Principal criteria

Scientific significance:

The introduction of a new measurement system undoubtedly contributes to scientific progress in the field of atmospheric measurement technology. In particular, a new system for the continuous observation of variable aerosol parameters in the stratosphere, a region considered sensitive to such fluctuations and where such fluctuations can influence climate variability.

Scientific quality:

In some respects, the scientific quality is high, as the authors have access to an impressive series of measurement data at various locations over a long period of time. In the sections dealing with technical implementation and calibrations, the discussion seems to be limited to the bare essentials. As a result, specific details regarding the implementation of modifications, adjustments, and calibrations, which should have been focused for an AMT article, are unfortunately missing in some cases.

Presentation quality:

Given the way it is presented, I have my doubts that this article, in its current form, should already be considered ready for submission. A few specific comments (below) may indicate what I found objectionable.

Access review (quick report), peer review, and interactive public discussion

The article titled
"A New Optical Particle Counter with a Profiling Function for the Study of Stratospheric Aerosols"
by Jean-Paul Vernier, Nicolas Dumelié, Amit Kumar Pandit, Gwenael Berthet, Lilian Joly, Giovanni de Souza, Eduardo Landulfo, Demilson Quintao, Bruno Biazon, Ravi Kiran, Venkat Ratnam, James Flaten, Rubel Das, David Paraiseau, Frank G. Wienhold, and Yaowei Li
discusses the modification, calibration, and measurements of a commercially available instrument designed for size-resolved scattered light detection of aerosol particles, which they converted into a detector for use in balloon-borne vertical soundings extending into the stratosphere, up to ~ 30 km. As the authors rightly note, this is not an entirely new approach. Nevertheless, the selection of available measurement systems for comparable applications remains expandable.

Judging by the headlines, the article does indeed promise a well-structured introduction to the topic. The authors begin with an overview of the importance of stratospheric aerosols, followed by a selection of various light-weighted OPCs (where, in my opinion, further details would be appropriate), and finally move on to the main topic: the presentation of their instrument development. Although the authors claim L140 "We will discuss the steps of making such a sensor qualified for atmospheric/stratospheric applications.", the reader is provided with a section on the device's technical specifications rather than an expected guide on how the device was converted into the balloon-operated version. In this part of the article the authors also start distinguishing between an older POPC-06 version, and a newer POPC-30 version. It is mentioned several times that the older POPC-06 was replaced (in a certain year) due to the quality and superior performance of the newer POPC-30. This raises the question why parts of the article focus solely on measurements of the POPC-06 in later sections, which then seems somewhat unbalanced. The authors then move directly to the measurement (Mie) theory underlying particle detection using OPCs; some clarifications are required at this point. This section is followed by an introduction to the calibration and testing procedures, although the reference device is referred to by the generic term "SMPS" without further specification. The results are presented, but a schematic representation of the calibration setup, e.g. by a flowchart (at least in the

supplement) would have completed this part. This is followed by a description of the balloon applications of the OPC system, initial comparisons of counting events, investigations into volume flow control, and an error analysis that points out necessary corrections. However, the text that follows provides no further indication of how these corrections were actually applied to the specific measurement results presented. The authors then jump back and forth between measurement comparisons (POPC-06 versus POPC-30, as well as POPC-06 and POPC-30 versus POPS), followed by a digression on size corrections, the calculation of aerosol parameters (such as aerosol size distribution; by-the-way an inter-instrumental comparison (e.g., POPC vs POPS) of resulting number size distribution is not shown at all in the article); the authors then consult Mie calculations to determine other aerosol parameters. This latter part (entire Section 3) of the article should be restructured more clearly along a narrative thread.

