We would like to express our gratitude for the valuable feedback from the Referee. We appreciate your tireless efforts in reviewing our manuscript so it allows us to improve the standard of our article.

RC1

This work is devoted to an interesting and insufficiently studied topic. Results of variations in mesospheric inversion layers are presented, but there are some comments:

1. Line 41: missing letter «e» in mesosphere

   Response: We have corrected the typographical error "mesospher" by mesosphere in line 40.

2. Line 74-77: The sentence is too long. And Figure 1 shows temperature, not causatives. Please rephrase.

   Response: We have modified and corrected the sentence in lines 80-84 in the new revision as well as corrected the expressions of Figure 1.

3. Figure 2 (as well as Figure 7.8) is not entirely clear. There are too many black isolines and the fill behind them is not visible. Try increasing the size of Figures and thinning out the isolines. Maybe leave one isoline that satisfies the inversion criterion.

   Response: We have enlarged Figures 2, 7, and 8 for visibility as well as rewritten the sentences for clarity.

4. Line 140-141: You claim that the base of the lower MILs lies in the range of 73-79, the upper MILs is 86-89 km. Sorry, but I don't see this in Figure 2. Why did you indicated these heights?

   Response: We have corrected the sentences in lines 178–18, and we can see the corresponding Figure 2.

5. I don't understand why histogram 4(c) shows a maximum MILs at 78 km, but histogram 4(f) does not have a maximum at this altitude.

   Response: Thank you. Figure 4(c and f) shows the number of upper and lower inversions in the given intervals of base height ~ (70–80) for the lower MLT region and base height ~ (80–90) for the upper MLT region. Figures 4(c) and 4(f) are corrected in the revised manuscripts.

6. In my opinion, the conclusions are overloaded with numbers. All parameters are listed in the text of the manuscript.

   Response: Thank you for your valuable suggestion. The conclusion has been modified based on your suggestion.

7. To what extent is it physically justified to explain the MILs by changes in the Brunt-Vaisala frequency? After all, in essence these are the same things; if there is an inversion, it means the atmosphere is not stable.

   Response: In this investigation, the Brunt-Vaisala frequency is only used to show the instabilities in the Mesospheric inversion layers (MILs).

8. The manuscript also lacks interpretation of physical processes. There is no attempt to speculate about the sources of gravity waves. Although Figure 1 clearly shows a quasiperiodic structure of temperature fluctuations. The periodicity is also visible in Figure 3. How does this relate to the QBO phase? Or with other processes in the low-latitude atmosphere. Perhaps winds and shears in the mesosphere generated gravity waves. Your figures show significant interannual variability. What features were there in 2012-2013 and 2019? Gravity wave potential energy is increased at longitudes 35-40E, maybe the source was the lower layers of the atmosphere, for example, The Low-Level Somali Jet? Discussions about the physical nature of the origin of gravity waves should be added to the manuscript.

   Response: We thank you for your valuable suggestion. After carefully considering your recommendations we tried to modify the interpretation of physical processes even the source of the gravity waves and their impacts are clearly stated in the manuscript. We can see section 3.4.

RC2

Comments on, **"The Role of Gravity Waves in the Mesosphere Inversion Layers (MILs) over low latitude (3-15° N) Using SABER Satellite Observations"** by Lingerew and Raju. Using sixteen years of SABER temperature data, the authors investigated the role of gravity waves (GWs) in the mesospheric inversion layers. To understand the role of GWs in the MILs, they estimate the potential energy, and based on the results they argue that the lower and upper MIL distinctions are due to the GWs. The strength of the manuscript is they used a long-term data set however their methodology is not clear. Moreover, this manuscript also lacks the scientific discussion. The present form of the manuscript needs major changes before acceptance for publication. Therefore, I recommend to the editor for a major revision. The detailed major and minor comments are as follows:

Major comments:

1. In section 2, latitudinal information of the data used is given however there is no information about the longitudes! Which reading the whole manuscript, I could see the longitudinal limits of 32 to 48° in Figures 10 and 11 (in section 3.4). Are the temperature profiles averaged over 3-15°N and 32-48°E? If so mention it in section 2. More importantly, the information about how do the MILs are identified is missing. They have only written as a diagnostic technique is used. What kind of diagnostic technique, whether the authors validated the diagnostic method all this information should be provided in the methodology (e.g. Gan et al., 2012; Sivakandan et al. 2014, etc.).

Response: Thank you for your comment. Based on your comment, we have mentioned the criteria to separate the inversion phenomenon from the observation data in lines 92–106 by including the references you mentioned in Section 2 from lines 101-104, as well as adding the longitudinal information in line 82.

