Response to Referees' Comments on egusphere-2026-4809, "Quantifying Mesosphere and Lower Thermospheric Density Variability in December and June"

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

The manuscripts uses SABER observations to show that summertime variability of the polar temperature at 80 km is well correlated with variability over a range of altitudes and latitudes.

We sincerely thank Reviewer #1 for the summary on our manuscript.

Major Comments

Section 3.2 describes a trend but provides no evidence that this is statistically significant. By eye, it is evident that the year-to-year variability is so large that it is difficult to identify a trend. Later, in Section 4.3 and Figure 12, the authors indicate that no statistically significant trend is seen. For these reasons, descriptions of the trend and speculation of its cause in Section 3.2 should be deleted. It is appropriate to show the analysis in Section 4.3 since the text is clear about the lack of significant response.

We thank the reviewer for this constructive criticism. We agree that the trend description in the original Sect. 3.2 was presented without statistical evidence and that the physical interpretation was premature. We have therefore removed all trend estimates and trend-based causal interpretation from Sect. 3.2. The trend analysis is only discussed in Sect. 4.3. Sect. 3.2 now focuses strictly on interannual covariability and its physical interpretation in terms of year-to-year T80 anomalies.

Lines 183-188: in Fig. 2, the texts of T80 and density trends have been removed.

The text is written as if T80 "causes" the interactions. At various points, the text implies that T80 is driving a polar thermospheric response and a low latitude response rather than the reverse (or that both time series are responding to events at some other location). The paper attributes the variations to polar upwelling but does not address the question of what causes the upwelling to be variable. A full investigation of driving mechanisms is outside the focus of the paper. Nevertheless, the authors should be careful to point out that the physical process driving the T80 variability could also directly affect the variability at other locations.

We thank the reviewer for this important clarification. We fully agree that the T80 index is a diagnostic proxy for the strength of the summer polar upwelling rather than the physical process itself, and that causality should not be attributed to T80.

First, we have revised the language throughout. Most of causal verbs previously attached to T80 ("dominates", "controls", "influences", "drives", "response to T80", "T80-driven") have been replaced with statistical or indicative language such as "covaries with", "is associated with", "captures", and "is tracked by".

Second, we thank the reviewer and agree with the inspiring comment that the physical

processes driving T80 variability could also directly affect density at other locations. The T80–density correlation is therefore presented as diagnostic covariability, not as evidence of unidirectional polar-to-global forcing.

Lines 375-379: in Sect. 4.4, we point out that the physical process (e.g., interannual perturbations in gravity wave drag and planetary wave breaking) driving the T80 variability could also directly affect the density variability at other locations.

Minor Comments

1. Please give a precise definition of T80; "near 80 km over the summer polar region" is not sufficient.

The T80 index in December during 2004–2025 is derived from MLS data by averaging the zonal and monthly temperature between 75°S–82°S at 0.0215 hPa (corresponding to ~80 km), which is then extended back to 2002–2003 by the SABER data. The T80 index in June is similar, applying temperature between 75°N–82°N at 0.0215 hPa. In the revised manuscript, the T80 applies the temperature anomaly.

Lines 112-121: A detailed definition of T80 is provided.

Lines 123-127: Table 1 lists the values of T80 in December and June.

2. (l. 47-48) This sentence doesn't belong in the list of evidence for external forcing of MLT variability since the events it describes result from internal dynamical coupling.

Agreed and thanks. Stratospheric sudden warmings are not an external forcing but result from internal dynamical coupling.

Lines 49-50: The sentence about SSW has been moved to the paragraph of internal dynamical coupling.

3. The description of the physical processes contributing to the higher temperature above the summer mesopause (l. 64-67) omits the contribution of the downwelling circulation (see e.g. N Wang 2022; JC Wang 2023). Acknowledgement of the downwelling is also appropriate at l. 121, 162, 252. In Section 4.1, the statement that "Stronger upwelling elevates local temperature …" is misleading. Although the correlation has been shown, it is not appropriate to imply that upwelling raises the temperature.

We thank the reviewer for this important correction and agree that the original description was incomplete. The downwelling is treated as the primary driver of the warming above the mesopause, with O_x chemical/radiative heating playing a secondary role. We have cited Wang et al. (2023) and Wang et al. (2022) to introduce the downwelling circulation.

Lines 52-55: The downwelling at lower thermosphere has been described in Introduction.

Lines 63-65: The influence of downwelling on temperature has been introduced.