This is followed by a comparison of the POPC systems with a whole series of other detectors. This is where the greatest value of this article comes into play: the sheer volume of available data is impressive. The number of data points included here, collected at various launch sites under a wide variety of conditions, is extremely large. In my view, this reflects a bit the authors' desire to include the large volume of obtained data in this article. This raises the question, at least for me, of why the comprehensive dataset of POPC-06 measurements was included if this system (at least as I understand it) is no longer to be used in the future. Why was the extensive presentation of the POPC-30 measurements also included (three missions in France, two in Brazil, two in the USA, and two missions in India; Figs. 14 & 15), rather than focusing on just a few cases, e.g., just one volcanic event, one pyroCb example, and one background measurement? To me, the big picture is clearly lost here, and one wonders whether this results in an overrepresentation of pure observations. On the one hand, this overrepresentation dilutes the technical nature of the paper. On the other hand, the scientific discussion of the undoubtedly impressive variety of data does not go into sufficient depth (neither in a scientific context nor in terms of technical limitations or performance). In this regard, I would recommend a clearer selection of measurements, focusing on those that serve to illustrate the instrument's performance. All other measurement data should be discussed appropriately within the context of atmospheric science rather than being sidelined within the context of instrument technology.

Aside from aforementioned concerns, I had some issues with the presentation in this article. Overall, the authors should work on how they present their figures. Some of the graphics are displayed in low resolution; I find some of the captions, axis labels, legends to be rather colloquial and not comprehensively informative; furthermore, it is inconsistent to use a dimensioned axis for a dimensionless ratio (Fig 7 and Fig 8). Overall, the article leaves much to be desired in terms of consistency (variable labels, formula notation, date format, units, etc.).

Please find in the following a few comments that may refer specifically to examples related to the general criticism I have expressed.

Specific comments

L23. The authors write that "the sources of stratospheric aerosols have become more complex", which I find a bit unclear: (1) pollution from Asia: Why should this contribute more to the complexity than pollution from other regions? (2) Isn't the problem of rocket and satellite debris largely one that has increased (in terms of volume) since the year 1957 (Sputnik 1)? Exactly how does these contribute to the complexity of the sources? Please, specify in more detail.

L32. "...aerosol optical diameter d between 0.3...." I suggest introducing the variable "d" (later in the text D_p is used) right after "diameter" at this point as in line 33 the "d" suddenly appears without introduction.

L33. In my pdf version, I see a "." right after the unit µm.

L34. Please clearly indicate if "correlates near 0.97" is meant by r or r^2.

L37. This is where a series of volcanic eruptions comes into play. The new system is apparently capable of detecting them. In my opinion, it would make sense to include this type of sources of stratospheric aerosols also in the statement above (L23).

L39. Is it expected that all potential readers of this article will be familiar with the abbreviation "pyroCb"? By the way, here's another source of stratospheric aerosols (see L23). One might speak of the diversity of sources rather than their complexity (if this is meant) though it could be that the potential interactions among stratospheric trace substances from the various sources have become increasingly complex...

L52. Do heterogeneous reactions exclusively lead to the conversion of gas species? Or in other words, the process can act as a sink for some species and a source for others, right?

L55-59. Because of this piece of text, the entire Section 1.1 appears a bit unbalanced as it is. Radiation effects and interactions, as well as heterogeneous reactions and aerosol's role as condensation nuclei, are briefly touched. One-third of Section 1.1 is devoted to health effects. The main title of the article points to vertical profiles and stratospheric aerosols, the abstract mentions measurements to be taken with weather balloons. Therefore, as far as a ground-based monitoring instrument was concerned, I would not question the significance of this point. With balloon measurements at an ascent rate of 3-7 m/s at launch, a quite limited number of data points will be collected from the near-surface region, while far more observation time is spent in the free atmosphere, the tropopause region, and in the stratosphere. Monitoring of pollution in the boundary layer does not appear to be primary focus of the instrument this article is about. However, if the authors wish to retain this section, some balancing is required - then, in my opinion, the general role of atmospheric aerosols should be reported in broader detail (their impact on cloud formation and precipitation, their role as nutrient carriers, their atmospheric sources and sinks, their average lifetime, etc.).

L62-64. This section is titled "Stratospheric Aerosols," but it begins with a sentence about processes that occur "especially in the troposphere". This would better fit into previous paragraph.

L64. Please indicate (e.g. by a factor) how much dryer is the stratosphere compared to the troposphere. The dryness may be the exclusive point controlling the aerosols' lifetime – What about the extremely stable, stratiform layering and the reduced vertical dynamics,...?

