

ACP, Pyrostrat paper Lebrun et al, review. Response to reviewer 2.

This manuscript estimates the global direct radiative forcing (RF) of stratospheric smoke injected by the 2019/2020 Australian New Year Super Outbreak (ANYSO). The authors first constrain the aerosol single scattering albedo (SSA) by reproducing the observed self-lofting rates of smoke-charged vortices (SCVs) from the 2017 Pacific Northwest Event (PNE) and ANYSO with the ARTDECO/DISORT radiative transfer model, driven by CALIOP/OMPS-LP plume tracking, ERA5 profiles, and MODIS cloud and surface albedo products. The constrained optical properties are then applied to OMPS-LP extinction profiles to compute Southern Hemisphere forcings over 2020, yielding a global top of atmosphere (TOA) RF in both all-sky and clear-sky conditions.

The paper addresses a real gap, namely the wide spread of published ANYSO forcing estimates. The demonstration that clouds reverse the sign of the TOA forcing is clearly presented, and the literature synthesis in Table 2 is a useful contribution in its own right. However, several aspects of the methodology are incompletely described and some conclusions rest on assumptions that need firmer justification. My recommendation is that the following key points, as well as the specific comments below, be addressed before publication.

We thank you for your work and review on this paper. Your comments - especially the three key points - were very helpful to complete the description of our model, its hypothesis, the uncertainties regarding the geometrical thickness of the plumes, or the choice of 2019 as a reference year to compute the radiative forcing of ANYSO over 2020.

Please find our response to your key points and comments below. The corresponding corrections were made in the manuscript, and we also added mentions to the DZ uncertainties in the conclusion, as well as how the choice of 2019 as a reference year influences the computed values of global radiative forcings for ANYSO.

1. Matching the observed heating rates cannot uniquely constrain SSA because SSA is degenerate with the assumed geometrical thickness DZ. The plume heating rate scales approximately as SAOD $\times$ (1-SSA)/DZ; with SAOD measured, the observed ascent rate constrains only this ratio, and a more absorbing aerosol in a thicker plume fits the observations identically to a brighter aerosol in a thinner one. The authors' own Figure 3 shows halving DZ nearly doubles the heating rates, comparable to the full effect of varying SSA from 0.95 to 0.83 in Figure 4. Yet the DZ values actually used in the Section 3.2 computations are never stated, nor is it explained how they were derived from the CALIOP/OMPS-LP tracking. As presented, the "best estimate" SSAs of 0.95 (PNE) and 0.90-0.95 (ANYSO), and therefore the headline forcing ranges, are conditional on an undocumented DZ. This needs to be documented and the joint SSA/DZ uncertainty at least discussed semi-quantitatively.

Thank you for pointing this out. Indeed, we primarily showed the parametric sensitivity figure illustrating how the geometrical thickness (DZ) of a plume linearly affects its heating rate, and therefore how uncertainties in DZ directly translate into uncertainties in the computed heating rates. However, the DZ values used in the final radiative computations were not documented.

The geometrical thickness of the ANY plume was derived from CALIOP L1B data as in Khaykin et al. (2020). For the PNE plume the same approach has been used for the period when CALIOP data were available and then assumed constant during the OMPS-LP plume observation period.

For the ANYSO case, CALIOP observations of the plume can be found in K2020; they are also in line with Allen2020. That is, the geometrical thickness of the plume increases fast during early January from 3km to 5km, and then oscillates between 5 and 7km in thickness during its lifetime, with deviations of ± 1 km. Using this span as a proxy for the overall uncertainty over DZ, we find the corresponding uncertainty over the heating rates as ranging from -20% to +33%. This is also coherent with Onheiser et al. 2023 which mentions a 20% uncertainty of the geometrical thickness of the plume in heating rates computations.

For the PNE case, observations by CALIOP provide a mean value of $DZ = 2.18$ km for the plume in August 2017, within a range [1.32 km, 2.98 km] and a standard deviation of 0.5km. We use this average value as constant over the lifetime of the plume, keeping in mind that CALIOP underwent technical failures in September 2017. Like for the ANYSO case, this range of values might result from the instrument's vertical resolution, the instrument sometime measuring the core or the edge of the plume, as well as the geometrical evolution of the plume itself. Considering values in the range 2.18 ± 0.5 km, the corresponding uncertainty on the computed heating rates is also -20% to +30%.

We added the description of the DZ used for the computations line 105, and added the impact of the DZ uncertainties in the heating rates discussion line 190 as well as in the conclusion.

