Response to Reviewer #1

We highly appreciate the insightful comments and suggestions provided by the reviewer, which have helped us enhance the quality of our manuscript. We have carefully considered each comment and implemented the suggested revisions or provided detailed clarifications where appropriate. Note that our responses are represented in blue.

Wildfires have been a hot topic recently due to more frequent and stronger influences due to global warming. This study investigated the influences of atmospheric pollutions from wildfire biomass burning in the HTP. Multi-source data (including MODIS fire produce, CALIPSO, ground-based AERONET and so on) and SBDART simulations were used to test the influences of wildfires on temperature structures over the region. It is found that the wildfire activity in 2021 is possible to contribute mid-tropospheric warming and alterations in the vertical temperature structures. The manuscript is well organized and presented, and both the observations and simulations are well discussed. The paper could be considered for publication after addressing following specific comments.

Thank you for your thoughtful and helpful comments on our manuscript. We have revised our manuscript taking all these comments into account.

General comments:

1. On the conclusion of the manuscript: This study compared the observations during wildfire active (2021) and less-active (2022) years, and indicated the influences due to wildfire from the differences. This is the main conclusion of the paper, while it should be presented more carefully due to the annual and seasonal differences on the atmospheric temperatures between the two years.

Thank you for the insight comment. We acknowledge the importance of considering annual and seasonal differences in atmospheric temperatures between 2021 and 2022. This is exactly why we chose to compare temperature lapse rates rather than temperatures. The temperature lapse rates reflect the vertical temperature structure and present similar regulatory patterns in the typical atmosphere. In our study, we observed a consistent reduction in the vertical temperature lapse rate in regions affected by wildfires, while no such change was detected at the NADOR site, which is situated far from the influence of wildfires. We recognize that further robust evidence, including model results and additional measurements, are necessary in future work to strengthen our findings.

2. A large number of observations and retrievals of different kinds was used in this study to tackle the problems, and they have quite different accuracy. A brief discussion on the accuracy of different observations is suggested to be given (maybe in the supporting materials), because it will help the authors as well as the readers to better understood the reliability of the conclusions.

Thank you for pointing that out and we highly agree. We now have added a description of "data quality" for different types of data included in Part 2: Data and Method.

Specific comments:

3. For some of the figures, the colors of similar kinds were used, which may make the comparing confusion. For example, the red lines in Figure 8 can be hardly differed. Similar conditions were noticed in Figure 2 as well.

We highly appreciate the reviewer's detailed comments and suggestions. We have adjusted the colors in both Figure 8 and Figure 2 to enhance clarity and avoid confusion during comparisons.

4. I suppose the word "fire counts" indicates the number of pixels detected as fires by MODIS product, and this could be different for general understanding of fire count. Please correct me if I am wrong, while the concept may be confusing for readers if I am right.

You are correct regarding the term "fire counts" and we appreciate your insight on this matter. Actually, many studies use "fire counts" based on this definition. To avoid confusion, we have replaced "fire counts" with "fire pixel counts" in the main text and used "number of fires" in all figures.

5. The vertical profiles of absorbing aerosol play an important role for the heating of aerosol radiative effects, Lu et al. (https://doi.org/10.1016/j.atmosres.2020.104891) gave an estimation of heating due to aerosol based on their numerical regression. Their estimations may be briefly compared with this study.

Thank you for the suggestion and we appreciate it. We have incorporated a discussion on this comparison in Line 438-440: "*The observed atmospheric warming and heating rates of the smoke aerosols are consistent with the radiative effects (5.85–21.56 W m^{-2}) and heating rates (0.37–1.71 K day^{-1}) of BC from in situ aircraft observations (Lu et al., 2020)*".

6. Figure 6 is difficult to be read, and suggested to be reorganized.

Thank you for pointing this out. We have removed the section titled "(a) CALIPSO observations of aerosol and cloud layers over the HTP region on April 12, 15, and 28, 2021" to the supplementary materials, allowing us to focus more effectively on the VFM data presented in Figure 6.

Response to Reviewer #2

We highly appreciate the insightful comments and suggestions provided by the reviewer, which have helped us enhance the quality of our manuscript. We have carefully considered each comment and implemented the suggested revisions or provided detailed clarifications where appropriate. Note that our responses are represented in blue.