L69. Since the Brewer-Dobson circulation is a phenomenon that occurs primarily in the middle atmosphere (with effects on the stratosphere in the tropics and polar regions), wouldn't it be more consistent to refer to "the aerosols' presence in the stratosphere"?

L70: More specifically, I would write "Stratospheric sulfate aerosols also participate...."

L71. To make the location change clearer, a phrase such as "in the tropopause region" should be added here in connection with the effects on cirrus clouds formation. I'm not entirely sure what the phrase "from the transport" means - does it mean "as a result of the transport" or "due to the transport"? please clarify.

L75. Is this really a new source? Isn't the occurrence of pyroCb phenomena actually older (though the frequence of occurrence may have increased) and isn't the discovery of its impact

on stratospheric (aerosol) composition, instead, a new insight into a source that isn't actually that new?

L77. To fully represent the acronym UTLS, “Lower” would need to be inserted before “Stratosphere.”

L78. Suggest rephrasing: “The two most impactful events…” or “The two events with greatest impact…”

L82. A blank space should be added between the bracket and “where”.

L80-84. In my opinion, this sentence should be divided into two parts, since the first part describes a transport pathway through the ASM into the UTLS. The enriched aerosol layer (which acts as a reservoir), on the other hand, is located specifically in the tropopause region. One could add “….observed (Vernier et al. 2011) and further characterized by airborne observations (Höpfner et al.,2019 and Mahnke et al., 2021)”

Höpfner et al. https://doi.org/10.1038/s41561-019-0385-8 , 2019

L99. One could add the UK (Girdwood et al. 2022 and Girdwood 2023), and Renard et al. 2016 is updated by Renard et al. 2026.

Girdwood, J.: https://doi.org/10.18745/th.27277, 2023.

Girdwood, J., Stanley, W., Stopford, C., and Brus, D.: https://doi.org/10.5194/amt-15-2061-2022, 2022.

Renard et al. https://doi.org/10.3390/s26123786 , 2026

L101-102. or use an ascent-driven passive flow, as e.g. the UCASS (Girdwood et al. 2022 and Girdwood 2023) → therefore Table 1 could be updated.

L111-118. Consider updating this paragraph by new findings from Renard et al. 2026 (https://doi.org/10.3390/s26123786)

L121.& L126 The text refers to “Kalnajs & Deshler, 2022”, the reference list says “Kalnajs & Deshler, 2022a”

L123. What specifically means “against larger Wyoming OPCs“ - “larger” is probably not a robust criterion for characterizing suitability as a reference, is it? Which type of reference OPC was used here? Is this described in the cited literature Kalnajs & Deshler, 2022?

L127. Previously, the term “OPCs” has been used in the plural—here, as well, the plural form seems appropriate.

L128. Is probably the following meant? : there is a risk of incurring a financial loss in the amount mentioned if the payload is not recoverable.

L128: I can't comment on LOAC, but at least as far as POPS is concerned, it's worth noting that the cost of the instruments is about four to five times higher than $3k when purchased through official distribution channels. Of course, “above $3,000” still holds but is still an understatement in the case of POPS.

L103-137. One might consider mentioning UCASS in a small paragraph and briefly presenting its technical specifications (Girdwood et al. and Girdwood,see above), with its application (Kezoudi et al https://doi.org/10.5194/acp-21-6781-2021, 2021) and including the improvements

introduced by Jost et al. (https://doi.org/10.5194/amt-18-4397-2025) 2025. Then Table 1 could be expanded by the UCASS with its specs and the two Girdwood references. Maybe LOAC-II finds a place as well (cf. Renard et al. https://doi.org/10.3390/s26123786 , 2026)

L145. Shouldn't it say "…those aerosols' moments…" ?

Fig 1.: One might consider placing a familiar coin (or similar) or a length scale next to the objects in panels a and b to better illustrate the objects' size.

L157. This sentence is not clear to me: I guess it should say either "The POPC follows the generic principle of particle counting via a diode laser directed into an optical chamber." or "The POPC follows the generic principle of particle counting via a diode laser within an optical chamber."