2. The cloud representation is not described, despite clouds being central to the paper's main conclusion. Section 2.3 names the MCD06COSP product but says nothing about how monthly 1-degree cloud information enters the 1D radiative transfer: Is cloud fraction handled by weighting the fluxes of separate clear and overcast columns (independent column approximation)? What cloud optical depth and cloud top height are prescribed? Note also that a monthly product cannot represent the diurnal cycle of cloudiness, and MODIS samples only the Terra/Aqua overpass times; the resulting bias in the hourly all-sky computations should be acknowledged.

Our description of the cloud representation was indeed lacking, which is corrected in the new version of the manuscript (line 135). That is, we use the product MCD06COSP daily version (D3) when computing daily radiative fluxes for the individual SCVs (Fig. 3 top-right panel, Fig. 4, and Fig. 6 bottom panels), and we use the monthly version (M3) when computing the global radiative forcing of ANYSO monthly (Fig. 5 bottom panels and Fig. 6 top panels). This product gives us access to a 1-degree resolution grid for cloud optical thickness and effective radius (for liquid and ice cloud), as well as cloud top pressure and cloud fraction. We indeed use the Independent Column Approximation by computing separately the fluxes in clear-sky and cloudy conditions before weighting them to obtain the all-sky computations. We consider the cloud conditions given by MODIS constant over the 24 hourly computations made for each day to represent the varying SZA. The bias introduced by the fact that MODIS only averages daytime pixels (measured by both Aqua and Terra) is thought to be small in our case as the radiative effect of the studied aerosols is predominantly in the shortwave. The bias in the hourly all-sky computations introduced by the time-average of the cloud conditions was acknowledged in the heating rate discussion (line 195).

3. The forcing is defined relative to 2019 extinction profiles, with the full 2020-minus-2019 difference attributed to ANYSO and assigned smoke optical properties. The 2019 SH lower stratosphere was itself perturbed (e.g., the June 2019 Ulawun eruption), so part of the difference field is not smoke. Please discuss this attribution uncertainty, justify the choice of a single anomalous year over a multi-year background climatology, and clarify how grid cells with negative extinction differences are treated.

The choice of 2019 as a reference year to compute ANYSO's radiative forcing was made to compute the RF relative to the environment directly preceding the stratospheric outburst. It is true that in 2019 the stratosphere was itself perturbed by the Ulawun, and that choosing an multi-year mean background could have prevented the overlap of the two events in the computation of the smoke field. We believe that the magnitude and location of the Ulawun eruptions make their impact small on the computation of the radiative forcings of ANYSO :

The 2019 Ulawun eruptions in June and August increased the stratospheric aerosol burden from summer 2019 for about a year (Kloss et al. 2021, Peterson et al. 2021 Fig. 10), but mainly in the tropics [20°N-20°S] (with a peak SAOD of 0.01, Vaughan et al. 2021). It is indeed very likely that mixing occurred with ANYSO's aerosols in 2020, but mostly in the [20°S,40°S] latitude band where both anomalies are not at their peak (which is 0.02 for ANYSO). The sensitivity to Ulawun's perturbation was nevertheless tested by replacing the background profile $k_{bkg} = k_{2019}$ by the mean extinction profile of 2019 from January to June before the Ulawun perturbation. This made ANYSO's radiative forcing slightly more effective : for SSA=0.9 in all-sky (Fig. 6, top panels, solid orange lines), the annual mean TOA RF goes from 0.19 to 0.22 and the Surf RF from -0.06 to -0.065. This 10% impact of Ulawun on the RF as well as a clarification on the choice of 2019 as a background aerosol field was added in a new subsection (3.3.3) after the discussion of the global RF values, as well as mentioned in the conclusion.

Pixels where $[k_{2020}(z) - k_{2019}(z)] < 0$ were discarded in the computation of ANYSO's aerosol field, and considered only as background. Interpolating the extinction on the missing pixels was tested but as it mostly affects pixels with small extinctions it made almost no difference on the integrated optical depth for ANYSO, with a 2% difference on the mean optical depth of ANYSO' SAOD over 2020. This is now mentioned line 219.

References :

Kloss, C., Berthet, G., Sellitto, P., Ploeger, F., Taha, G., Tidiga, M., Eremenko, M., Bossolasco, A., Jégou, F., Renard, J.-B., and Legras, B.: Stratospheric aerosol layer perturbation caused by the 2019 Raikoke and Ulawun eruptions and their radiative forcing, Atmos. Chem. Phys., 21, 535–560, <https://doi.org/10.5194/acp-21-535-2021>, 2021.

Vaughan, G., Wareing, D., and Ricketts, H.: Measurement Report: Lidar measurements of stratospheric aerosol following the 2019 Raikoke and Ulawun volcanic eruptions, Atmos. Chem. Phys., 21, 5597–5604, <https://doi.org/10.5194/acp-21-5597-2021>, 2021.