Lines 269-275, 286-289: The downwelling is proposed as the main mechanism in Sect. 4.1.

4. There is no figure that shows how the magnitudes of the day-to-day changes compare with that of the interannual variability of monthly means. This value can affect the reader's confidence in the predictive power of the previous monthly mean. A rough idea of this can be gained from the changes in the range of values in Figures 7 and 8. Can this be illustrated or summarized?

We appreciate the reviewer for raising this point. The daily standard deviation of T80 index

σ(T80) and density σ(ρ) in each southern summer are illustrated in Figs. 6 and 7.

Within each summer, the day-to-day standard deviation is large for both hemispheres: σ(T80) = 6.1 K in the SH (November-January) and 6.3 K in the NH (May-July). The interannual standard deviation of the monthly means is smaller and strongly hemispheric: 5.1 K in December versus only 1.6 K in June (Table 1). The T80-density linkage is strong on both daily and interannual timescales in the SH, whereas in the NH it is strong on daily timescales but weak on interannual timescales, consistent with the smaller June interannual variance.

Regarding the reliability of the monthly mean, the key metric is the standard error of the mean (SEM), i.e., the error bar of the monthly averaged T80. With about 30 days per month, the SEM is given by daily σ(T80)/sqrt(30), yielding an SEM of ~1.1 K for December. This error bar is much smaller than the December interannual variability (σ = 5.1 K), confirming that the monthly mean T80 reliably captures the year-to-year signal. For June, the SEM is ~1.15 K, which is also smaller than the June interannual variability (σ = 1.6 K). Although the signal-to-noise ratio is lower in June than in December, the monthly mean remains a robust indicator of interannual upwelling variations in both SH and NH summer seasons. Therefore, the T80-density linkage reported here is not an artifact of day-to-day noise; the monthly mean T80 has a sufficiently small error bar to resolve the interannual upwelling signal.

Lines 209, 229, 231-239: The values of daily σ(T80) are given in Sect. 3.3 and Figs. 6-7.

Editorial Comments

l. 259: "subjacent" is this a word?

Thanks. The word "subjacent" has been deleted.

References

Wang, N., Qian, L., Yue, J., Wang, W., Mlynczak, M. G., & Russell, J. M. III. (2022). Climatology of mesosphere and lower thermosphere residual circulations and mesopause height derived from SABER observations. Journal of Geophysical Research: Atmospheres, 127, e2021JD035666. https://doi.org/10.1029/2021JD035666

Wang, J. C., Yue, J., Wang, W., Qian, L., Jones, M., Jr., & Wang, N. (2023). The lower thermospheric winter-to-summer meridional circulation: 2. Impact on atomic oxygen. Journal of Geophysical Research: Space Physics, 128, e2023JA031684. https://doi.org/10.1029/2023JA031684

We thank the reviewer for suggesting above articles, which are really helpful for the manuscript.

Lines 55, 65, 270-271: The two references have been cited to strengthen the background of mesospheric meridional circulation.

Response to Referee #2

This study uses 24 years of SABER observations (2002–2025) to analyze the correlation between T80, a temperature-based proxy constructed from fused MLS and SABER temperature measurements, and the density variations in the mesosphere lower thermosphere (MLT) region. The authors show a robust correlation regarding interannual variability during November, December and January in some regions, especially the southern hemispheric mesopause region. They then try to extent this approach to

day-to-day variability to test the applicability of the T80 proxy as a tool for density prediction in the MLT. Lastly, the authors also attempted to attribute density variations to natural drives, such as solar radio flux or CO_2.

We thank the reviewer for the accurate summary. We confirm that the study uses 24 years (2002–2025) of SABER density observations together with the fused MLS/SABER T80 proxy, with the strongest interannual coupling over the southern high-latitude mesopause. All predictive language has been removed from the revised manuscript. For clarity, the day-to-day analysis is intended not as density prediction but as a robustness test of whether the T80-based linkage holds across timescales. The solar and CO_2 terms are evaluated by the multiple linear regression in Sect. 4.3, which shows that they cannot be reliably separated from the dominant internal upwelling variability at the summer high latitudes.