L158. I suggest restructuring this sentence: e.g. into: "Near (About) 2.83 lpm of air enter an inlet and the flow is compressed which forms to a thin high-velocity stream containing the particles to be detected." However, one could debate whether the word "near" (or about) is appropriate for a quite precise specification of "2.83 lpm". I mean, given this specific flow rate I would say its nearly 3 lpm.

L159. Can one really say with such certainty that the particles arrive "one at a time"? Wouldn't "in sequence" be more appropriate?

L164-165. How is the amplitude of the voltage pulse indicative for the particle concentration? (this should be the number of pulses per time or air volume). Which kind of composition is meant? Knowledge of particles' chemical composition is required for the pulse interpretation (complex part of the refractive index) but a chemical composition is not retrievable from the pulse amplitude. This sentence needs to be revised.

L166. What are "those different bins" as before bins were not mentioned?

L167. In which metric is this "channel threshold" defined? Which measure/unit?

L169. It has not yet been explained how the volume flow rate is actually measured. Please clarify this here or refer to the relevant section in this article.

L179. Blank space is missing between "theory" and bracket.

L178-180. This is a bit to general: OPCs do not measure the scattering efficiency, instead the scattered intensity is measured that has been collected within a specific solid angle. Mie theory provides the complete angular distribution of intensity not just the total scattering efficiency. The angle dependence of Mie scattering (differential scattering cross section) is missing, particularly for given range 58-108°.

L184 The scattering cross-sectional area is not an actual area over which light is scattered. So, instead of the statement within brackets (which could be erased) the term "effective" could be placed before "scattering cross-sectional area".

L188-189. This relationship (Equ 3) approximately describes the total scattering power. However, an OPC does not measure the total scattering power. It merely measures a portion of it. In fact, the detected intensity should be the integral over the differential scattering cross section within the detection angle.

Fig 2 (a) the x-axis label should be more specific (e.g. "particle size" or even better "particle diameter"). Is the vertical displacement of the lines for "High gain" or "Low gain" of importance?

(b) x-axis label shows a variable "D_p" that is not introduced yet. In the abstract "d" was used for diameter. One term, introduced once and used consistently, would be helpful.

I personally prefer placing the unit after the variable without brackets but separated only by a comma or a "/" → e.g. "particle diameter / µm". Regardless of whether the authors wish to maintain their own style, it should be consistent throughout the article—in terms of font style and font size used for axis scales and labels; the way units are indicated in parentheses should not alternate between () and [] or between "size" and "D_p."

Fig 2 (a) Why is the x-axis scale limited to 9.25 µm, even though data for a larger size is displayed? I could understand it if the data point for the largest "size" were explicitly shown on the scale and 9.0 µm were displayed instead of 9.25 µm.

Fig2 (b) Why does the calibration data point at 10 µm appear to be cut in half?

L204. Please explain the term "threshold" in more detail in this context. I suggest this to clarify already in the section around L164. It's unclear as the singular form is used here, whereas there should be multiple thresholds that a POPC has, if I haven't misunderstood.

Another question: If the upper and lower size limits of each of the 30 POPC channel are known, why isn't each of the data points shown accompanied by a horizontal bar that extends exactly to those limits?

L210. Please replace "2-micron" by "2 µm". In addition, I would use the term "diameter" (here and elsewhere) if that is what is meant (not even just "size"), especially since "radius" is also used in connection with Mie theory.

L213. It seems as if one or more words are missing; otherwise, I don't understand what "sulfat PSL" means.

Fig 3: Why do the x-scales extend beyond 20 µm, while the scale ranges in Fig. 2 are so strictly limited to 10 µm? What additional information is provided in this way?

L222-224. The sentence seems incoherent and incomprehensible to me - maybe it's incomplete?

L228. Is there a specific brand/manufacturer/distributor of PSL that would be recommended by the authors?

L229. Shouldn't it be the USA's "National Institute of Standards and Technology" – NIST?

L230. Not clear what "digital cut point" means, particularly not in this context, therefore the sentence is unclear.