Atmospheric pollutions from biomass burning contribute to climatic and cryospheric changes by influencing solar radiation and the albedos of snow and ice surfaces in the Himalayas and Tibetan Plateau (HTP). Wildfires are an important source of atmospheric pollutants in this region. This paper uses a variety of long-term satellite and ground-based data to investigate the primary effect of wildfires from the south slopes of Himalayas on aerosol loading. This can deepen our understanding of the climate effects of aerosols in the HTP region. The topic of the paper is well suited for Atmospheric Chemistry and Physics and the results are interesting. However, there are some important information is missing and several issues need to be revised. If these comments are addressed, I believe the paper should be accepted for publication.

Thank you for your insightful and valuable feedback on our manuscript. We have carefully addressed all your comments and revised the manuscript accordingly.

1. In abstract, Please add more quantitative results. The abstract should explicitly include the key finding of your manuscript. Currently, only some quantitative research results are given in lines 30-31 of the abstract.

Thank you for your suggestions and we agree. We have added more quantitative results and revised the description of the results in the abstract to improve clarity and readability.

2. lines 113-115, Different types of biomass sources have different effects on atmospheric heating on the HTP, but this article does not discuss this.

Thank you for the valuable feedback we highly appreciate it. Previous studies have generally treated the various biomass sources from South Asia as a single pollution category when examining their effects on atmospheric pollution over the HTP (Li et al., 2016; Lüthi et al., 2015). In our study, through correlation analysis and Forward HYSPLIT modeling, we show that wildfires in the Himalayas have a more significant impact on aerosol loading south of the HTP than biomass burning from the Indo-Gangetic Plain (Lines 327-336). To solve the issue the reviewer proposed, we added a short description in the manuscript at Lines 337-340: "*Various types of biomass sources could exert distinct effects on atmospheric heating over the HTP. We analyze the impacts of wildfires on the HTP during the fire season, whereas the influence of biomass burning from the IGP warrants further detailed investigation in future research*".

3. I recommended to supplement the information of observation instruments, observation elements, data efficiency, etc. of each station in Table S1.

Thank you for the suggestion. We have added information regarding the observation instruments and measured elements. Concerning data efficiency, we apologize but was unable to find relevant details in the documentation from https://doi.org/10.11888/Atmos.tpdc.272995 and https://aeronet.gsfc.nasa.gov/. However, we have addressed data quality in Part 2, Data and Methods, where the data quality assessment approach is described.

4. The analysis of Figure 2 in 3.1 is too simple. The long-term interannual variation characteristics and spatial distribution characteristics should be discussed and analyzed. In particular, the analysis of the differences in temporal and spatial distribution, such as lines 277-279, what are the reasons for such a large difference? Are there similar differences in satellite data?

Thank you for your feedback and suggestions. we appreciate your insights on the need for a more comprehensive discussion regarding the long-term interannual variation characteristics and spatial distribution of AOD at these sites.

First, the temporal variations in total AOD and fine-mode AOD can mainly be attributed to wildfire emissions from the Himalayas. This connection is supported by consistent temporal variations and significant correlations between the AOD at these sites and the active fire counts in the southern Himalayas, which we have elaborated on in the next two paragraphs (Line 284-326).

Second, for lines 275-277 "During these extreme peaks, the monthly mean AOD increased by 5 to 10 times relative to the baseline values at Nam Co and QOMS, and by up to 50 times at Pokhara site", these large differences include the temporal and spatial differences. The temporal differences stem from the factors that we have already mentioned in the previous sentence. Although the spatial differences were discussed in Lines 277-279, where we stated that "exogenous aerosol has a more significant influence on the southern slopes of the Himalayas compared to the Tibetan Plateau and the northern slopes," this variation can be attributed to the transport distance from the wildfires' origin.

Lastly, regarding satellite data, we analyzed the annual average AOD at 550 nm over the HTP region from 2011 to 2024, as shown in Figure S1, which reveals similar pronounced spatial differences. Concerning temporal variation, we aim to further investigate this over a larger spatial scale; however, the resolution of the satellite data is insufficient to fully align with the station measurements. Therefore, we did not incorporate satellite data into our analysis of the seasonal and temporal multiplicative differences in AOD in this study. Thank you for your understanding.

5. There are some formatting and spelling errors in the article, which need to be carefully revised. For example, line322.

Thank you for helping point it out. We apologize for the spelling errors and have now carefully reviewed and made a thorough correction.

6. What are the reasons for the differences in the interannual variation characteristics of the extinction coefficients of different types of aerosols in Figure 4?

