

Response to Referees' Comments on egusphere-2026-3749, "Quantifying and Predicting Austral Summer Mesopause Height with a Temperature-Based Upwelling Proxy"

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 paper describes the relation between a temperature index, T80, and mesopause height of the summer hemisphere at high latitudes. The index, along with the mesopause height, is derived through the combination of SABER and MLS observations. Based on the correlation coefficients and further sensitivity study, especially the one between T80 and the mesopause height, the author argues that prediction of the summer mesopause height at high latitudes in the southern hemisphere is possible through the measurements of T80.

We sincerely thank the reviewer for the careful reading of our manuscript and for the fair summary of the study. We appreciate the reviewer's recognition of the core relationship between the T80 index and the summer mesopause height, as well as the prediction potential demonstrated in the paper.

We would like to take this opportunity to clarify one point regarding the data usage. The mesopause height is derived exclusively from SABER temperature observations, whose ~2 km vertical resolution is required to reliably locate the cold-point mesopause. The MLS data are used solely for the computation of the T80 index at 80 km, where MLS's 5–7 km vertical resolution is adequate. The two instruments are not combined to compute a single variable; rather, they serve independent roles. We apologize for any ambiguity in the original manuscript and have substantially revised Section 2 to make this distinction explicit, including a summary table of the data usage. We have also adjusted the wording in the Abstract and Conclusion to clarify the respective roles of SABER and MLS.

We address each of the reviewer's specific comments in detail in the following responses.

The work also highlights the so called "dipole response" in the mesopause region during the summer of southern hemisphere, which I think is a new terminology for a well-known phenomenon/process. Furthermore, the description is somewhat hand waving and a more scientifically thorough description is needed.

We thank the reviewer for this constructive comment. We agree that the summer polar MLT region exhibits well-known climatological and long-term opposite-sign temperature structures. In the revised manuscript, we have clarified that our "vertical temperature dipole" is not intended to be a new name for any of these established structures. Instead, it refers specifically to the interannual covariability between the summer polar temperature at 80 km (T80) and temperature anomalies.

We now explicitly state that the dipole is distinct from previously known phenomena: (1) Climatological upwelling/downwelling circulation. Below ~95 km, adiabatic cooling by

mesospheric upwelling produces the cold summer mesopause; above ~95 km, adiabatic heating by lower-thermospheric downwelling contributes to warmer temperatures [Qian *et al.*, 2017; Wang *et al.*, 2022]. This upwelling-to-downwelling transition is about 10 km above the mesopause, whereas our dipole's sign-reversal altitude is at the mesopause height (~88 km). (2) CO₂-driven shrinking effect. Long-term CO₂ increases cause radiative cooling in the mesosphere and a downward displacement of the temperature profile, producing an unexpected warming near the mesopause on multi-decadal timescales [Lübken *et al.*, 2013]. This is an anthropogenic secular trend, not year-to-year variability. (3) Ozone-hole-driven long-term changes. Simulations show that stratospheric ozone recovery can alter gravity-wave filtering and produce opposite-signed long-term temperature trends, with cooling below the mesopause and warming above it [Lubis *et al.*, 2016; Smith *et al.*, 2010]. Again, this is a multi-decadal trend driven by stratospheric chemistry, not interannual variability.

In contrast, the vertical temperature dipole in our study is defined as the T80-regressed interannual temperature anomaly pattern. Its defining characteristics are: (i) a sign reversal that closely coincides with the mesopause height; (ii) a structure derived from correlations with T80; and (iii) an interannual time scale. The T80 index itself has only recently been introduced as a quantitative proxy for upwelling intensity [Zhang *et al.*, 2025]. To our knowledge, this particular T80-linked interannual pattern has not been explicitly quantified before.

To address the reviewer's concern that the description was somewhat hand-waving, we have now added a quantitative definition in Section 4.1. The revised text specifies the vertical profile of the T80-temperature correlation, the regression sensitivity, the precise altitude of the sign reversal, and the latitudinal extent over which the pattern is robust. We have also strengthened the mechanism discussion by acknowledging that the positive temperature anomalies above the mesopause can be maintained both by adiabatic warming from the lower-thermospheric downwelling and by chemical/radiative heating.

We thank the reviewer again for prompting this clarification.

