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.).
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.

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 bandpass filter?

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.)

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!

6. There is no clear information about how the occurrence frequency is estimated. Provide it?
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))?
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)).
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 investigate the latitudinal and longitudinal variations in a small region using satellite data, each temperature profile could be nearly 500 km spatial averaged.
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.
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.

Minor comments:

12. Lines 8-9, The mesosphere…This is a transitional region not only in the low latitudes! So modify the statement.
13. Lines 39-40, define the MILs.
14. Line 41, a typo, ‘mesosphere’
15. Line 73, these references are irrelevant here. Provides references about the data validation and limitation as well as instrumental specifications.
16. Line 75, longitudinal information is missing!
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?
18. Line 218 …that the inversion temperature is in the range of…It is not an inversion temperature range only a temperature range.
19. Line 242 onwards, the longitudinal information is suddenly introduced here, it should be introduced in section 2.
20. Lines 245-247, these lines are not clear. Please see the major comment 3.
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):

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.