L233. SMPS – please specify the manufacturer/model

L233 there should be a unit after "10"

L233. How is the charge equilibrium (Boltzmann charge distribution) of the particles (which may be important for high-voltage selection with the DMA) generated in the field? What about the comparability between the electromobility-diameter and the optical-scattering-diameter?

L231-245 I would suggest completely restructuring this paragraph, clearly identifying the sentence components (subject and object), and choosing clearer wording. Some examples: What is the authors' definition of "false count rate"? what means "60 sample time"?, "10 units" of what? "from the mean" of what? "differences for 2398" what does the number mean?

L237. “Ten units…” could be a better start of a sentence.

L244 “In the section” which one? “this percent difference”: I guess 20 % is meant? Am I correct in understanding that POPCs with a deviation of less than 20% from the average concentration (for all calibration particle diameters) are considered suitable?

Fig 4, panel a) y-axis label is not clear (e.g. “particle number concentration, cm^{-3}”) , x-axis label does not exist. The image title with “@” appears too informal (all the panel titles could be eliminated for the sake of clarity as soon as all axis labels are improved in clarity), the legend should be clearer: if these numbers are “serial numbers” of POPC then I would indicate it here? A grid within the graph would provide an upgrade. Panel b: that the x-axis represents the POPC-SN should be indicated. As soon as the y-axis label says “percentage deviation, %” or similar the headline could shrink to the important info “D_p of calibration aerosol: 300nm” or similar. I assume that the calibrations refer to PSL as calibration aerosol, this should be mentioned in the Figure and/or its caption.

Did I get correctly that panel b is the result of the time series shown in panel a ? If so it may be indicated? Is there a rational why only the 300nm time series is shown? Apart from that the time format is not specified (thus not unambiguously clear whether the series runs over six hours or six days) a few words about temporal resolution of the shown measurement series (e.g. 1Hz) are missing or whether the time series were anyhow treated, e.g. 1 Minute averaging or a running-average over several seconds, or similar.

Further questions that arise from Fig 4 and associated text: Why is the time series shown only for 300 nm (PSL?) and not for the other diameter? What other insights can be gained from the data presented in the box-and-whisker plots? Which POPCs are considered good and which are not? How should the outliers, which vary from device to device, and the scatter be interpreted? The caption indicates measurements on “3/26/26”, the authors may consider writing the month in clear text, e.g. “Mar” and the full year, i.e. 2026, for the sake of clarity. Moreover, the caption says “lasted for 4hours”: if my guess is correct the last two digits in the x-axis scale of panel a is a day time hour. If so the entire time series runs over almost six hours, not four – please clarify. How many data points (measurement minutes) are included in each of the box&whiskers of panel b) c) and d)?

L260. Why is the newer version preferred? Only the specifications for the new version are listed, there is no mention of a previous version that would explain the reasons for this preference.

L268-270. If I understand this correctly, the SMPS isn't taken out into the field to perform the calibrations; instead, a POPC that was previously calibrated against the SMPS now serves as the reference, right? How do you ensure that the reference POPC isn't also affected by optical misalignments and dirt contamination on the mirror and/or the detector? In any case, instead of a generalized “reference instrument,” at this point it should specifically say “reference POPC” if such is the case.

L274. Which one is supposed to be the reference and which one the flight unit?

Fig 5. I suggest revising the graphic and the captions: (1) make the headings clearer or omit them entirely (here, they read like entries in a lab notebook), (2) x-axis label is not clear, what means “datetime” without further format info (e.g. “hh:mm:ss”), in particular if another scale is chosen than in previously shown time series. (3) legends should be clearer: why does the legend say “POP1” and the caption says “POPC1” - why is the presentation of SN or reference numbers not consistent throughout the article? (4) x-axis label of panel b: why “difference”? Difference from

what? Previously this was given as percentage deviation from a mean value. (5) panel b: the zero deviation should be centred in the graph (scale the axis from -11 to +11%) (6) caption: I guess, it is only one “comparison” (instead plural). I suggest “between 0.5 Hz counts of two…”, make the display of serial or reference numbers consistent. Is “The mean difference was -3.0%, with a standard deviation of 3.0%.” meant? Somewhere in the text it should be defined that

$$\frac{C_{\mathrm{POPC}} - C_{\mathrm{Ref}}}{C_{\mathrm{Ref}}} \cdot 100\%$$ (or similar, with *C* = counts or concentration)

is what you describe as deviation or difference (either of which, chose one and stay consistent) – name it, e.g., δ in %… et voila! Clearly state, which one is the reference POPC and which one is the POPC to be tested.