Specific Comments

In 42: "OC/BC=2-3%" seems inverted; an organic-to-black-carbon ratio of 2-3% would imply nearly pure BC. Presumably BC/OC is intended. Corrected.

In 63/68: "occured" -> "occurred" Corrected.

In 87: "New-Zeland" -> "New Zealand" Corrected.

In 91: "initialy" -> "initially" Corrected.

Figure 1: How is the in-plume SAOD at 550 nm obtained? CALIOP measures at 532 nm (with a lidar ratio assumption) and OMPS-LP retrievals are at other wavelengths with coarse limb-viewing resolution. The wavelength conversion and, more importantly, the potential low bias for a compact SCV smeared over the OMPS-LP averaging volume should be discussed, since any SAOD bias maps directly onto the retrieved SSA. Also, the x-axis here uses DOY while other figures use month and day. It may be clearer to use a single convention in the manuscript.

The in-plume SAOD of the self-lofting smoke layers was derived using event-specific observational constraints and then spectrally adjusted to 550 nm. For the ANYSO event, CALIOP Level 1B V4.51 532 nm backscatter profiles (Winker 2026) were converted to extinction using a smoke lidar ratio of 85 sr, (Ohneiser et al., 2022). The resulting 532 nm extinction was vertically integrated over the plume altitude range and converted to 550 nm using the assumed smoke Ångström exponent of 1.6. For the 2017 Pacific Northwest Event (PNE), the same CALIOP-based approach was used until September 1, 2017, when CALIOP observations became temporarily unavailable. During this later period, OMPS-LP v2.0 aerosol extinction at 869 nm was used instead and converted to 550 nm using an Ångström exponent of 0.9 derived from SAGE III (multiwavelength extinction profiles sampling the self-lofting plume, Cisewski et al. (2014)), and then vertically integrated over the plume altitude range. For the OMPS-LP-based estimates, an uncertainty arises from the coarse limb-viewing sampling volume: compact smoke-charged vortices may be spatially smoothed, leading to a possible low bias in the in-plume SAOD. This was added line 85.

References :

Winker, David. (2026). *CALIPSO Lidar Level 1B profile data, V4-51* [Dataset]. NASA Langley Atmospheric Science Data Center Distributed Active Archive Center.
https://doi.org/10.5067/CALIOP/CALIPSO/CAL_LID_L1-STANDARD-V4-51

Cisewski, Michael, et al. "The stratospheric aerosol and gas experiment (SAGE III) on the International Space Station (ISS) Mission." *Sensors, Systems, and Next-Generation Satellites XVIII*. Vol. 9241. SPIE, 2014.

For the x-axis of Fig. 1, corrections were made on the figure.

In 103: The refractive index is assumed spectrally constant, but brown carbon absorption increases sharply toward blue/UV wavelengths. Some comment on the sign and rough magnitude of the resulting bias in SW absorption is warranted.

Due to the limited information available on the complex refractive index of stratospheric aerosols and its spectral dependence, we assume spectrally invariant refractive indices in the radiative effect and heating rate calculations, over the shortwave (0.2–4 μm) and longwave (4–500 μm) spectral ranges. The SSA and asymmetry parameter are therefore calculated, prior to the radiative transfer computations, under these assumptions using a Mie scattering routine. Although spectrally invariant refractive indices are assumed and the SSA and asymmetry parameter in Table 1 are reported only at 550 nm, the values used in the heating rate calculations do exhibit spectral dependence. This is because the spectral dependence of both the SSA and asymmetry parameter is determined not only by the refractive index but also by the ratio of wavelength to particle size (i.e., the size parameter). This was added line 123. Consequently, spectral variations in these optical properties arise even when the complex refractive index is assumed to be spectrally invariant. The spectral variations of the SSA computed with the chosen refractive index do exhibit sharp increase of absorption toward blue/UV, and more generally show very similar trends of absorption as that of pure brown carbon. It should be noted that neglecting the spectral dependence of the refractive index still introduces a source of uncertainty. However, BrC absorption is strongest at short wavelengths, generally below approximately 0.4 μm , and the 0.2–0.4 μm range accounts for only about 8% of the total solar energy considered here (0.2–4 μm). Therefore, the impact of neglecting the spectral dependence of the refractive index on the integrated shortwave radiative effect and heating rate is expected to be limited.

In 105: "dependant" -> "dependent" Corrected.

Table 1 vs In 101: The text states $\text{Im}(\text{refind}) = [0.01, 0.04]$ is used for both cases, but Table 1 lists 0.01-0.03 for ANYSO, while Figure 4 shows ANYSO curves up to $\text{Im}(\text{refind}) = 0.04$. Please reconcile. Corrected.

Table 1: "Assymetry" -> "Asymmetry" Corrected.