I have several concerns with the study in its present form. The methodology is unclear and the results are largely overinterpreted. Specifically, the manuscript does not clearly establish the novelty of the relationship between the T80 index and the latitude dependent density profile compared with the results of Zhang et al, 2025c. The presented results appear largely derivative of the earlier study, with established temperature relationships extended to density through the ideal gas law and hydrostatic balance. Furthermore, the results from investigating the lagged correlation between T80 and density do not support the claim that T80 provides predictive capabilities. I also question the novelty of the proposed physical mechanisms “local thermal effect” and “column adjustment effect” as they appear to follow mainly from the assumption of an ideal gas and hydrostatic equilibrium.
Therefore, my recommendation is “reject”.

We thank the reviewer for the frank assessment and for identifying aspects of the original manuscript that were not presented with sufficient clarity. While we respectfully disagree that the work is derivative, we agree that the original manuscript did not make its methodological basis and its specific novelty sufficiently explicit, and we have made substantial revisions as summarized below.

1. Novelty relative to Zhang et al (2025c) and the "derivative" concern. We agree that the two physical components are not new atmospheric physics, and we now state this openly at the start of Sect. 4.1: they follow from the ideal-gas relation and hydrostatic balance. The novelty is not a new mechanism but the first observationally constrained, quantitative decomposition of the density response on fixed geometric heights. On a fixed pressure surface, density is indeed constrained to vary as $\rho \propto 1/T$, which is the part that can be derived from the earlier temperature result. On a fixed geometric height, however, the pressure surface itself moves, and the column-adjustment term can reverse the sign of the response. Neither the sign reversal in the transitional layer (85–91 km) nor the non-local response at 95 km follows from the ideal-gas law applied to local temperature; both require the observed response of the full column. The density and temperature transitions are vertically offset by about 11 km (near 81 km versus 92 km), a purely observational feature that cannot be obtained a priori. Zhang et al (2025c) reported temperature only, with no density, no daily-scale analysis, no multiple regression, and no variance decomposition.

2. Methodological clarity and record length. The revised manuscript now states the methods explicitly, including the construction and fusion of the T80 record, the treatment of the SABER yaw cycle, the density anomaly definition, and the full multiple linear regression with

the interpretation of each coefficient. The December T80 in this study spans 2002–2025, longer than the earlier 2004–2022 time span of the December T80 in Zhang et al (2025c). This extended window covers approximately two 11-year solar cycles, which allows the internal dynamical, solar, and long-term trend contributions to be investigated through the multiple regression. The longer sample also improves the statistical reliability.

3. Predictive capability. We have withdrawn this claim. All predictive language and the word "prediction" in the title have been removed.

4. Novelty of the two mechanisms. We do not claim that the local thermal effect and the column adjustment effect are new physics. Their value here is operational: separating them allows the counterintuitive sign reversal and the strong, non-local upper-MLT response to be quantified from observations for the first time. The multiple regression further shows that the upwelling-related term alone accounts for more than 90% of the interannual variance at the summer high-latitude mesopause, while the solar and trend terms cannot be reliably separated there.

Major comments:

Methodology (Section 2):

- Section 2.1: It is mentioned that the density profile is calculated from SABER temperature and pressure profiles to cover the time period 2002–2025. Later, in Section 2.2 it is mentioned that the high–latitude coverage of SABER during boreal summer months is highly variable. Thus, a detailed description of how this issue is handled when computing the density profiles is necessary.

We thank the reviewer for pointing out that the handling of the SABER yaw sampling needed to be stated explicitly. The SABER 60-day yaw cycle alternates hemispheric coverage, and the number of days available over the summer high latitudes within a given month decreases in the later part of the record. Using all available days would therefore base the monthly means on different sampling windows in different years, which could bias the interannual comparison.

To ensure a consistent and comparable sampling window, the high-latitude density analyses poleward of 50°S (December) and 50°N (June) are computed using only the first 10 days of each month (Figs. 4, 5, 11, 12).

We have verified that this restriction does not drive the results. The first 10 days and full month means agree closely and both give the same T80–density correlation (R = 0.98) in December, consistent with Fig. 2. The relationships are therefore robust to the yaw-related sampling changes rather than being an artifact of them.

Lines 96–97, 164-166, 180-182: The handling for yaw drift is described in Sects. 2.1 and 3.2.

Lines 169-170: The first-10-day (Fig. 4a) and full-month (Fig. 2) results are compared, illustrating that the yaw drift does not affect the T80-density relationship in December.

- Section 2.2: I am missing an explicit description of how the T80 index is calculated.

