Comments on "Impact of the Indian Ocean Sea Surface Temperature on the Southern Hemisphere Middle Atmosphere" by Yang et al.

This study investigates the impacts of the midlatitude Indian Ocean sea surface temperatures (SSTs) on the Southern Hemisphere middle and upper atmosphere based on the proposed midlatitude Indian Ocean Dipole (MIOD) index. The authors show that positive MIOD events enhance planetary-wave propagation from the Indian Ocean sector, leading to variations in temperature, zonal winds, as well as a strengthening of the residual meridional circulation, while negative MIOD events have relatively weak impacts on the Southern Hemisphere middle and upper atmosphere. The issues tackled in this study are worthwhile and well within the scope of this journal. However, some conclusions are lack of sound verification. It needs major revisions before it is accepted for publication. The following are some specific comments and suggestions:

1. Line 38-39: The stratospheric thermal radiation only can not insert significant influences on both tropical and extratropical circulation， it is radiative-chemical-dynamic coupling that is important.

Response: Thank you for the comment. We agree that stratospheric impacts on circulation arise from the combined effects of radiative, chemical, and dynamical processes rather than thermal radiation alone. Accordingly, we have revised the sentence in lines 37–41 to read:

"Stratospheric processes—including thermal radiation and radiative–chemical–dynamical coupling—have been shown to influence both tropical and extratropical circulation, with further effects on surface temperature (Joshi et al., 2006; Maycock et al., 2013; Shindell, 2001; Solomon et al., 2010; Tandon et al., 2011)."

This revision clarifies that it is the coupled radiative–chemical–dynamical processes that underpin the stratosphere's influence on the climate system.

2. Line 104-105: The statement "Yet the atmospheric background conditions during austral winter are more favorable for planetary wave propagation into the stratosphere" needs reference support.

Response: Thank you for the comment. We have added a citation to **Charney and Drazin (1961)**, which demonstrated that planetary waves can propagate vertically only under westerly background flow, thereby providing the theoretical basis for why austral winter conditions favor upward planetary-wave propagation.

3. *Line 164-166: what is the top level of MERRA-2 reanalysis? The WACCM6-SD run at the model top near 140 km. On which model level does the nudging begin to perform?*

Response: Thank you for the comment. We have added a clarification in lines 164–170 of the revised manuscript as "*In the SD configuration, meteorological fields are nudged toward MERRA-2 reanalysis every six hours to reduce internal variability and model bias. WACCM6 is nudged toward MERRA-2 below approximately 0.1 hPa (~50–60 km), with a smooth tapering of the relaxation coefficient near the upper boundary of the nudged region. Above this altitude, including the mesosphere and lower thermosphere, the model evolves freely. This setup allows the stratospheric variability to follow the reanalysis while retaining internally generated dynamics in the mesospheric region*".

4. *Line 180: "between 40 and 80 kilometers" >>>"between 40 and 80 km"*

Response: Revised.

5. *Line 346: "positive-phase MIOD events" >>> "negative -phase MIOD events"*

Response: Revised.

6. *Line 362: what is hgt?*
7. *Line 368: HGT>>hgt*

Response: Thank you for the comment. We have clarified in the revised manuscript that *hgt* denotes geopotential height, as introduced in the Data section. In addition, the inconsistent appearance of the uppercase form *HGT* has been corrected, and the notation has been standardized to *hgt* throughout the manuscript to ensure clarity and consistency.

8. *Line 440: Figure 6: Longitude>>Latitude*

Response: Revised

9. *Line 510: "ozone deletion" >> "ozone decrease". The depletion generally means destroyed rather the transported.*

Response: Thank you for pointing this out. We agree that "ozone depletion" may imply chemical destruction rather than transport-related decreases. We have revised the wording in the manuscript and now use "ozone decrease" to accurately describe the transport-driven changes.

10. My major concern is related to Section 4. This section presents the results in the mesosphere. It looks strange to put those results in Discussion Section. Are those results are preliminary?

Response: Thank you for raising this important point. Section 4 is not intended to introduce preliminary or additional observational results. Instead, its purpose is to extend the analysis vertically into the mesosphere and to provide a dynamical interpretation of how the MIOD-related stratospheric perturbations documented in Section 3 may project upward. Because gravity-wave processes and MLT variability cannot be directly observed, we combine merged HALOE–SABER temperature data with the free-running mesosphere of SD-WACCM6 to evaluate whether the observed mesospheric patterns are dynamically consistent with those generated internally by the model. To clarify this intent and avoid the impression that Section 4 presents a separate set of results, we have revised the opening paragraph as "*The stratospheric responses described above suggest that MIOD-related perturbations may extend upward into the mesosphere, raising the question of how far the influence of MIOD projects vertically. To investigate the full vertical structure of the atmospheric response, we complement the stratospheric analysis with merged HALOE–SABER temperature observations spanning 10–100 km and SD-WACCM6 simulations. Because the free-running nature of SD-WACCM6 above ~50–60 km allows the mesosphere–lower thermosphere (MLT) variability to evolve independently of the imposed stratospheric state, the comparison between observations and model output provides a basis for examining whether the mesospheric anomalies inferred from observations are dynamically consistent with those that arise internally in the model. This framework enables us to assess potential pathways through which MIOD-related stratospheric perturbations may influence the mesosphere, without presupposing the underlying dynamical mechanism*" in lines 666-677 of the revised manuscript to provide a smoother transition from the stratospheric analysis and updated the title of Section 4 to better reflect its interpretative nature.