Thank you for your insightful feedback. As you pointed out, there are indeed distinct differences in the interannual variation characteristics of extinction coefficients for different types of aerosols, particularly dust aerosols. Previous studies have indicated that dust is a significant aerosol type over the Tibetan Plateau, influence by dust emissions from Northwest India and Pakistan and the plateau itself (Kang et al., 2019; Liu et al., 2008). These dust aerosols are closely associated with dust events in both local and surrounding regions. The occurrence of dust events is primarily controlled by factors such as surface wind speeds and vegetation cover (Kang et al., 2016).

Consequently, the interannual variation characteristics of the extinction coefficients for different aerosol types, particularly dust and polluted dust, are likely influenced by fluctuations in meteorological factors, such as wind fields, as well as changes in vegetation cover.

In our paper, we focused on smoke aerosols and their radiative effects on the Tibetan Plateau, which explains our decision not to delve into the emission sources and their influencing factors in greater details. In response to your valuable suggestions and particularly your comment #7, we have now incorporated a more comprehensive analysis into our article. We have included detailed explanations (Lines 365–369) to elucidate the observed disparities in dust aerosols.

7. In Figure 5, the vertical distributions of aerosol extinction coefficients and occurrence frequency profiles of different aerosol types from 2021 to 2023 are quite different. For example, the occurrence frequency of dust in 2022 is significantly lower than that in 2021, but the extinction coefficient in 2022 is higher. Similarly, the occurrence frequency of polluted dust is not much different in 2022 and 2023, but the difference in extinction coefficient is quite large. Similar differences in the vertical distribution of extinction coefficients and occurrence frequencies need to be analyzed in detail in the article.

We highly appreciate this valuable comment. As detailed in our response to comment #6, we have added the analysis in Lines 365–369. "*Additionally, dust aerosols, as another major aerosol type in the HTP, are identified in Figures 4 and 5. These aerosols are mainly influenced by dust emissions originating from Northwest India and Pakistan, and the plateau itself (Kang et al., 2019; Liu et al., 2008). The temporal variations of dust aerosols are linked to dust events, which are primarily governed by complex factors including surface wind fields, vegetation cover, and the westerly jet (Kang et al., 2016).*"

8. Conclusion and Perspective: It is recommended to focus on the conclusions in this section. The conclusions need to be condensed and summarized. The conclusions are not refined enough now. I recommended to put the Perspective

in 3 Results and discussion.

Thank you for your suggestions. We now have condensed and refined the conclusions. However, we believe that it is important to retain the Perspective within the Conclusion and Perspective section. The third part primarily focuses on discussing our findings in relation to the actual data and model results, while the Perspective aims to contextualize our research's significance within the broader Earth system. This allows readers to gain a holistic understanding of our work. Therefore, we feel it is more appropriate to keep the Perspective in its current section. Thank you for your understanding.

Reference

Kang, L., Huang, J., Chen, S., and Wang, X.: Long-term trends of dust events over Tibetan Plateau during 1961–2010, Atmospheric Environment, 125, 188-198, 10.1016/j.atmosenv.2015.10.085, 2016.

Kang, S., Zhang, Q., Qian, Y., Ji, Z., Li, C., Cong, Z., Zhang, Y., Guo, J., Du, W., Huang, J., You, Q., Panday, A. K., Rupakheti, M., Chen, D., Gustafsson, Ö., Thiemens, M. H., and Qin, D.: Linking atmospheric pollution to cryospheric change in the Third Pole region: current progress and future prospects, National Science Review, 6, 796-809, 10.1093/nsr/nwz031, 2019.

Li, C., Bosch, C., Kang, S., Andersson, A., Chen, P., Zhang, Q., Cong, Z., Chen, B., Qin, D., and Gustafsson, Ö.: Sources of black carbon to the Himalayan–Tibetan Plateau glaciers, Nature Communications, 7, 12574, 10.1038/ncomms12574, 2016.

Liu, Z., Liu, D., Huang, J., Vaughan, M., Uno, I., Sugimoto, N., Kittaka, C., Trepte, C., Wang, Z., Hostetler, C., and Winker, D.: Airborne dust distributions over the Tibetan Plateau and surrounding areas derived from the first year of CALIPSO lidar observations, Atmos. Chem. Phys., 8, 5045-5060, 10.5194/acp-8-5045-2008, 2008.

Lüthi, Z. L., Škerlak, B., Kim, S. W., Lauer, A., Mues, A., Rupakheti, M., and Kang, S.: Atmospheric brown clouds reach the Tibetan Plateau by crossing the Himalayas, Atmos. Chem. Phys., 15, 6007-6021, 10.5194/acp-15-6007-2015, 2015.