We apologize for the vague expression. The T80 index in December during 2004–2025 is derived from MLS data by averaging the zonal and monthly temperature between 75°S–82°S at 0.0215 hPa, which is then extended back to 2002–2003 by the SABER data. Similarly, the June T80 is derived from MLS data during 2005–2026 and SABER data during 2002–2004.

Lines 112-121: A detailed definition of T80 is provided.

Lines 123-127: Table 1 lists the values of T80 in December and June.

Results (Section 3):

- Section 3.2: The relation between T80 and the latitude dependent temperature profile was already investigated in Zhang et al, 2025c. The work in its present forms seems largely derivative of these results, extending them to density through ideal gas + hydrostatic balance.

We thank the referee for this important point. We agree that the T80–temperature relationship during solstice months was documented in Zhang et al (2025c) . However, the present paper is not a re-derivation of that result. Its novel contribution is the quantification of the MLT neutral density response on fixed geometric heights, which cannot be inferred from the temperature field alone. Four features demonstrate this:

(1) Density is a column-integrated quantity rather than a pointwise function of local temperature. On fixed geometric heights, the density response is determined by the hydrostatic integral of the entire atmospheric column. When the column expands or contracts, pressure surfaces move, producing a column-adjustment term that is absent from the local temperature response and can reverse the sign of the density response. This is not captured by the fixed-pressure relation $\rho \propto 1/T$. Although the column response is governed by hydrostatic balance, evaluating it requires the observed, co-located response of the full column, including the displaced pressure field. Zhang et al. (2025c) reported neither the full-column temperature structure on a common sampling needed to perform this integral nor any density; the reversal height and the response magnitude are therefore fixed by the present measurements rather than derived from the governing equations.

(2) The density and temperature dipoles transition at different heights. The density dipole in December reverses near 81 km (Figs. 4, 9a), whereas the temperature dipole reverses near 92 km (Figs. 9b, 10). This 11 km offset reflects the column-integrated nature of density and cannot be inferred from the temperature structure alone.

(3) The 85–91 km transitional layer exhibits an anomalous positive temperature-density correlation (Fig. 9c). In this layer, local cooling is accompanied by an increase in density, because the column-adjustment effect dominates over the local thermal effect. This sign reversal would not occur if density were a simple restatement of temperature.

(4) The two approaches also differ fundamentally in their data requirements and measurement reliability. The density at a given height is not a function of the local temperature at that height, but a function of the entire temperature column. As governed by the hydrostatic relation $d(\ln p)/dz = -g/(RT)$, a deterministic derivation requires both a reference-level pressure $p(z_0)$ and the complete, co-located profile $T(z)$, i.e., $p(z) = p(z_0)\exp[-\int_{(z_0 \to z)} g/(RT)\,dz]$. In this approach, temperature errors at every level accumulate toward the target height. Furthermore, the retrieval precision of temperatures degrades rapidly with altitude between 80 and 100 km. Our regression takes the opposite, low-dimensional route: a single scalar, T80, statistically proxies the column state, with no need for the full temperature profile. T80 is anchored near 80 km (0.0215 hPa), where the retrieval is most tightly constrained and the SABER, MLS, and SOFIE records show the best cross-instrument consistency. We emphasize that this is a statistical rather than a deterministic result: T80 captures the dominant fraction of the interannual variance (>90%) at December high latitudes, rather than reproducing every individual density value as a full-profile integration would attempt to do.

In addition, the study provides two contributions not present in Zhang et al (2025c): (i)

A quantitative assessment of the relative roles of internal and external forcing. Using the multiple linear regression framework, we show that T80 alone accounts for more than 90% of the December density variance at summer high latitudes, whereas the solar f10.7 and linear trend terms are small and statistically insignificant and cannot be reliably isolated from the dominant internal variability. This demonstrates that the internal term dominates, while the external signals are obscured rather than reliably separated at high latitudes. (ii) A daily-scale analysis demonstrating that the T80–density linkage is robust beyond monthly means.

In our view, establishing a previously unmeasured, counterintuitive, and directly applicable field (the fixed-height density response) through observational analysis is a legitimate scientific contribution. The governing ideal-gas and hydrostatic relations are established physics, but the quantitative fixed-height density response, including the sign reversal and the 11 km vertical offset, cannot be obtained from the temperature study without the measurements presented here.

Sect. 4.1: We have added a more explicit statement of the mechanism.

- Section 3.3: The presented results, showing a lagged correlation between T80 and density, are not strong enough to support the claim of T80 having considerable predictive capabilities. In addition, using monthly means inherently benefits from low month to month variability. To actually demonstrate predictive capabilities, density anomalies should be considered instead.