The author conducted considerable amount of work, including the data from two satellite and calculations on several parameters. But the efforts of data processing of the two instruments are missing. It is critical, because the two instruments have very different sampling scheme, thus, the biases due to long-period waves are different. This and the long-term consistency of the instruments need to be handled carefully, when combining the two measurements, and should be addressed in the early part of the paper.

We thank the reviewer for this important comment. We would like to clarify a key point: the two instruments are used for different variables, not combined to compute the same quantity. Specifically:

(1) Mesopause height is computed exclusively from SABER, because locating the cold-point mesopause requires high vertical resolution, which SABER provides. MLS is not used for mesopause height due to its coarser vertical resolution.

(2) The T80 index is computed primarily from MLS (2004-2025), with SABER used for cross-validation and to extend the series backward to 2002–2003 (before Aura/MLS launched).

Because the two instruments are not fused to compute a single variable, the differences in sampling scheme and long-period wave biases do not affect the mesopause height results. More details on the definition of T80 and mesopause height are given below respectively.

The close relation between T80 and mesopause height at high latitudes is interesting, but I would not conclude it can be utilized to forecast the mesopause height in the coming month. On monthly basis, there are little changes of mesopause height once the dynamic and thermal balance is achieved. Therefore, this claim is somewhat misleading, in my opinion.

We thank the reviewer for this valuable comments. We agree that in the summer polar region, the month-to-month persistence of mesopause height itself provides a valid basis for estimation (November-December $R = 0.85$), consistent with the reviewer's observation that the dynamical and thermal balance is established by November. However, the T80 index provides a fundamentally different and more capable predictor, with three key advantages:

(1) Broader predictive reach as a dynamical proxy for summer polar upwelling. The autocorrelation of mesopause height is strongly latitude-dependent: it is high in the polar region where the summer dynamical state is already established in November, but weakens substantially at mid-latitudes (e.g., near 50°S), where the stable summer regime is not fully established until December. The November mesopause height at mid-latitudes therefore cannot reliably predict the December value ($R = 0.48$ at 50°S). In contrast, the T80 index captures the large-scale state of summer polar upwelling, which influences the mesopause across 30°S -pole ($R > 0.8$). The November T80 index thus predicts December mesopause height across this entire latitudinal range, including mid-latitudes where local persistence fails. This is not merely local autocorrelation: it reflects how polar upwelling variability modulates thermal structure and mesopause height over a broad region.

(2). Higher measurement precision. The SABER-derived mesopause height requires locating the coldest point in the temperature profile, a procedure sensitive to vertical resolution, temperature noise, and the interpolation. T80, in contrast, is a direct temperature measurement at a fixed pressure level (~ 80 km), with substantially smaller uncertainty. Consequently, even in the polar region where persistence is valid, November T80 predicts December mesopause height with higher skill ($R = 0.90$ vs. $R = 0.85$).

(3). Broader and more durable data availability. T80 can be derived from multiple satellite instruments (e.g., MLS, SABER, SOFIE), whereas the SABER-derived mesopause height depends specifically on SABER's high vertical-resolution temperature profiling. As SABER approaches the end of its mission, the mesopause height may become unavailable, but T80 can continue to be provided by other MLT temperature sounders.

In short, the T80 index is not merely a substitute for mesopause-height persistence: it is a more precise, physically meaningful, and durable predictor with higher skill. We have revised the manuscript to avoid overstatement, and we thank the reviewer again for prompting this clarification.

The author also states that "... the summer mesopause height in the SH is almost perfectly controlled by the T80 index..." in the conclusion, which is an overstatement, I think.

Agreed. This sentence has been changed to "this study demonstrates a strong linear relationship between the summer mesopause height in the SH and the T80 index". We thank the reviewer for helping us improve the precision of the manuscript.

Both T80 and mesopause height are symptoms, not the source that "controls" and leads to these symptoms. Thus, the author needs to modify some of these statements.

We fully agree with the reviewer. Both the T80 index and the mesopause height are symptoms or indicators of the underlying dynamical forcing — primarily gravity-wave driving and the associated residual circulation—rather than the source that controls one another. The T80 index serves as a proxy for upwelling intensity, and the mesopause height responds to the same dynamical forcing through adiabatic and chemical/thermal processes. We have revised all statements that imply a direct causal "control" relationship, replacing them with neutral expressions such as "is associated with" or "is linked to".