L296. Likely, a “differential pressure flow meter” is meant – which type/manufacturer, specifications (accuracy, precision)?

L298. I suggest “thermal vacuum test” is meant (instead of “VACcum”). TVAC tests (commonly used in the aerospace industry) combines two stress conditions (simultaneously):

1. vacuum: a) exposure of equipment to the space/flight environment, b) testing for outgassing, leak tightness, material behaviour, and thermal conditions
2. temperature cycles a) repeated heating and cooling, b) testing of: thermal expansion of various materials, solder joints and connections, electronic drift, mechanical stresses

Was the POPC actually subjected to a full standard protocol for testing space equipment? Or did the POPC undergo a procedure similar to fundamental principles of a TVAC-test, which then is just called TVAC-test?

L298. It says here that the POPC-06 was tested. Did that also apply to the POPC-30 (which is, after all, a new version of the instrument and therefore requires a new technical sample inspection)?

L299. I guess there are several mass flow meters available from this manufacturer, please provide model/type and specs.

L301. Please clarify what “interpolated date from a TVAC test” means. To understand and judge the graphic (Fig 6) and the conclusions drawn in Section 3.2, the calculation methodology and the variables taken into account should be clearly presented,

L303-304. The problem illustrated here: Expected/calibrated flow rate (based on in-flight measured Δ*p*) compared to the actual flow rate (Δ*p* -based) measured during flight, right? Then "mass flow meter suggests a flow of 2 lpm" is misleading. How were the various normalisations performed? Were the “TVAC” calibrations normalized to STP (or NTP), or were the in-flight measurements normalised to the laboratory (calibration) conditions? There is no specific information that clearly indicates whether all variables here refer to the same standard, even though one would generally assume so. “50% higher” is misleading. Either the expected/calibrated flow rate is by 100% higher than the actual flow rate, or the actual flow rate is 50 % smaller than the flow rate expected/calibrated.

L305. Please define/introduce, what exactly is *Q* and Δ*Q* - then Δ*Q/Q* is just a relative deviation why is that already being referred to as an error?

Fig 6. Wouldn't it make sense to plot not only how flow depends on altitude, but also (in separate graphs) how it depends on pressure and ambient temperature? After all, there is an expected behaviour, and any deviation from it would provide a little more insight. The legend could be clearer.

L308. “reconstructed and interpolated” – Please, specify in more detail what (which variable) was treated and how.

L310. This line is too thin and easy to overlook. It should be thicker (and maybe in a colour that makes it stick out from the grid lines).

L314-326 Please distinguish strictly between "counting efficiency correction" that is caused by the counting efficiency η and the "uncertainty of counting efficiency" $\Delta\eta$. The error $\Delta\eta/\eta$ comes from the uncertainty $\Delta\eta$ only.

The counting efficiency of an OPC is $\eta(D_p)$ rather than η.

L318. The authors wrote, " assuming their independence”. This should be discussed, particularly with regard to the potential dependency between <flow ←→ particle counting> or <counting efficiency ←→ particle size>

L320. $\Delta Q/Q$ may be a bias rather than an error (as the text says).

L324. Please replace “poison” by “Poisson”

L314-326 what about considering statistical errors as a) coincidence b) dead time c) electronic thresholds, d) background counts

L327. Please clarify whether this really refers to an “error” or rather to “uncertainty.”

L328. “counts near 100” appears incomplete – what is meant, 100 counts per second? per measurement interval?

L334. L434. The article “J.-P. Vernier et al., 2028” is probably referred to as “Vernier et al. 2018” in the references.

L336-337. I would recommend a clearer formulation, because the goal of this comparison should certainly be to identify the differences in the measurement results, not the differences between the instruments. Afterward, one would try to understand what might be causing these differences.