We agree with the reviewer. The apparent one-month-lead correlation between the T80 in November and the density in December is mainly mediated by the strong month-to-month persistence (autocorrelation) of T80 itself: T80 in November is highly correlated with T80 in December, which is in turn associated with the December density. The apparent lead skill therefore contains no predictive information beyond the concurrent T80-density relationship and reflects the persistence of the index rather than an independent physical pathway.

As a result, we have withdrawn the claim of prediction. The former Sect. 3.3, “Statistical forecast of MLT density with one-month lead”, has been removed. All predictive language has been deleted throughout the manuscript, and the word "prediction" no longer appears in the title. The paper now quantifies and diagnoses the T80-density relationship rather than presenting it as a forecast.

In addition, as suggested by the reviewer, the density anomaly has been used throughout the work.

- Section 3.4: Here, T80 is calculated from SABER temperature to relate it to density, also calculated from SABER temperature and pressure. This strong dependence does not allow for an independent and meaningful analysis. Additionally, density perturbations should again be considered to truly test the predictive capabilities of T80.

We thank the reviewer for raising this point and address both the independence of the data and the use of perturbations.

(1) In the revised manuscript, the daily T80 index and the density are not derived from the same instrument. Over 2004–2025, the T80 index is computed from MLS temperature, while the neutral density is derived from SABER measurement. MLS and SABER are independent instruments with independent retrievals, so the T80–density relationship is a cross-instrument comparison rather than a self-correlation within a single dataset. Only the

2002–2003 extension of the T80 record uses SABER temperature. In fact, the daily relationship based on MLS T80 remains essentially unchanged (Figs. 6-8).

(2) The interannual analyses are now expressed as density perturbations (anomalies). For each calendar month the 24-year climatology is removed. This removes the deterministic seasonal background and makes explicit that the covariation reflects the interannual variability, meaning the departure of a given year from the climatology, rather than the mean state or the seasonal cycle. We note that the one-month-lead forecast in the original submission has been removed in response to the related comment, so the anomaly analysis here characterizes the interannual covariability rather than claiming predictive skill.

Lines 207-209: The daily T80 are described in detail.

Discussion (Section 4):

- Section 4.1: The study in its current form does not convincingly demonstrate that the effects "local thermal effect" and "column adjustment" are novel atmospheric mechanisms. To me, it seems more likely they directly follow from the treatment of the atmospheric column under the assumption of an ideal gas and hydrostatic balance.

We agree with the reviewer and have lowered the claim accordingly. The local thermal and column-adjustment effects follow directly from the ideal-gas relation and hydrostatic balance and are not presented as new atmospheric mechanisms; we now state this explicitly at the start of Sect. 4.1.

The contribution of this study is observational and quantitative rather than mechanistic. Using a single observable proxy (T80), we provide the first observationally constrained decomposition of the fixed-height MLT density response to interannual upwelling variability, including the vertical dipole, the sign reversal in the transitional layer, the latitude-dependent response, and the non-local response at 95 km. These structures are not given by the local identity $\rho \propto 1/T$, which holds on fixed pressure surfaces. They arise on fixed geometric heights through the movement of pressure surfaces. We have made this fixed-pressure versus fixed-geometric-height distinction explicit at the start of Sect. 4.1 and now frame the two effects as a thermodynamic decomposition rather than as novel mechanisms.

Lines 248-255: The column-adjustment effect and local thermal effect have been discussed, and in Sect. 4.1 we clarify that they are not new mechanisms.

- Section 4.3: In its current form, this analysis is not reproducible. I would include an explicit regression formula. Later, you state that the linear density trend contradicts the behavior expected from CO_2 cooling. You then attempt to explain this apparent inconsistency with a linear T80 trend that is insignificant. In addition, the claim that summer polar upwelling dominates the density variability is not supported by the results.

We thank the reviewer for this comment and have replaced the descriptive comparison with an explicit, reproducible multiple linear regression (MLR), now given as equation (1) in Sect. 4.3.

1. Explicit model. At each latitude and altitude we fit the December and June density as $\rho(t) = c_0 + c_1 t + c_2\, T80 + c_3\, f10.7 + \varepsilon(t)$, where t is the year since 2002. The coefficients c_1, c_2, and c_3 represent the linear-trend, upwelling, and solar f10.7.