The other results presented in the paper are mostly expected and/or well known, but, overall, the paper is decent.

We sincerely thank the reviewer for the overall positive assessment of the manuscript. We appreciate for the constructive comments throughout, which have helped us improve the precision of the manuscript. We hope that the revisions described above have strengthened the manuscript and addressed the reviewer's concerns.

It does, however, need some clarification on the data processing. The most critical T80 is loosely defined in figure caption of Fig. 1, which is quite strange. I would like to see detailed definition and algorithm for the derivation of this index. The author refers Zhang et al., 2025c, but I think it is important to have it clarified in the early section in this manuscript.

We fully agree with the reviewer that the T80 index was insufficiently defined in the original manuscript and that relying solely on the reference to Zhang et al. (2025c) was inadequate. We have thoroughly rewritten Section 2.2 and the caption of Fig. 1 to provide a complete definition and derivation of T80:

(1). T80 is defined as the monthly, zonal-mean temperature at 0.0215 hPa (~80 km) averaged over the summer polar cap (75°S–82°S for MLS; 75°S–83°S for SABER; the slight difference reflects the differing latitudinal coverage of the two instruments).

(2). The choice of 80 km is physically justified: the very low ozone concentration at this altitude makes radiative and chemical heating negligible, so the temperature is controlled primarily by adiabatic processes, and the cross-instrument agreement is highest there.

(3). The computation algorithm is now described step by step: orbit-level profiles are averaged into daily and zonal means over the polar cap (poleward of 75°S), then averaged into monthly means.

(4). The cross-calibration between SABER and MLS is performed using monthly linear regressions over their overlapping period, and the series is merged into a continuous 24-year (2002–2025) T80 record.

Section 2.2 now contains all these details and no longer depends on the external reference.

In addition, how the mesopause height is defined and calculated? This work is based on combined SABER and MLS observations. Yet, only SABER is mentioned in the abstract and conclusion sections.

We thank the reviewer for pointing out the need for a clearer definition of the mesopause height and for noting the inconsistency in the abstract and conclusion.

As noted in our earlier response, the mesopause height is computed exclusively from SABER, while MLS data are used solely for the T80 index. The mesopause height is defined as the altitude of the temperature minimum (cold point) between 80 and 105 km. The calculation

proceeds as follows:

(1) The daily and zonal-mean SABER temperature profiles are computed by averaging all available profiles at each 5° latitudinal bin.

(2) The temperature minimum between 80 and 105 km is identified, and its altitude is recorded as the daily mesopause height.

(3) Monthly mean mesopause heights are then computed from the daily values,.

We have added these detailed definition to Section 2.1 of the revised manuscript, and the abstract and conclusion are also revised correspondingly.

Minor technical issues:

Line 94, “zonal-mean temperature near 80 km” need to be defined in details.

Thanks. Done.

Line 113-114, this statement is confusing, needs to be rephrased.

Agreed. Corrected.

Line 125, in caption of Fig. 1, it should be red-solid lines and blue-dashed line, respectively. What does the black solid line represent in a, b and c?

Thanks. Corrected.

The black solid line in Fig. 1(a-c) represent the number of days for SABER data available at the high latitudes (53°S-83°S), which varies from year to year due to the shift of satellite orbits.

Line 153, the word “perfect” is used in several places in the manuscript. I would not use it in a scientific publication.

Agreed. The word “perfectly” has been replaced by “strongly”.

Line 175, in caption of Fig. 3, “red diamonds” and “blue dots”

Thanks. Done.

Line 245, in captions of Fig. 7 and 8, please define the red and blue solid lines in the figure.

Thanks. Done.

Response to Referee #2

This manuscript shows a strong correlation between mesopause height and the T80 index and argues that the T80 index “controls” mesopause-height variability and can be used to predict interseasonal mesopause height. I agree that the temperature near 80 km is strongly correlated with mesopause height and that both temperature and mesopause height may exhibit substantial persistence on subseasonal to seasonal timescales during summer. Therefore, it may be possible to estimate mesopause height in December/January from the temperature in November/December.

We sincerely thank the reviewer for the careful reading of our manuscript and for the constructive summary of this study.