2. One of the major issues in the manuscript lack of a literature survey, though they have cited some of the important papers (Meriwether and Gardner 2000; Gan et al., 2012) but the essential points from those papers are not reflected in their approach. There are various sources proposed as the causative mechanism of the lower and upper MILs. For example, the planetary waves are believed to be the causative mechanism of lower MILs similarly, gravity wave tidal interactions and chemical heating are proposed as a cause of upper MILs. These points are not considered and there is no reason why the authors only focus on the GWs. It is well understood that in most of the cases the GWs breaking in the mesosphere can cause only very few Kelvin temperature changes (>10K). If this is the scenario it cannot explain the higher amplitude MILs. Comment on it.

Response: OK, thank you. all tidal, planetary, and gravity waves, as well as chemical reactions, are causative of an inversion, but here in our study region between 60 and 90 km, only the gravity wave is considered, which is generated from the lower atmosphere and propagated to the upper atmosphere till it reaches the saturation level for breaking and impacts the atmospheric variability as causative of an inversion.

3. As mentioned in comment 1, the authors should provide longitudinal information, because this has an important role if they try to understand the role of GWs which are highly localized in nature. It is not clear how the 1hr cutoff frequency applies to the data, if the authors used a particular region then in a day maximum of two to three satellite passes can be observed based on the area, with this limited data set how effective or logical is the 1hr band pass filter?

Response: we have used only SABER temperature data over the spatial regions of latitude (3–15), longitude (32-48), and altitude (60–90 km) during 2005-2020. So, as mentioned in the text, we have applied a one-hour interval cut-off frequency of the low-pass band filter to separate the

gravity waves from those other wave activities, such as planetary and tidal atmospheric wave impacts. For more clarity, we can see the explanations of the techniques of how to use the low pass band filters, which have a 1-h cut-off frequency to separate the short periodic gravity waves, from tidal and planetary waves in lines 148-153.

4. Why 3rd order polynomial fit? Ramesh and Sridharan (2012) do not elaborate on any method, instead they have cited Leblanc and Hauchecorne (1997). Therefore the article cited here is not relevant. Provide more information about the methodology and its validity (how good it is? if the authors did any test to validate the method etc.)

Response: The scientific community prefers the 3rd-order polynomial fit to estimate the background information relative to other orders (Leblanc and Hauchecorne, 1997; Ramesh and Sridharan, 2012).

5. Lines 138-140; in this context, Gan et al., (2012) could be a more suitable paper to cite here than Sivakumar et al. (2001), because they also used SABER data, on the other hand, Sivakumar et al. (2001) only used Rayleigh lidar data over a single location (the data quality above 80 km is questionable). Gan et al. (2012) also found the seasonal variation of MILs in the low latitudes and planetary waves as the cause of lower MILs, whether these authors could find such a relationship? If yes or no provide reasons!

Response: We have used the relevant references to support our findings about the base of the lower inversions as presented in lines 178–182.

6. There is no clear information about how the occurrence frequency is estimated. Provide it?

Response: The occurrence rate (percentages) for lower and upper inversions is estimated by counting the number of inversion days every month from 2005 to 2020.

7. How the mesopause altitudes are taken care or eliminated from the statistics? Which could be a false indication of inversion. And could the authors note any solar activity dependency of MILs occurrence (for example, Sivakandan et al. (2014))?
Response: We did our work about inversions and their causative gravity waves in upper and lower MLT dynamic regions over low latitudes, but we didn't consider the mesopous.

8. Lines 151-155, In the literature there are different causative mechanisms are proposed for the multiple MILs, (I suggest the authors go through Meriwether and Gardner (2004); Gan et al. (2012)).

Response: We have different causative atmospheric waves for an inversion, such as planetary waves, tidal waves, and gravity waves, as well as chemical reactions, but here we are considering only gravity waves as an impact on an inversion.

9. Section 3.2, is a good point to investigate but before doing that the data need to be binned properly with local time. I am a bit concerned about how good to investigating the latitudinal

and longitudinal variations in a small region using satellite data, each temperature profile could be nearly 500 km spatial averaged.

Response: Thank you for your comment. However, it is not a local time investigation; instead, we have used the period from 2005 to 2020 over the latitudinal (3–15) and longitudinal (32-48) spatial regions.

10. The scientific discussion is very spare and weak. They should compare the present results with earlier studies based on the similarities and differences the scientific reasoning also should be included in the manuscript.

Response: Thank you for your suggestion to improve our manuscripts. Hence, based on your comment, whole sections of the manuscript were developed by rewriting the scientific discussion in the revision.

