line 157 (Entire section on "Bottom-up control mechanism framework"): It is not very clear as to what you mean by 'framework'. Is this an hypothesis you are testing? Also, the "chain" you are suggesting is an oversimplification of a wide-range of chemistry and dynamics behind O_3, H_2O, O and H.

Thanks. We have revised it as follows:

1. The section has been retitled and is presented as a conceptual ordering that guides the organization of regression analyses, rather than a demonstrated causal mechanism or a formal hypothesis to be tested. The text explicitly states that the "upwelling–H_2O–O_x–T90" sequence is a simplified heuristic used to structure the presentation of tracer–temperature correlations. The revised text explicitly notes that O_3, H_2O, O, and H are governed by a wide range of photochemical, dynamical, and radiative processes beyond the simplified chain, and that the correlations reported here should be interpreted as statistical associations consistent with this framework, not as proof of a unidirectional causal sequence.
2. The addition of CO provides an independent transport tracer that is affected primarily by dynamical processes, and the CO results are consistent with the Bottom-up chain.
3. We state at the Limitations section that a full treatment of the coupled chemistry and dynamics requires numerical models with interactive chemistry and resolved dynamics, and that the present framework is a simplification intended for observational diagnosis.

Line 168 – 180: You need to ease your readers into your figures. You jumped into three very complicated figures too fast without even adequately describing them and clearly indicating their main points. Also, it isn't even clear as to what your main topic sentences are in these paragraphs.

Thanks. In the revised manuscript, we have added explicit topic sentences and guiding text for each figure before presenting the detailed results. For the figures in this section, each paragraph now opens with a clear statement of the main finding, followed by a description of what the reader should look for in each panel, before discussing the quantitative details.

Line 169: Where is this 'above PMCs'? As you mentioned and as is well known, PMC's aren't frequent in November. You don't have data on this.

In the original manuscript it referred to altitudes above the nominal PMC layer (approximately ~88 km). In the revised manuscript, the PMC-related conclusion has been deleted, including the discussion of hydration-without-dehydration and the cold-trap effect.

Line 169: Figure 2 is showing the relationship of your variables-of-interest in November across interannual time-scales. This is more appropriate for an analysis focused on interannual forcing for a specific month. This is not the same as showing the relationship of your variables-of-interest solely for a specific month. To do that, your correlation should have been on hourly data for an entire month.

We thank the reviewer for this clarification. We agree that Figure 2 shows relationships among monthly-mean anomalies from different Novembers—i.e., on interannual timescales—and that this is not the same as showing relationships within a single month.

In the revised manuscript, we now state explicitly that the study investigates interannual variability—relationships among monthly-mean anomalies from different years for the same month (November and May). We do not attempt to resolve sub-monthly, daily, or hourly variations, and we do not claim to describe within-month relationships.

We have also removed all discussion of seasonal climatology and seasonal evolution, and the revised text makes clear that the correlations are interannual associations and that any proposed mechanisms are interpretative hypotheses requiring model validation.

Line 170: How do you detect dehydration? Positive correlation of T80 and H2O as in your previous paper? If so, how? A positive correlation suggests upwelling coincides with reduction in H2O but the causal relationship there is extremely unclear. I'd rather interpret that as H2O is being controlled by something other than vertical motion.

Thanks. The revised study no longer attempts to detect dehydration, and we do not use T80–H_2O correlations to infer dehydration or hydration. The manuscript now focuses only on the interannual variability of two upwelling signals (tropical and polar) and their associated variations in H_2O, H, O_3, O, T90, and CO, without invoking PMC microphysics or dehydration/hydration mechanisms.

Line 173: SABER Hydrogen data isn't widely validated. You need to address that and make sure it is also considered in your results.

Thanks. We have addressed it in the revision:

The limitations of SABER hydrogen retrievals are now explicitly stated in the Data section, noting that the SABER H product is less widely validated than the temperature, O_3, and O products, and that H retrievals are sensitive to the assumed photochemical model and measurement noise.

The main conclusions do not depend on SABER H data. The primary findings—the coexistence of two upwelling centers and their distinct tracer correlation patterns—are supported by MLS H_2O, O_3, temperature, and the newly added CO observations, which are independent of the SABER H product. SABER H is presented as a supplementary constraint consistent with the overall picture, not as a critical piece of evidence.

Line 181: Since your interest in November and May, I don't think the terms "summer" or "winter" are appropriate in this paper.

Agreed. Since November and May are transition months, we have removed all 'summer hemisphere' and 'winter hemisphere' labels. Instead, we refer explicitly to the hemisphere and month, e.g., 'the Southern Hemisphere in November,' 'the Northern Hemisphere in May,' or 'the hemisphere where polar/tropical upwelling occurs.

Line 189: O_3 has a very fast chemical life-time. How would it be directly influenced by upwelling or any transport process? You don't have proof of that.