However, I have several concerns regarding the physical interpretation of the results. I am also not yet convinced that the scientific advance demonstrated in the present manuscript is sufficiently clear for publication in ACP. In particular, the manuscript needs to better distinguish correlation from causality and to clarify the scientific value of predicting monthly mean mesopause height.

We thank the reviewer for these important concerns. We recognize that the original manuscript did not sufficiently distinguish correlation from causality, and that the scientific value of the mesopause height prediction was not articulated clearly enough. We also acknowledge that the physical interpretation of the 90 km warming could benefit from a more thorough discussion of alternative mechanisms.

We have revised the manuscript extensively to address these concerns. Specifically: (i) we have replaced all causal language ("controls," "dominates") with neutral, correlation-based terminology ("is associated with," "serves as a proxy for," "is linked to") throughout the manuscript; (ii) we have added a new paragraph in the Introduction and Discussion explicitly articulating the scientific significance of this work; (iii) we have substantially revised the physical interpretation in Section 4.1 to explicitly discuss both dynamical adiabatic heating (from residual circulation downwelling) and chemical/radiative heating as contributing mechanisms; and (iv) we have clarified the precise nature, timescale, and limitations of the prediction.

I suggest that the authors address the following four major comments before publication. I therefore recommend major revision.

We sincerely thank the reviewer for the thorough evaluation and for the recommendation of major revision. We have revised the manuscript extensively to address each of the four major comments, as detailed in our point-by-point responses below.

Major comments

1. Interpretation of lower-thermospheric warming

The authors, following Zhang et al. (2025c), appear to attribute the warming above approximately 90 km mainly to chemical and radiative processes. However, I am not convinced that this is the only, or necessarily the dominant, interpretation. Dynamical processes associated with changes in the residual circulation can also produce mesospheric cooling together with lower-thermospheric warming through adiabatic cooling and heating.

For example, Miyoshi et al. (2015) demonstrated with a whole-atmosphere model that the high-latitude mesosphere cools while the lower thermosphere warms during an SSW event. They showed that this temperature response is associated with changes in gravity-wave drag and the residual circulation. The review by Baldwin et al. (2021) also discusses several studies demonstrating the impact of circulation changes on lower-thermospheric temperature during SSWs (e.g., Liu and Roble, 2002; Funke et al., 2010).

Importantly, the numerical model used by Miyoshi et al. (2015) reproduced the lower-thermospheric warming through dynamical changes, demonstrating that changes in the residual circulation alone can make a substantial contribution to this temperature response.

I therefore wonder whether the chemical and radiative effects proposed in the present manuscript are sufficiently large to dominate over the dynamical/adiabatic temperature response associated with the

same circulation changes that produce mesospheric cooling. In particular, the observed correlation between O/O₃-related chemistry and lower-thermospheric temperature does not by itself establish that the chemical/radiative processes cause the temperature variations. Both quantities could potentially respond to a common dynamical driver, such as changes in the residual circulation.

A quantitative separation of the dynamical and chemical/radiative contributions would probably require additional modeling and may be beyond the scope of the present manuscript. Nevertheless, I strongly suggest that the authors discuss the alternative dynamical explanation, particularly adiabatic heating associated with changes in the residual circulation, and clarify that the relative contributions of dynamical and chemical/radiative processes remain uncertain.

We thank the referee for this incisive comment and for bringing the dynamical perspective to our attention. We agree with the referee that the warming above approximately 90 km cannot be unambiguously attributed solely to chemical and radiative processes. As the referee correctly notes, enhanced downwelling associated with a strengthening of the residual circulation could itself produce adiabatic heating in the lower thermosphere, accompanying the adiabatic cooling near 80 km. The examples of Miyoshi et al. (2015) and the SSW-related studies summarized in Baldwin et al. (2021) clearly demonstrate that dynamical processes alone can generate mesospheric cooling together with lower-thermospheric warming.

Firstly of all, we want to clarify that in our previous paper [Zhang et al., 2025] the downwelling has also been viewed as an important alternative mechanism, as shown in the section 4.3 therein. We feel sorry for not fully citing the explanation of that work.