2. Trend and the apparent CO_2 inconsistency. The original discussion of a density-CO_2 contradiction has been removed. The MLR shows that the linear trend term is

statistically insignificant at SH high latitudes, so no CO_2-driven trend is detected over the present record. Non-detection does not imply absence of a CO_2 signal; rather, the internal interannual variability is large enough to obscure it. In the NH, the trend term explains a larger relative share around 80-90 km, due to weaker internal background.

3. Dominance. In the SH summer high latitudes, T80 alone accounts for more than 90% of the December density variance while the f10.7 and trend terms are insignificant, so the upwelling-related component clearly dominates. In the NH, T80 remains the largest single predictor over the high-latitude upper MLT, but its high-contribution region is more confined and its overall share is smaller; the external terms take a larger relative share.

Lines 320-351: The explicit MLR and the interpretation of each coefficient are given in Sect. 4.3 and equation 1.

Lines 353-359: Figs. 11 and 12 provide the December and June contribution fields.

- Section 4.4: The authors mention that they intend to conduct the same analysis for NH summer months. It is unclear to me how this analysis would constitute a separate scientific contribution. Instead, I think it should be included in this study for additional robustness and generalization.

We thank the reviewer for this constructive suggestion and fully agree that the NH analysis should be part of the present study rather than deferred to future work. We have incorporated the complete NH summer (June) analysis into the revised manuscript.

The NH results are now presented within the same framework as the SH December analysis. This addition provides the requested generalization and also adds information beyond a simple replication. The same vertical density dipole and interhemispheric anti-phase pattern are reproduced, showing that the upwelling-based mechanism is not specific to the SH. At the same time, the coupling is quantitatively weaker in the NH: the peak interannual correlation is only about 0.6–0.7, compared with 0.98 in the SH, consistent with the smaller June T80 variability (σ(T80) = 1.6 K versus 5.1 K), and the external solar and trend terms therefore carry a larger relative share. These hemispheric differences are reported in Sect. 4.3.

Lines 112-126: The definition of T80 in June is provided in Sect. 2.2, and the values are listed in Table 1.

Lines 179-182 and 198-200: The interannual latitude-altitude structure of the T80-density relationship in June is shown in Sect. 3.2 and Fig. 5.

Line 227-229 and 240-244: The daily-scale T80-density relationship in the NH summer (May-July) is provided in Sect. 3.3 and Fig. 8.

Line 342-351 and 358-359: The multiple linear regression for June, including the partial variance contributions, is given in Sect. 4.3 and in Fig. 12.

- Throughout the manuscript, it is frequently implied that T80 causes density related variability. This could be misleading because T80 is a metric, whereas upwelling is the physical process causing density-related variability.

We thank the reviewer for this critical point. We agree that T80 is a metric, whereas upwelling is the physical process causing density variability. We have revised the manuscript to remove any implication that T80 itself exerts a physical influence.

Minor comments:

- Title: “Prediction” is an overstatement (see major comments on Section 3.3 and 3.4).

Agreed. The content about “prediction” has been removed throughout the paper.

- Lines 29-30: “practical necessity for satellite drag and orbit determination”: I would change practical necessity to something like “understanding facilitates” as the MLT is still below LEO altitudes.

We agree that the MLT lies below the main LEO altitudes, so describing the density as a "practical necessity" for satellite drag and orbit determination overstates the direct application. We have reworded the sentence to state that an improved understanding of MLT density variability facilitates satellite drag and orbit determination rather than being a prerequisite for them.

Lines: 29-30: the sentence has been revised as: “Quantifying MLT density (ρ) is a scientific imperative for understanding atmospheric coupling, and such understanding also facilitates satellite drag modeling and orbit determination”.

- Line 38: “Adiabatic dynamical processes” do not belong to external forcings.

We agree that the adiabatic dynamical processes induced by solar activity are an internal response of the atmosphere rather than an external forcing. Because this indirect dynamical pathway is not essential to the present study, we have removed it.

Line 38: The "adiabatic dynamical processes" has been removed from the sentence.

- Figure 3: Inverting the density axis could lead to confusion when noting the anticorrelation between T80 and density. At first glance, the plot appears to show a strong correlation.

We thank the reviewer for this suggestion. The density axis in Fig. 3 was indeed inverted (values increasing downward), which made the two series visually co-vary and could be misread as a positive correlation despite R = –0.80. We have reset the density axis to the standard orientation (values increasing upward), which displays the anti-correlation visually.