11. How the GWs potential energy is connected to the MILs? First establish the connection by showing a single case in which a physical connection should be clear and then go for the statistics.

Response: Thank you. We have presented their connections in Section 3.4, particularly in lines 310–313, as follows: “The saturation stage of the wave propagation is broken at the upper region to dissipate the energy, which impacts the normal mesospheric temperature by increasing its temperature with elevation, known as an inversion. This is the reason the gravity wave potential energy is connected with an inversion.”

Minor comments:

12. Lines 8-9, The mesosphere…This is a transitional region not only in the low latitudes! So modify the statement.

Response: The sentence you mentioned in lines 8–9 is modified as follows: “The Mesosphere transitional region is a distinct and highly turbulent zone of the atmosphere."

13. Lines 39-40, define the MILs.

Response: OK, thank you. We have modified the inversions, MILs, in lines 38–40 as follows: “The mesospheric inversion layers (MILs) are a common feature that appeared to increase the mesosphere temperature variability.

14. Line 41, a typo, ‘mesosphere’

Response: OK, this typo error is corrected

15. Line 73, these references are irrelevant here. Provides references about the data validation and limitation as well as instrumental specifications.

Response: We have added the references to express the validation and the limitations of SABER observations in the MLT region in lines 75-79.

16. Line 75, longitudinal information is missing!

Response: Ok now it is corrected in the revised manuscript from line 82.

17. Figure 4: Sivakandan et al. (2014) also did such a statistical analysis using the SABER data over Indian low latitudes, could you compare the present results with their results and provide some scientific reasoning for the observed differences or similarities?

Response: Yes really we have seen his scientific work results as a comparison with our work results and we mention their difference and similarity based on their statistical analysis in the newly revised manuscript from section 3.1.

18. Line 218 …that the inversion temperature is in the range of…It is not an inversion temperature range only a temperature range.

Response: It is the inversion-day observed temperature.

19. Line 242 onwards, the longitudinal information is suddenly introduced here, it should be introduced in section 2.

Response: Now it is corrected by introducing the longitudinal information in section 2.

20. Lines 245-247, these lines are not clear. Please see the major comment 3.

Response: It is corrected by rewriting the sentences in lines 316-326

21. Figure 5b, a typo 'thickness' References suggested to read and compare with the present results and include in the discussion part (some of the articles are cited here but those results are not utilized to improve the discussion part):

Response: We have corrected the typo error and included the references you have mentioned below based on their relevance.

1. Gan, Q., S. D. Zhang, and F. Yi (2012), TIMED/SABER observations of lower mesospheric inversion layers at low and middle latitudes, *J. Geophys. Res.*, 117, D07109, doi:10.1029/2012JD017455.
2. Meriwether, J. W., and C. S. Gardner (2000), A review of the mesosphere inversion layer phenomenon, *J. Geophys. Res.*, 105(D10), 12405–12416, doi:10.1029/2000JD900163.
3. Sivakandan, M., Kapasi, D., and Taori, A.: The occurrence altitudes of middle atmospheric temperature inversions and mesopause over low-latitude Indian sector, Ann. Geophys., 32, 967–974,https://doi.org/10.5194/angeo-32-967-2014,2014.
4. Ramesh, K., S. Sridharan, and S. Vijaya Bhaskara Rao (2014), Causative mechanisms for the occurrence of a triple layered mesospheric inversion event over low latitudes, *J. Geophys. Res. Space Physics*, 119, 3930–3943, doi:10.1002/2013JA019750.

RC3

The manuscript presents an investigation of the likely effects of atmospheric gravity waves on Mesospheric Inversion Layers (MILs) in equatorial latitudes. Indeed, the topic is interesting and not explored very well by the communities. Additionally, it is within the scope of Annales Geophysicae because it is an experimental investigation of the mesosphere using satellite measurements. Before I consider the paper suitable for publication, some concerns could be explored and revised by the authors to improve the quality of the manuscript. In general, the observations present in all figures were not explained or explored very well. Consequently, I missed consistent interpretations of the present results.

1. For example, Figure 1 shows the variability of the temperature in the mesosphere and lower thermosphere, what is the objective of it? Is it possible to see MILs, where? If not, why is it not possible to see? In summary, how can Figure 1 help the authors?

   Response: Figure 1 presents the observed temperature variability of the MLT region before segregating the inversions.

2. Figure 2 shows upper (left) and lower (right) MILs, to be sincere, I did not understand the bottom panels. May the authors explain them better? I also suggest enlarging captions and the size of the panels.