Fig 7. The graphic is provided in a quite poor resolution. Y-axis label: please write “altitude, km” instead of “Alt(km)”. Legends: why “new” and “old” while the text includes specific instrument name as POPC-06 and POPC-30? Headlines: cryptic – and, compared to the previous examples, show even another, new date format: here and elsewhere decide on a clear date format that is used consistently. “New/Old”: Instead of using the logarithmic scale (of the particle number concentration) for this ratio, which, after all, contains essential information of intended comparison, I recommend using a second x-axis (e.g. at the top of each graph) that shows the ratio on a linear scale. In addition, since the quotient “New/Old” of two concentrations has no units, it doesn’t make sense to use the lower x-axis (“concentration [/cm#3]”? - which should be corrected as well into, e.g. “particle number concentration, cm^{-3}”) for this anyway.

Why is the x-axis scale of panel c limited to 10^{-2} cm^{-3} while the measurements seem to show smaller values as well? Why is the scale of panel a and b limited to ~25 cm^{-3} while the measurements seem to show also larger values?

Table2. “ID” seems to be a good term to use for these POPC numbers - maybe this will be incorporated into previous charts, tables, and texts for clarity. Again, another date format....

I propose to use a format in which the day date (e.g., a two-digit number) is clearly distinguished from the month (text, e.g., Jan, Feb, Mar, ...) and the year (four-digit number) in a preferred order but which is used consistently throughout the entire article, in all figures, and in all tables.

L344 & 346. Clearly indicate Latitude and Longitude or indicate North/South, East/West directions.

L345. The article “Vernier et al., 2026” is not listed in the references.

L348-349. Isn’t there a feature also at about 24 km altitude?

L350-367. I would suggest revising the text to replace various, somewhat colloquial formulations with clear specifications, descriptions, and explanations, examples are:

> Systems depict atmospheric layers → measured concentrations reflect the presence of atmospheric aerosol stratification
>
> some general behaviours → who behaves? What is meant – different performance?
>
> POPC-30 is higher than POPC-06 → resulting/measured concentrations of one are higher than those of the other
>
> effect is especially pronounced → Is this differing result really an effect, or rather a phenomenon/observation?
>
> for most profile → what is meant? A range of altitude?
>
> particles associated with d>0.3 µm → particles of diameter d > 0.3µm (or maybe D_p instead of d)
>
> the US flight → the flight with the ID DCOTSS2-F#1 launched in the USA
>
> after a pyroCb → this reads as if the pyroCb is the event, but it is a type of cloud, so “after a pyroCb *event* in New Mexico” or “*under influence of* a pyroCb in New Mexico”

L357. “which could reflect difference in aerosol composition.” - This stands out a bit on its own here and, in my opinion, requires a little more explanation than just this brief comment.

L354 & 358 & 364 The authors may specify which altitude ranges they are referring to so that readers can better follow this discussion in Figure 7.

L366. This section mentions the possible effects of Hunga on the measurements several times. Is there more conclusive evidence that the observations coincide with the passage through the (aged) plume of a Hunga (e.g., particle samples—and their chemical analysis)?

368-370. From the generally higher measured values for d > 0.3 µm the authors conclude that the new POPC is an improvement compared to the old one in this measurement range. The lower concentrations measured by the newer POPC-60 for d > 0.5 µm (panel c: ~5-10km and > 18km; panel d: largly above ~8 km), however, are not assessed.

Fig 8. Most of the comments from Fig 7 I would repeat here. The most problematic aspect still is the use of a unit-based, logarithmic scale (particle number concentration, cm^{-3}) for a dimensionless ratio C_{POPC}/C_{POPS}, whereas a comparison on a linear scale would provide much clearer insights. Why is in panel b the POPS d>0.3 profile shown as a dashed-line plot while in panel a as a simple line-plot? The legends within the two panels may also distinguish between POPC-06 (panel a) and POPC-30 (panel b).

L378. Should read as “POPC-06 -measured concentration” or “concentration measured by POPC-06”

L378-379. Please provide more specific hights or hight ranges rather than just separating atmospheric regions.