We acknowledge that our observational dataset, based primarily on temperature measurements, does not allow a quantitative separation of the relative contributions of dynamical adiabatic heating and chemical – radiative heating from O₃/O. Both mechanisms are consistent with the observed dipole structure, and both may well operate simultaneously. We have revised the Discussion to explicitly present the dynamical/adiabatic explanation as an important alternative to the chemical – radiative pathway, and we have added the references suggested by the referee. We now explicitly state that the relative contribution of the two mechanisms remains uncertain and that a definitive attribution would require coordinated model experiments with fully interactive chemistry and dynamics, which we consider beyond the scope of the present observational study.

At the same time, we wish to emphasize that our main conclusions—namely, the strong association between T80 and mesopause height, the coherent vertical dipole structure, and the subseasonal predictability—do not depend on which of the two heating mechanisms dominates. Regardless of whether the lower-thermospheric warming is caused by dynamical downwelling or chemical-radiative heating, T80 remains a robust indicator of the residual circulation strength that drives both the mesospheric cooling and the mesopause height variability. We hope this revision adequately addresses the concern.

2. Scientific significance and applicability of the prediction

I am not yet convinced that the scientific significance of the main finding is sufficiently clear. Because gravity-wave-driven residual circulation is a major control on temperatures in the mesosphere and lower thermosphere, a strong relationship between mesospheric temperature and mesopause height may be expected from the underlying dynamics.

The authors should therefore clarify what new physical understanding is provided by the

relationship between T80 and mesopause height beyond the known dynamical coupling in the MLT region.

We thank the reviewer for this important comment. We agree that, given the well-established role of the gravity-wave-driven residual circulation in controlling MLT temperatures, a statistical relationship between a temperature-based dynamical index and mesopause height may be qualitatively expected. However, we believe the specific relationship documented here is not a simple restatement of known dynamical coupling. We have revised the manuscript to more clearly articulate three aspects that go beyond this qualitative expectation.

1. Temperature change does not automatically imply mesopause height change. A uniform warming or cooling of the 80-100 km layer would alter the mesopause temperature but leave its height nearly unchanged, since the cold-point minimum would shift in value rather than in location. A vertical displacement requires a change in the vertical shape of the temperature profile. Our central finding is that the T80-associated temperature response is a dipole whose turning point almost exactly coincides with the climatological mesopause height. This coincidence—not merely the existence of dynamical coupling—is what physically enables the strong T80-mesopause height relationship. If the turning point were situated only ~ 10 km higher or lower, the temperature profile near the mesopause would warm or cool nearly uniformly, and the height sensitivity would largely disappear.

2. The observed dipole structure itself has mechanistic implications. The lower thermospheric downwelling usually locates at ~ 10 km above mesopause [Qian *et al.*, 2017; Wang *et al.*, 2022]. The adiabatic heating of downwelling would produce a vertically broad warming pattern above ~ 95 km, and it would not naturally generate a sharp reversal of the temperature response locked to the mesopause. The strongly negative correlation above 90 km suggests that an additional heat source—most plausibly chemical/radiative heating associated with enhanced O_3 and O —plays an active role. Even if part of the O_3 and O enhancement is supplied by downwelling transport from the lower thermosphere, the resulting thermal effect is realized through exothermic chemical reactions and subsequent radiative relaxation, not through adiabatic compression alone. The summer polar mesopause height is therefore governed by a coupled dynamical-chemical-thermal response, which is less a priori obvious than a purely dynamical expectation. The relative contributions of adiabatic and chemical heating remain an open question.

3. Our results provide a quantitative demonstration of the dominance of internal dynamics over external forcing on interannual timescales in this region. The T80 index alone explains more than 90% of the interannual variance of the December mesopause height at high latitudes, while the 11-year solar cycle signal and the CO_2 -trend signal cannot be robustly separated from this internal variability within the 24-year record. Internal dynamical variability is therefore the first-order term governing mesopause height variations on interannual timescales in the summer polar MLT. This has a direct implication for long-term trend studies: any credible attribution of solar or CO_2 -related signals in this region must account for internal variability, and the T80 index provides a practical way to do so.

I am also uncertain about the practical or scientific value of predicting monthly mean mesopause height one or more months in advance. If this prediction has applications to other research areas—for example, atmospheric coupling, space weather, long-term climate studies, or interpretation of upper-atmospheric

observations—the authors should explain these applications explicitly in the Introduction and/or Discussion. This would help establish the broader significance of the proposed prediction method.