In 121: "where dT/dz is the net radiative heating rate" — I think this should read dF/dz (the flux divergence). Please also state the sign convention for F. Corrected.

In 132: The base case $DZ = 10$ km appears thick relative to reported SCV depths of a few km (e.g., Khaykin et al. 2020). Please justify, and state the DZ used for each plume in Section 3.2 (see key point 1).

For ANYSO's SCV geometrical thickness, minimal and maximal observed values are 3km and 7km (see answer to key point 1.), so values of 5 and 10km were chosen on the sensitivity plot to emphasize the factor 2 impact on the heating rates, though 3/6 km or 4/8km could have been chosen too for the same result.

Figure 4: The observed ANYSO heating rate is drawn as a constant 5 K/day, but the theta trajectory in Figure 1 is not obviously linear early on. Similarly, how was the PNE "observed" curve in this figure derived? Was this a fit, and over what period? To my eye, the ANYSO temperature curve in Figure 1 is actually stepper in the first month, which could potentially better agree with the trend of the corresponding computed heating rates.

In this first version the heating rates were computed in two ways : with an exponential fit for PNE and with a single linear fit for ANYSO. Thanks to your comment and to show the sensitivity to the way the potential temperature was derived into the “observed” heating rate, we chose to also add for both cases the Savitzky-Golay differentiation method (Savitzky and Golay 1964). We used it with a window of 30 days to filter out high-frequencies in the derivation (different values for the window were tested from 10 to 40 days, with small impacts on the order of magnitude of the heating rates, but a window too small added a lot of noise). These new observed heating rates were added in Fig. 4 as “Ref”, with an explanation of how they were computed line 176. We also changed the ANYSO single linear fit to a monthly piecewise linear derivative. Like you suggested, the trend of the newly computed heating rates for the ANYSO case show higher values in the beginning of January than a simple linear fit. For the piecewise heating rates, the January value is 7.2 K/day ($R^2=0.99$), and for February 4.6 K/day ($R^2=0.98$) – which are comparable to the 8 K/day and 6 K/day of Allen2020.

This did not modify the SSA values we find the best for the ANYSO case : 0.95 fits better the Savitzky-Golay derivative (with a RMSE of 1.4 K/day versus 4.5 K/day for SSA =0.90), while SSA=0.90 fits better the piecewise function (RMSE of 1.9K/day versus 2.6K/day for SSA=0.95). This explanation was added in the paper line 183. For the PNE case, the choice of SSA = 0.95 stays the best for both derivation methods of the heating rate.

References :

Savitzky, Abraham, and Marcel JE Golay. "Smoothing and differentiation of data by simplified least squares procedures." *Analytical chemistry* 36.8 (1964): 1627-1639.

In 160: Aerosol growth is offered as the sole explanation for the trend discrepancy. In addition to my prior comment, a discussion on other plausible explanations (e.g., evolution of DZ, chemistry that could impact composition/RI, etc.) should be added.

Indeed these other explanations were lacking and were added line 192.

In 175: Please state which OMPS-LP wavelength underlies the 550 nm extinction and how "stratospheric" is defined (tropopause product?).

The stratospheric aerosol extinction profiles were obtained from the OMPS Limb Profiler (OMPS-LP; Jaross et al., 2014) Version 2.0 aerosol extinction product (Taha et al., 2021). We used the 869 nm extinction coefficient and converted it to 550 nm using an Ångström exponent of 2 (from SAGE III multiwavelength extinction profiles). The stratospheric column was defined relative to the thermal tropopause supplied with the OMPS-LP product, and the integration was performed from 1 km above the tropopause upward, in order to reduce possible contamination from upper-tropospheric aerosols and clouds. This was added line 205.

Figure 5: Please note the cause of the white regions (polar night?) Added in the figure caption.

ln 185: Given the background is fixed to 2019, how are locations/months where 2020 extinction falls below 2019 handled (negative k_ANYSO)? See answer to key point 3.

ln 193: "local SH annual mean" is confusing terminology for a hemispheric average; consider rewording. Corrected.

ln 211: "solild" -> "solid" Corrected.

ln 229/231: For the local SCV forcing, over what area is "local" defined (plume core, tracked extent)? The area of the SCV is considered to be constant at 10^6 km^2 (Khaykin et al. 2020), which was added in the text line 263.

ln 250: "a values" -> "values" Corrected.

Table 2: Please double check the TOA ERF value in the last row corresponding to Yu et al. (2023).

Their TOA ERF is $-37 \text{ W/m}^2/\text{sAOD}$ (the unit was missing, which was corrected).

ln 275: "the the" -> "the" Corrected.

References: Ohneiser et al. (2020a) and (2020b) appear to be the same paper listed twice, and the 2020b entry contains a " " artifact. Corrected.