Review of "Spatial and temporal variation in long-term temperature and water vapor in the mesopause Region" by Gul et al.

This article investigates long-term change in temperature and water vapor using observations from the NASA SABER satellite instrument. The quality of writing, overall organization, and implementation of the English language are substandard, which detracts from the task of reviewing the scientific merit of the work. Regarding the scientific quality of the work, I believe there are major flaws that lead me to recommend rejecting this paper. It seems that poorly written papers are increasingly common, and I feel that the community is in danger of either lowering our standards or exhausting the review process.

Regarding the scientific quality of the paper, I have some important concerns which are the basis for my recommendation to reject this paper. Foremost is that there is no description of how the Authors determined trends from the observations. There are numerous resources that describe the derivation of trends from geophysical observations, and the Authors need to consider these methods and include the references. An important factor here is that the observed parameter is being modulated by another forcing mechanism, perhaps one that is periodic in nature, and that this dependence contaminates the derived trend. Of relevance here is that temperature and H_2O in the mesosphere respond to the 11-yr solar cycle (see references in this paper), with less H_2O and higher T near solar maximum. Looking at Figure 3, there is a clear 11-yr. solar cycle dependence in T and H_2O (solar maxima were roughly 2002 and 2013). This is extremely important because the SABER time series begins near solar maximum and ends near solar minimum, giving the appearance of a massive cooling trend (and rising H_2O). If the Authors derived their trends from simple linear regression to the time series, then the results are likely not representative of the actual trends due to rising greenhouse gasses. The trends should be derived using multiple linear regression with the inclusion of at least two terms, 1) the solar cycle (e.g., using Lyman – alpha) and 2) time (i.e., the trend). The trend derived in this manner will be less affected by haphazard alignment between the observations and the solar cycle, as is clearly evident here. Many authors would choose to also include terms such as the QBO, AO, and ENSO. Again, there are accepted ways to do this and the Authors must adopt these approaches and describe what they did in the paper.

The paper suffers from a high degree of ambiguity in the presentation of their results, but also in the quotation of results from previous work. For example, look at the paragraph starting on line 470. The quoted trends are widely varying, yet there is no mention of the relevant latitude, altitude, or season for these results, and it is bewildering to try and make sense of it. This is just one example of the inadequate writing in this paper, and I feel that publishing these results in the present form would do more harm than good.

While this paper should be rejected, I believe that the subject matter is of interest and that it could represent a useful contribution after major revisions. To this end I offer some high-level suggestions below, but refrained from commenting on the ubiquitous flaws in writing, organization, and English, as this would consum too much time. I hope that the Authors will find some expert help to improve the writing and use of English. Please note that properly revising this paper will require *much* more consideration than offered in my comments below.

Specific Comments

1) The writing, organization, and use of English are substandard. To demonstrate this point, examine the first four sentences of the introduction:

> "Mesopause is one of the complex and intricate domain regions of Earth's atmosphere. It is the thermal transition area that plays an important role in the vertical coupling of the Earth's atmosphere. In the global mean temperature, mesopause is the coldest layer of the atmosphere (Zhao et al., 2020; Ortland et al., 1998). Polar summer mesopause is considered the coldest place on Earth (Ortland et al., 1998)."

The first sentence is confusing and serves no purpose. The second sentence is vague, and technically incorrect because much of the atmosphere is involved in some aspect of vertical coupling. The third and fourth sentences are awkwardly stated and somewhat redundant.

One more example from lines 99-100:

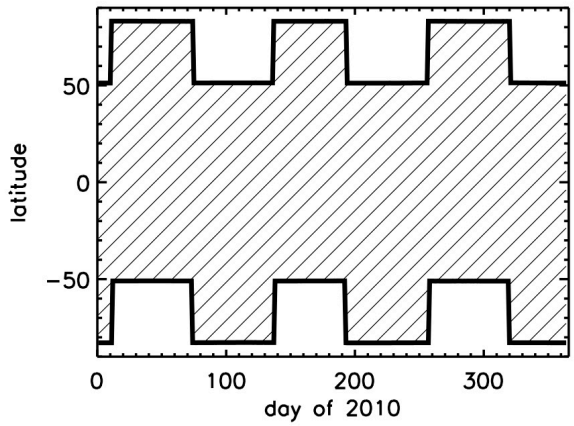
> "Spatially the region was divided into four parts (North Pole, Equator, and South Pole). Two two-degree latitude areas were selected for all longitude ranges (Figure 1)."