We thank the reviewer for this constructive comment. We agree that the broader significance of the prediction should be clarified. We have added a brief discussion in the revised manuscript that addresses the following points.

1. The prediction skill itself. The MLT is often viewed as a highly variable region. Our result that the November T80 index predicts the December mesopause height with $R = 0.90$ demonstrates that the large-scale residual circulation evolves coherently on subseasonal-to-seasonal timescales, providing a new observational constraint on MLT dynamics. To our knowledge, predictability at this lead time and skill has rarely been demonstrated for MLT parameters.

2. Implications for other MLT observations. Mesopause height is closely linked to the thermal and density structure of the MLT. A one-month-ahead estimate of mesopause anomalies therefore has potential relevance for interpreting PMCs, metal layers, airglow, and the satellite drag environment. Whether these related fields can also be predicted with similar lead times remains an interesting open question.

3. Empirical model improvement and evaluation. Empirical climatological models (e.g., NRLMSISE-00) do not represent year-to-year dynamical variability of the MLT. The T80-based relationship offers a practical way to estimate mesopause height, and the observed relationship also provide a useful test for whole-atmosphere models.

We note that these are potential applications of the demonstrated predictability, which is beyond the scope of this paper. We hope these clarifications address the reviewer's concern.

3. Use of the term “control”

Line 260 and several other parts of the manuscript state that “the T80 index controls the mesopause height variability.” I do not think that the word “controls” is physically appropriate here. Temperature at 80 km does not itself drive or control mesopause height. Rather, T80 and mesopause height may both respond to common dynamical processes, particularly gravity-wave forcing and the associated residual circulation. Indeed, the authors themselves discuss such a mechanism in Lines 268–270.

I therefore suggest replacing “controls” with a more neutral expression such as “is strongly associated with,” or “serves as an indicator of” mesopause-height variability throughout the manuscript. The distinction between correlation and causality is particularly important for the interpretation of the results presented here.

We fully agree with the referee that “controls” is not physically appropriate. T80 is a temperature index and does not itself drive mesopause height variability. Instead, both T80 and mesopause height respond to common dynamical processes, primarily gravity-wave forcing and the associated residual circulation. We have replaced “controls” with more neutral expressions such as “is associated with” and “is linked to” throughout the manuscript.

4. Section 4.2: CO₂ and solar-cycle effects

I strongly suggest reconsidering, or possibly removing, Section 4.2 in its present form. Numerous previous studies have demonstrated detectable effects of increasing CO₂ and solar variability on temperatures and/or the thermal structure of the mesosphere and lower thermosphere using SABER and

other observations (e.g., Ramesh et al., 2015; Tang et al., 2016; Zhao et al., 2020, 2021; Mlynczak et al., 2022; Liu et al., 2024).

Therefore, the absence of statistically significant relationships in the present analysis should not be interpreted as evidence that CO₂ or solar variability has little influence on mesopause variability. Rather, the lack of statistical significance may result from the methodology used here, particularly because solar-cycle, long-term CO₂-related, dynamical, and other sources of variability are not explicitly separated.

If the authors wish to retain this section, I suggest applying an appropriate multiple-regression framework that separates at least the long-term trend and solar-cycle contribution, together with other major sources of variability where possible. Otherwise, I recommend removing this section or substantially revising the interpretation.

We thank the reviewer for this important comment and for bringing the relevant references. Following the reviewer's suggestion, we have retained Section 4.2 but substantially revised it by applying a multiple-regression framework that explicitly separates the contributions of internal dynamics (T80), the solar cycle (F10.7), and the long-term trend (representing CO₂ forcing).

1. Multiple-regression analysis. We applied a multiple-linear regression to the December mesopause height across 80°S-50°N:

$$h(t) = c_0 + c_1t + c_2T80(t) + c_3F10.7(t) + \varepsilon(t),$$

where h is the December mesopause height, t is the year (the linear trend representing CO₂ forcing), T80 is the dynamical index, and F10.7 is the solar proxy. Importantly, the T80 index during 2002 to 2025 is uncorrelated with F10.7 ($R = 0.08$) and with the linear trend, so the regression does not suffer from collinearity.

