Earth Syst. Sci. Data Discuss., referee comment RC1
https://doi.org/10.5194/essd-2022-272-RC1, 2022

Comment on essd-2022-272

Anonymous Referee #1

Referee comment on "The recovery and re-calibration of a 13-month aerosol extinction profiles dataset from searchlight observations from New Mexico, after the 1963 Agung eruption" by Juan-Carlos Antuña-Marrero et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-272-RC1, 2022

This study reported the recovery and re-calibration of an extensive dataset of vertical profile measurements of the 1963~1964 stratospheric aerosol layer measured from a two-site searchlight measurement facility at White Sands missile base and Sacramento Peak observatory, in New Mexico, US. This work has scientific significance for the observation of stratospheric aerosol increase due to historical volcanic eruptions. However, the authors heavily cite figures and tables from previous studies and do not have a comprehensive flow chart of this study, and the manuscript is not written with sufficient standardization, making it difficult to read. In addition, calibration or re-calibration is often done with reference to higher standards and is a bottom-up effort to seek higher accuracy. This is also the guarantee of traceability of measurement results. Only in this way can the results after calibration or re-calibration be more reliable and comparable. If more uncertainty is introduced into the re-calibration process, the results are hard to convince.

There are some detail comments as followed:

- According to the introduction, the detector response and aerosol extinction profile are given in the original literature table, why do the authors need to invert the detector response using the aerosol extinction profile and then compare the accuracy of the detector response with the original table?
- I can understand that many parameters need to be reacquired when recovering historical data, however, the use of these data in section 4.4 requires more rigorous argumentation and validation to prove that they are plausible and do not introduce too much error into the re-calibration data. The authors have not argued enough in this regard and suggest a fuller justification of the uncertainty analysis of the data used and its impact on the results, e.g., scattering phase functions, etc.
- For the aerosol extinction profile, did the authors re-digitize the figure data from the original literature in order to obtain observations for each layer and thus use Fernald's algorithm? However, it is possible that the large uncertainty of the data in the near-

surface layer caused a large error of the re-calibrated results from the literature digitization results (Fig. 6). In this regard, the authors should give other supporting information to show that the reader can trust the rescaled data.

- From the AOD assessment in Figure 7, the recalibration data only systematically increased the value of AOD, while the correction of the overall trend was more problematic. Why not use AOD as a constrain to retrieve the atmospheric column aerosol extinction when simultaneous AOD data are available?
- There are also some puzzling descriptions in the text, such as
  - Line 268-269: "…… an order of magnitude lower than the values in figure 1", but the figure 1 is the "searchlight scenes geometry".
  - Line 323: "…… in the digitization procedure (see section 2.3 above)", but section 2.3 is not included in this manuscript.
  - Line 638: "…… from one of the major volcanic eruption of the XX century.", what does this XX mean? I did not list all of these problems. Please revise them carefully.