   Response: The upper panel of Figure 2 (a) and (c) represents the upper and lower MLT SABER observed temperatures whereas the lower panel of Figure 2 (b) and (d) represent the upper and lower inversions of the MLT temperature variability, respectively, which is after separated from Figure 2 (a & c). The figure is now enlarged.

3. Figure 3: what is the bin size and which criteria were used to determine the percentage of occurrence of MILs?

   Response: The occurrence rate (percentages) for lower and upper inversions is estimated by counting the number of inversion days for every month from 2005 to 2020.

4. Figure 4: Are the red curves indicating a Gaussian fit? If yes, please, explain it in the text. Please, note that the statistic used for panel (c) does not represent the data, in this case, the authors could use another statistic or explain what are causing the discrepancies.

   Response: The red curve fitting is the Gaussian fit distribution, which is a probability distribution that is symmetric about the mean, showing that data near the mean are more frequent in occurrence than data far from the mean, which is mentioned in lines 199–205. I

have mentioned their statistical values by corrected the Figure 4(c) & 4(f) in the revised manuscripts.

5. Figure 5: What is the relevance of these results and how could they be related to gravity waves? I guess the quality of the presentation of this figure could be improved by enlarging the caption and size. Figure 6: Same comments as Figure 5.2.

   Response: Before investigating the impacts of gravity waves on the MLT inversions, the spatiotemporal (time vs. latitude) variabilities of an inversion are presented in Figures 5 and 6 to characterize the upper and lower inversions, respectively. The figure is now enlarged for clarity.

6. I recommend the authors use a more complete expression of the potential temperature (Vadas and Fritts, 2005; Vdas, 2007) instead of what was presented in Equation (1). In addition, the discussion of atmospheric stability is very superficial and it does not include a real aspect of the atmosphere that is certainly present in the SABER data. I suggest including an example of the methodology used to calculate the MILs in the real data (section 2). In my opinion, it could help the readers to promptly understand the process.

   Response: Thank you for your valuable suggestion. Based on your recommendation, we have used the references [(Liu, 2011; Vadas and Fritts, 2005); (e.g. Gan et al., 2012 and Sivakandan et al. 2014)] along with equations 1–13 to elaborate on the expressions of the gravity wave potential energy instead of using a single equation, as well as calculating the Brunt-Vaisala frequency, N^2, to characterize atmospheric stability in the methodology part of Section 2.2 for more clarification for readers.

7. Section 3.4: The authors wrote that they used a low pass filter to exclude the effects of tidal and planetary waves in the residual signal. If I understood the process, I guess they could use high-frequency filters to maintain low periods. Indeed, the blue lines in Figure 9 show a smoothed signal that excludes short-time variations. If I am correct, Figures 10 and 11 could be revised and the interpretations as well.

   Response: we have used a 1-h interval cut-off frequency of a low-pass band filter, in which Figure 9 shows smooth data signals by removing the peak while using 1-h interval perturbed temperature data profiles to exclude more than a 1-h time variation. The discussion part of the results in Figures 10 and 11 is nearly rewritten.

8. Lines 279-282: "The result concludes that the observation of high potential energy in the upper mesosphere region is due to the deposition of high energy and momentum at the background temperature by gravity wave breaking, which could influence the dynamics of the inversion phenomenon". Lines 291-293: "This result leads us to the conclusion that a high amount of gravity wave potential energy is a consequence of the high instability of the upper inversion relative to the lower." I guess, it is possible to reach these conclusions from the present work. Conclusions: I think it will be better to change the name of the section to Summary and exclude the last two ones that are very general.

   Response: The sentences are removed from the sections you mentioned in lines 279–282 and 291-293; instead, they are included in the summary section in lines 391–395. The subtitle, Section 4, conclusion, is changed to summary. The references you commented on are included in the revised manuscript.

Reference

1. Garcia-Comas, M., Lopez-Puertas, M., Marshall, B. T., Wintersteiner, P. P., Funke, B., Bermejo-Pantaleon, D., Mertens, C. J., Remsberg, E. E., Gordley, L. L., Mlynczak, M. G., and Russell III, J. M.: Errors in Sounding of the Atmosphere using Broadband Emission Radiometry (SABER) kinetic temperature caused by non-local-thermodynamic-equilibrium model parameters, J. Geophys. Res., 113, D24106, doi:10.1029/2008JD010105, 2008.
2. Meriwether, J. W., and Gerrard, A. J.: Mesosphere inversion layers and stratosphere temperature enhancements, Rev. Geophys., 42, RG3003, http//doi:10.1029/2003RG000133, 2004.