The results are shown in the revised Figure 13. In the summer hemisphere—where this study focuses—the T80 index alone explains more than 90% of the interannual variance of the December mesopause height, while the solar contribution is small and statistically insignificant. The CO₂-related linear trend is not statistically significant in the summer hemisphere but becomes significant in the winter hemisphere.

2. Why solar variability affects mesopause temperature but not mesopause height. We fully agree with the reviewer that solar activity influences temperatures in the mesosphere and lower thermosphere, as demonstrated by the cited studies. However, the present study analyzes the cold-point mesopause height rather than mesopause temperature, and a solar signal in temperature does not necessarily imply a solar signal in height.

As illustrated in Figure 11, if solar radiative heating acts coherently across the mesopause layer, enhanced solar activity raises the temperature of the entire vertical profile more or less uniformly. This produces a vertical translation of the temperature profile—the mesopause becomes warmer, but its cold-point altitude remains essentially unchanged. In contrast, the T80-related dynamical variability produces a dipole temperature response, which directly shifts the temperature profile and thus displaces the cold point.

3. CO₂-driven contraction. The absence of a detectable CO₂ trend in the December summer-hemisphere mesopause height does not contradict the well-established role of CO₂ in long-term mesospheric cooling and contraction. First, this study focuses on the summer months, when internal dynamical variability is strongest. The CO₂-induced linear signal is small compared to the interannual variability. Second, we note that many previous studies that

detected significant solar and CO₂ signals analyzed annual-mean or global-mean quantities, in which dynamical variability is largely averaged out and external forcing signals are correspondingly easier to detect. In the austral summer months at high latitudes—the focus of this study—the dynamical signal dominates, and the solar and CO₂ signals are simply too weak to be detected against this background within the 24-year record.

We thank the reviewer again for these valuable comments, which have led us to substantially strengthen Section 4.2.

Minor comments

Figure 1: If possible, please add standard errors or another measure of uncertainty.

Thanks. Done. The error bars have been added, which represent the standard deviation of mean. The values are typically of about 0.5 K to 0.6 K, much smaller than the interannual fluctuations of T80 index (~ 5K).

Lines 170–171: The wording appears to imply that T80 drives the upwelling. T80 should instead be regarded as an indicator or proxy of the circulation/upwelling. The upwelling is dynamically driven, primarily through wave forcing and the resulting residual circulation. Please revise the wording accordingly.

We thank the reviewer for this clarification. We fully agree that T80 is an indicator/proxy of the upwelling rather than its driver—the upwelling itself is dynamically forced by gravity-wave breaking and the associated residual circulation. We have revised the similar phrasing throughout the manuscript to make this relationship explicit, replacing any wording that could imply T80 drives the upwelling with neutral expressions such as "the T80 index serves as a proxy for" or "is associated with" the summer polar upwelling.

Line 215: I am not convinced that the method demonstrated here predicts “seasonal” mesopause-height variability. It appears to predict a monthly or subseasonal mean state based on the persistence/correlation between preceding and subsequent months. Please clarify the terminology and the actual prediction timescale.

Agreed. We have revised the terminology throughout the manuscript—replacing "seasonal" with "subseasonal-to-seasonal" or "one-month-ahead" where appropriate—and have clarified in the relevant sections (including the Abstract and Section 3.3) that the actual prediction timescale is a one-month lead time for the monthly mean state.

Section 3.4: Please explain more clearly the motivation for examining daily variability and how this analysis contributes to the main objectives of the manuscript.

We have revised Section 3.4 to clarify the motivation for the daily-scale analysis. A new paragraph at the beginning of the section now explicitly states three purposes: (1) to test the robustness of the T80-mesopause height relationship across timescales, (2) to identify the latitudinal domain where the relationship remains significant on daily timescales, and (3) to quantify the influence of transient wave noise. The summary at the end of the section has also been strengthened, specifically, the consistency of sensitivities across timescales supports a common underlying mechanism.