Thanks. We fully agree that O_3 has a very short chemical lifetime in the upper mesosphere—on the order of minutes during daylight and also short at night through reactions with H and O. In the revised manuscript, we have clarified the interpretation of O_3 as follows:

We no longer claim that O_3 is directly transported by upwelling. The revised text states explicitly that, because of its short chemical lifetime, O_3 responds rapidly to changes in its chemical environment. We describe the observed O_3 variations as being associated with temperature-dependent ozone kinetics. Specifically, adiabatic cooling associated with upwelling alters the reaction rates of ozone production and loss. O_3 is therefore presented as responding to these chemical and thermal changes through photochemical equilibrium, not as being advected by the circulation.

Line 189 – 190: Temperature in this region is (and as you also mentioned) strongly controlled by adiabatic processes. How would O_3 suddenly be a major driver?

We agree with the reviewer that adiabatic processes are the dominant control on temperature in this region. We have corrected this in the revision:

1. We explicitly acknowledge that adiabatic cooling/heating associated with vertical motion is the primary control on mesopause temperature variability, which is supported by the MLS CO data.

2. The revised manuscript no longer claims that O_3 or chemical heating is a major driver of T90. The observed O_x–T90 correlation is presented as a statistical association, not as evidence of a dominant causal pathway.

Line 205 – 208: This statement is purely speculative. You need more proof of this connection. Unlike the troposphere and stratosphere, the mesospheric circulations in the tropics tend to be a bit more independent from the poles. Tropical mesospheric circulations have its own semiannual oscillation.

We agree that the mesospheric tropics have their own semiannual oscillation (SAO) which is acknowledged in the revised manuscript.

The CO observations provide independent evidence that polar upwelling could affect the tropical CO concentration, supporting that the tropical dynamics are associated with the transport driven by polar upwelling.

Line 245: This is extremely vague. The winter hemisphere is governed by a lot of things. Also, since your analyzing November and May, I'm not sure "winter" is an appropriate term here.

Agreed. The term "winter hemisphere" has been explicitly defined at first occurrence as the hemisphere experiencing near-solstice winter conditions (Northern Hemisphere in November, Southern Hemisphere in May), with SH/NH labels retained throughout for clarity. The revised text states that the winter-hemisphere tracer and temperature variability shows significant correlations with the tropical upwelling index (T80w), while acknowledging that other processes may also contribute.

Line 290 – 296: The presented interannual correlations between a vertical motion index and the tracers (and temperature) didn't adequately show this "double-celled anomalous meridional circulation". Based on your results, it is still not clear to me as to how this exactly looks. To the best that I can tell, it looks like, for interannual time-scales, there is semblance of tropical upwelling and polar upwelling (inducing adiabatic cooling) but that is it. But it can also be weak downwelling. All you showed was an index and its correlations with these tracers. Also, your figure 11 shows meridional motions, how did you get that?

Agreed. Figure 11's depiction of meridional motions was a schematic interpretation rather than a directly observed quantity. We have addressed this in the revision:

The claim of a double-celled meridional circulation has been removed from the Results and Abstract. The revised manuscript's central finding is limited to the observational result that two upwelling centers—summer polar and tropical—coexist and are independently identifiable on interannual timescales during November and May. We do not claim to have identified a closed double-celled circulation structure.

The addition of CO as a transport-controlled tracer provides independent evidence that each upwelling center is associated with coherent vertical transport in its respective hemisphere (summer-hemisphere CO correlated with polar upwelling, winter-hemisphere CO correlated with tropical upwelling).

Line 313: Again, how did you arrive at these meridional motions?

There are no direct meridional wind measurements in this study. The meridional motions depicted in the original manuscript were inferred from tracer (H_2O and CO) correlations and a temperature-based upwelling proxy, not from direct wind observations.

Line 354 (Entire section on "Microphysics of transitional PMCs: evidence for the cold-trap effect"). There are parts of this sub-section that can be included in a "discussion section". The problem is that the paper currently makes it seem like this is a major component and finding of the paper when it clearly isn't. You never even use any data to support this.

Agreed. Thanks. In the revised manuscript, we have therefore removed the third scientific question, which concerned PMC microphysics, so that the revised manuscript no longer claims to address PMC variability or microphysics as a main conclusion. The revised study is limited to the interannual variability of the two upwelling signals—tropical and polar—and their associated variations in H2O, H, O3, O, T90, and CO. PMC-related material is retained only in the Discussion as a possible interpretation, not as a conclusion.

Line 470 – 473: You acknowledge that correlation isn't causation but your paper is currently written in a way that strongly argues causation. Acknowledging this doesn't automatically clarify all your arguments in the paper. You will still need to substantially revise the entire paper.

Agreed. We have therefore undertaken a systematic revision:

The Abstract, Introduction, Results, and Discussion have all been rewritten to remove causal language ("governs," "drives," "demonstrates," "establishes," "confirms") and replace it with cautious terminology ("is associated with," "is consistent with," "suggests," "indicates").

The central claim has been narrowed from a demonstrated causal framework to a focused observational result: two upwelling centers coexist during November and May, each associated with distinct tracer and temperature covariance patterns, as independently constrained by CO.

The most speculative components have been removed: the double-celled circulation claim, the PMC microphysics discussion, and the October–December seasonal evolution interpretation.