Lines 189-190: The Fig. 3 has been replotted.

- Line 54: When introducing the T80 index, it would be more helpful for a reader if you only reference one paper, for example the paper where the T80 index is introduced for the first time.

Agreed. Zhang et al (2025c) introduced the T80 index during solstice months for the first time.

Line 57: Only one paper, Zhang et al (2025c), has been cited to introduce the T80 index.

- Lines 194-195: You demonstrate a lagged correlation of T80 and density based on monthly means. Thus, the analysis does not support the claim of enabling “skillful (…) prediction of MLT density anomalies”. Also, density anomalies were not investigated.

Agreed. The content about “prediction” has been removed throughout the paper.

- Line 301: You mention that you want to isolate long-term CO_2 cooling. Since you show no explicit regression formula, it is unclear whether you actually included CO_2 cooling in your regression. This would need clarification as it is also of major importance for your derived conclusions.

We thank the reviewer for pointing out this ambiguity, which arose because the original manuscript did not state the regression model explicitly. We have now added the full multiple linear regression (MLR).

We would like to clarify how the long-term CO_2-related change enters the model. Over the 24-year record, atmospheric CO_2 increases almost monotonically, so the secular change attributable primarily to CO_2 is represented by the linear trend.

The linear trend contribution is small and statistically insignificant at the December southern high latitudes, where T80 alone accounts for more than 90% of the interannual variance. This is the basis for our conclusion that the long-term, primarily CO_2-driven signal cannot be reliably separated from the dominant internal variability in the current record, rather than indicating that CO_2 cooling was omitted from the regression.

In June, the interannual fluctuation of T80 is smaller, and the CO_2-driven trend can be extracted over the summer high latitudes.

Line 320-359: The contributions of T80, f10.7, and linear trend in the multiple linear regression are presented in Sect. 4.3 and in Figs. 11 and 12.

- Line 307: That statement is misleading. From Figure 12a alone, I do not see, where the density is influenced by upwelling (which is proxied by T80).

We thank the reviewer for identifying this mismatch. In the original manuscript, the cited figure displayed only the solar and linear-trend terms and did not include the T80 (upwelling) term, so the statement that density is influenced by upwelling could not be verified from that figure. We agree this was misleading.

We have removed the original figure and replaced it with a complete multiple linear regression presentation. The new Fig. 11 (December) and Fig. 12 (June) show, for each of the three predictors, both the sensitivity (left column) and the partial contribution to the interannual variance (right column). The upwelling influence is now directly visible. The T80 sensitivity and contribution in Fig. 11a-b show a strong positive response centered over the southern high-latitude upper MLT, where T80 alone accounts for more than 90% of the interannual variance, and the corresponding June result is shown in Fig. 12a-b. The statement in the text is therefore now supported by an explicit T80 panel rather than being inferred from the solar and trend panels.

Line 353-359: The multiple linear regression including the contributions of T80 is presented in the new Figs. 11 and 12.

- Lines 309-310: It is stated that no significant signal emerges, but Figure 12a indicates a significant area in exactly that region.

We thank the reviewer for this careful reading and agree that the apparent contradiction needs to be explained precisely. The two statements refer to two different quantities, an unconditional (simple) trend and a conditional (partial) trend.

The significant area in the original figure came from a simple regression of density on time without controlling for the other predictors. Over the summer high-latitude upper MLT, the density is dominated by the upwelling variability indexed by T80, which contains low-frequency fluctuations across the record. The simple regression therefore assigns to the time trend the variance associated with T80, producing the apparent positive trend near 95 km.

The multiple linear regression separates these two pathways. Once T80 and f10.7 are included, the variance associated with T80 is assigned to its partial term c_2, which is strong and accounts for more than 90% of the interannual variance at this location. The linear trend c_1,

estimated after conditioning on T80 and f10.7, is small and statistically insignificant (Fig. 11e, f). The significant area in the simple-trend plot is therefore not an independent secular trend; it is the T80 signal expressed along the time axis.

To remove this source of confusion, we have deleted the original simple-trend figure and now present only the MLR decomposition in Fig. 11 (December) and Fig. 12 (June), where the T80 and trend terms are shown in separate panels. The text has been reworded to state explicitly that the partial linear trend, rather than the density itself, shows no significant signal.

Line 353-356: The contributions of T80 and CO_2-driven linear trend is presented in the new Figs. 11.