Reference list

- Baldwin, M. P., et al. (2021): Sudden Stratospheric Warmings, *Reviews of Geophysics*, 59, e2020RG000708. DOI: 10.1029/2020RG000708. *This is a strong review citation for SSW-driven changes extending into the thermosphere; it explicitly summarizes lower-thermospheric warming found by Liu & Roble and Funke et al.*
- Miyoshi, Y., Fujiwara, H., Jin, H., and Shinagawa, H. (2015): Impacts of sudden stratospheric warming on general circulation of the thermosphere, *JGR: Space Physics*, 120. DOI: 10.1002/2015JA021894. *Particularly useful for Major Comment 1 because it explicitly shows high-latitude mesospheric cooling, lower-thermospheric warming, and a reversal/change of circulation around 90–125 km.*
- Zhao et al. (2020): Long-Term Trends and Solar Responses of the Mesopause Temperatures Observed by SABER During the 2002–2019 Period, *JGR: Atmospheres*. DOI: 10.1029/2020JD032418. *Directly relevant to separating long-term and solar-cycle effects in SABER mesopause temperatures.*
- Tang, C., et al. (2016): The response of the temperature of cold-point mesopause to solar activity based on SABER data set, *JGR: Space Physics*, 121, 7245–7255. DOI: 10.1002/2016JA022538. *Direct evidence that solar activity effects on mesopause temperature can be detected using SABER.*
- Ramesh, K., Sridharan, S., and Vijaya Bhaskara Rao, S. (2015): Influence of solar cycle and chemistry on tropical (10°N–15°N) mesopause variabilities, *JGR: Space Physics*, 120, 4038–4051. DOI: 10.1002/2014JA020930.
- Mlynczak, M. G., et al. (2022): Cooling and Contraction of the Mesosphere and Lower Thermosphere From 2002 to 2021, *JGR: Atmospheres*, 127, e2022JD036767. DOI: 10.1029/2022JD036767. *Particularly strong for your argument because it explicitly separates contributions from declining solar activity and increasing CO₂ and quantifies MLT contraction.*
- Liu, X., Xu, J., Yue, J., Liu, Y., and Andrioli, V. F. (2024): Trends in the high-latitude mesosphere temperature and mesopause revealed by SABER, *Atmospheric Chemistry and Physics*, 24, 10143–10157. DOI: 10.5194/acp-24-10143-2024. *This is especially relevant because it specifically examines high-latitude mesosphere/mesopause trends.*

We sincerely thank Reviewer #2 for recommending these references and for the valuable descriptions provided. All of the suggested references have now been cited in the revised manuscript. We are grateful for the reviewer's guidance, which has helped us strengthen the discussion.

References:

- Lubis, S. W., Omrani, N.-E., Matthes, K., and Wahl, S. (2016), Impact of the Antarctic Ozone Hole on the Vertical Coupling of the Stratosphere–Mesosphere–Lower Thermosphere System, *Journal of the Atmospheric Sciences*, 73(6), 2509–2528, doi:<https://doi.org/10.1175/JAS-D-15-0189.1>.
- Lübken, F.-J., Berger, U., and Baumgarten, G. (2013), Temperature trends in the midlatitude summer mesosphere, *J. Geophys. Res.-Atmos.*, 118(24), 13,347–313,360, doi:<https://doi.org/10.1002/2013JD020576>.
- Qian, L., Burns, A., and Yue, J. (2017), Evidence of the Lower Thermospheric Winter-to-Summer Circulation From SABER CO₂ Observations, *Geophys. Res. Lett.*, 44(20), 10,100–110,107, doi:<https://doi.org/10.1002/2017GL075643>.
- Smith, A. K., Garcia, R. R., Marsh, D. R., Kinnison, D. E., and Richter, J. H. (2010), Simulations of the response of mesospheric circulation and temperature to the Antarctic ozone hole, *Geophys. Res. Lett.*, 37(22), doi:<https://doi.org/10.1029/2010GL045255>.
- Wang, J. C., Yue, J., Wang, W., Qian, L., Wu, Q., and Wang, N. (2022), The Lower Thermospheric Winter-To-Summer Meridional Circulation: 1. Driving Mechanism, *J. Geophys. Res.-Space*, 127(12), e2022JA030948, doi:<https://doi.org/10.1029/2022JA030948>.
- Zhang, L., Liu, Z., and Tinsley, B. (2025), Interhemispheric Anti-Phase Variability in Mesospheric Climate Driven by Summer Polar Upwelling During Solstice Months, *Atmos. Chem. Phys.*, 25(20), 13141–13159, doi:<https://doi.org/10.5194/acp-25-13141-2025>.