The first sentence states that the region (the globe?) was divided into 4 parts then mentions only 3, while the second sentence goes on to discuss only 2 latitude bands.

2) Figure captions: A figure caption must describe every aspect of the image, including details of the results (such as latitude, time, and height), and the origin of the results (such as "SABER observations" or "model results", or "trends derived from linear regression", etc…). It is not acceptable to do this only in the text, and then force the reader to go back and forth to understand a figure. An acceptable caption would be something like this (using Fig 4 as an example):

> **Figure 4.** Temperature and water vapor at 80 km altitude from SABER observations near the equator (0° ± 1° latitude). a) Time series of yearly mean T and H_2O, based on months as indicated in Figure 4b. Trends are also shown, which were determined using multiple linear regression to the results. b) T and H_2O time series for individual months as indicated. c) T and H_2O versus month for individual years as indicated.

3) Figure 1: This illustration is not needed, as most readers already understand these concepts. What would be much more useful is a plot of the SABER latitude coverage vs. time, as this is somewhat complicated. I show an example below of how this could be done. Note also that the SABER latitude vs. month is slowly changing over the years, and one must be very careful when constructing a 20+ yr. time series. For example, coverage of high northern latitudes included July in early years, but not during the recent several years. As a result, the Author's choice of June and July for high latitudes introduces a systematic bias in the time series, in that July is no longer represented in recent years. Illustrating these aspects of the data would be much more useful and relevant.



SABER latitude coverage versus time for observations during 2010.

4) Latitudes used in the study: Given the excellent coverage provided by SABER, is there a reason to examine such narrow latitude bands (±1°), and only three latitudes (80°S, 0°, 80°N)? Regarding the ±1° latitude bands, I would generally expect a reduction in random variability for averaging over a wider latitude range (e.g., ±5°). The global mean (latitudes from 80°S - 80°N) is referred to as the "whole mesopause", which is ambiguous. Just call it the global mean. Furthermore, creating a global mean temperature can be misleading, since this approach combines different seasons in both hemispheres, and for SABER will include biases introduced by the changing latitude coverage with month (and year). For this reason, the best "global" representation of SABER data would be 52°S – 52°N, as has been done by previous authors.

5) Altitudes used in the study: It is not clear what altitudes were used for the T and H_2O shown in the paper. I think it might be an average for 80-100 km (based on the legend in Fig 1), but it is not really stated clearly anywhere. This is an important point because the SABER errors increase rapidly with height. Furthermore, you should probably not mix measurements below and above the mesopause in a single average. In any case the paper should describe, and justify, the altitudes examined. Additionally, the Authors should consider looking at all altitudes.

5) Table 1: There appears to be a wealth of useful information here, but the results are poorly described and somewhat confusing. For example, the list appears to contain both absolute values of T and H_2O, in addition to trends in these quantities. The trends are also listed alternately as per year or per decade, and this needs to be rectified. Results are also given for a wide range of altitudes, which is problematic given the strong altitude dependence in T and H_2O in the mesopause region. In addition, numerous investigations have shown that trends in the upper mesosphere vary strongly with height (references listed in this paper), and can even change sign at roughly NLC altitudes (depending on latitude and season). Given these complexities, the presentation of results in Table 1 needs to be substantially revised, including a consideration of the altitude dependence. Perhaps these results would lend themselves to being plotted vs. height instead. Finally, Table 1 neglects the H_2O trends derived from SABER and MLS observations by Yue et al. (2022, GRL; reference given in this paper), which are particularly relevant to the present study.

6) Figure 7: These results are not described very well. What altitude is this for? Are you comparing Northern March to Southern March and Northern Sep. to Southern Sep.? If so then the differences are not meaningful as they are for two different seasons (e.g., spring vs. fall). Also, the diagram of Earth's orbital positions is not needed. Finally, a better illustration of these results would be a line plot of difference vs. year.

7) Figure 8: Why are you comparing the temperature and H_2O, for summer and winter, in different hemispheres? If your aim is to illustrate the seasonality then do it in the same hemisphere. If you are concerned with differences between hemispheres, then compare the same seasons in the north and south (e.g., for summer use June in the north vs. December in the south). Later in the text (line 421-423) you quote large seasonal differences of 156, 210, and 186K. Nowhere in the results are such differences evident, and this should be checked.

8) Throughout the article you refer to the "north pole" and "south pole", yet your results are for 80°N and 80°S, which are not the poles. Please be specific and use the nomenclature 80°N and 80°S.

9) Figure 10: The results are very hard to interpret, please try another approach.