11. Line 540：Figure 9：above 80 km, there is no consistency between the satellite observations and model results. Is it due to nudging approach?

Response: Thank you for the comment. The lack of consistency above ~80 km arises from several factors. First, SD-WACCM6 is nudged toward reanalysis only below approximately 0.1 hPa (~50–60 km), and the mesosphere–lower thermosphere above this level is fully free-running. As a result, the model does not reproduce event-specific variability in the upper mesosphere that is not directly controlled by the imposed lower-atmospheric state. Second, the observational composite (HALOE–SABER, 1991–2022) and the model composite cover different sampling periods, which may further contribute to differences at altitudes where internally generated variability dominates. We have added a clarification in the revised text as "*However, the midlatitude warming in mesosphere/lower thermosphere region seen in observations is largely absent in the model, and the tropical anomaly remains below 1 K and is not statistically significant. This discrepancy between the observations and SD-WACCM6 may indicate that the processes giving rise to the upper-mesospheric and lower-thermospheric response are not fully captured, as SD-WACCM6 is not constrained in the mesosphere. An additional contributing factor may be the non-overlapping portions of the observational record (1991–2022) and the model simulation period used here.*" (Lines 700-706) to make these points explicit.

12. Line 590-594: The authors stated that "Discrepancies between thermal wind estimates and reanalysis winds are largely attributable to planetary wave breaking". This is not true! various processes may have contributions to those discrepancies.

Response: Thank you for pointing this out. We agree that our original formulation was overly assertive and did not adequately reflect the range of processes that can lead to discrepancies between thermal-wind estimates and reanalysis winds. Our EP-flux and planetary-wave diagnostics suggest that wave forcing is a plausible contributor, but other processes not explicitly analyzed here (e.g., diabatic heating and radiative–chemical tendencies) may also play a role. We have therefore revised the sentence as "*Deviations between thermal-wind estimates and reanalysis winds further point to a dynamical contribution from planetary-wave forcing, although diabatic, radiative, and chemical processes may also play a role*" in lines 748-750 of the revised manuscript to state that planetary-wave forcing likely contributes to the discrepancies, while acknowledging that additional processes may also be important.

13. Line 597-598: The authors stated that "The influence of the MIOD extends into the mesosphere and lower thermosphere (MLT) through gravity-wave filtering modulated by stratospheric wind perturbations". This statement has no support.

Response: Thank you for the comment. We agree that the original statement overstated the vertical extent and certainty of the mechanism. We have revised the wording to reflect only the supported mesospheric response and to frame the gravity-wave contribution more cautiously. The revised sentence now reads: *"In the mesosphere, SD-WACCM6 produces a response that is structurally similar to satellite observations. Within the model, MIOD-related stratospheric wind anomalies modulate gravity-wave filtering and wave-mean flow interactions, leading to coherent mesospheric drag and circulation anomalies. While discrepancies persist, particularly at higher altitudes, these results indicate that gravity-wave filtering provides a physically plausible pathway linking MIOD-related stratospheric disturbances to the mesospheric response"* in lines 751-756 of the revised manuscript.

14. Line 625-627: "The findings are consistently supported by satellite observations and WACCM6 simulations, lending robustness to the identified SST atmosphere coupling". However, there are no any comparisons between the model results and satellite observations in the stratosphere.

15. Line 628-629: "with the Southern Hemisphere atmosphere being more sensitive due to its unique background circulation during winter". There are no any discussions on this statement.

Response: Thank you for these helpful comments. We agree that the original wording overstated both the degree of observational–model consistency and the interpretation of hemispheric sensitivity. We have revised the conclusion to make clear that the consistency between satellite observations and SD-WACCM6 refers specifically to the mesospheric response. We have also removed the statement that the Southern Hemisphere atmosphere is "more sensitive due to its unique background circulation during winter" and now frame the role of MIOD more generally as a potential additional driver of large-scale atmospheric variability alongside established influences such as ENSO and the QBO. This part has been revised as "*Satellite observations and SD-WACCM6 simulations further indicate that MIOD-related anomalies extend into the Southern Hemisphere mesosphere, with the model suggesting a role for gravity-wave drag modulation in linking stratospheric wind anomalies to the mesospheric response. The MIOD-related atmospheric signal identified here indicates that Indian Ocean SST variability acts as an additional source of large-scale dynamical variability in the Southern Hemisphere, complementing established influences such as ENSO and the QBO, and highlighting a previously underappreciated pathway through which tropical ocean variability affects the middle and upper atmosphere on interannual timescale.*" In lines 773-780 of the revised manuscript.

16. Line 632-633: "The analysis further suggests that long-term trends in Indian Ocean SST may have contributed to the observed variability in Antarctic ozone depletion and recovery". There are no any discussions on the long term trends of variables. How can you draw this conclusion?

Response: Thank you for pointing this out. We agree that the original sentence introduced an implication regarding long-term SST trends and ozone variability that was not directly analyzed in this study. Since our focus is on the interannual response of the middle and upper atmosphere to MIOD variability, we have removed this statement from the conclusion to avoid overinterpretation.