L381. what means flow restrictions – e.g. a blocked inlet line? How might one explain that the situation apparently returns to "normal" at altitudes of about >18 km and above?

L385. please provide a reference to panel b of Fig 8.

L384. Why not POPC-30 if this is always meant as the new POPC?

Supplement, p2, please change at the very end of the paragraph as follows "....as reported by Li et al. (2023)". One may mention the meaning of the horizontal dashed line at 17 km explicitly within the caption. Why the change of units from particles per cubic centimetre to particles per litre? It is also not explicitly mentioned if ambient concentrations or standardized (e.g. STP) concentrations are displayed.

L385. Consider referring to panel b of Fig. 8 right after the date.

L388-389. How does a check used to avoid calibration drifts? Either the check was used to identify calibration drifts, or the check was meant to avoid that instruments with unrecognized drifts are used for profiling. If there is a paragraph in this article that describes these check procedures, here would be a good place to include a reference to it.

L389-390 By which means is this improvement reflected? Which improvement, the one resulting from the introduction of the drift check? Does "latest comparison" refer to the flight on 10/12/2022? Please specify clearly.

L397. Why is "refractive index" capitalized

L398. Further indication that the article is actually presented at a pre-submission stage can be seen here in the presentation of the percentage proportions of sulfuric acid and water.

L399-403. Please, specifically indicate which scattering cross section is derived? Total scattering? Instrumental scattering? Scattering integrated over the detector aperture? Calibrated pulse amplitude? OPCs generally do not directly measure the physical scattering cross section, but rather an electrical pulse amplitude that is related to the scattering signal only through the instrument's optics.

L403. The article "Hagen et al. (2022)" is not listed in the references.

L404. Please specify the criteria used to select these cases.

L405-406. Please specify the type of correction, e.g., the Mie correction, or any other/further corrections, if applicable.

L409. Please refer to corresponding section or paragraph.

Fig. 9: I would recommend revising this figure by, for example, removing cryptic and partially covered headings, using a consistent axis scale (whether the notation is scientific, engineering, or simply base-exponent) throughout the entire article, and ensuring that parts of the data or shown results are not hidden behind legends or labels. In the caption: why "Volume concentration size distribution" ? A number size distribution dN/dlogDp is shown but "Volume concentration size distribution" could be something like dV/dlogDp. The panel labels (i.e. a,b,c,d,e,f) are not shown in the image. In L424, please correct the unit. Please introduce "GM" and "GSD" used in the graphics. Please check here and elsewhere throughout the entire article for consistent sub-

and superscripts and fonts. What does the "1e2" at the upper end of the y-axis (I guess in panel c and d) mean, which is not shown in all other panels of Fig.9?

L432. Please explain the difference between the data shown in Fig. 9 and Fig. S2? Based on the labels (date and type of time notation) and the altitude range, no clear difference can be identified, other than the fact that different data are shown. Moreover, one might consider correcting the caption for Fig. S2 to match the notes concerning the caption of Fig. 9.

L432-445. According to the graphs, this section is intended to impact of the corrections; however, this is only explained to a limited extent. Instead, it describes the effects of volcanic emission plumes in relation to background measurements. Shouldn't the focus here be on the criteria used to select the cases and how the corrections affect the resulting findings? When talking about the different plume cases, the discussion should also include references to the specific image panels, provided they have corresponding labels.

L434. The article "Vernier et al., 2026" is not listed in the references.

L450. I suggest a consistent way of setting up equations, as well as a likewise consistent use of variables that have already been introduced. "*Q"*, for example, was already introduced in Section 3 as a flow variable. I recommend making a clear distinction.

L451-452. One may decide whether to use "index of refraction" or "refractive index" and stick consistently to that, as well as to the variable used for it (previously "RI", now "m").

L452. Why the flip to "d*n/*dln*(r)*" previously dN/dlogDp was introduced? This should be an aerosol number size distribution.

L455. It should be clarified somewhere that "sulfate aerosol" and "stratospheric aerosol (mixture of 70% H_2SO_4 and 30% H_2O)" are used synonymously, as can be inferred from the RI (correspondingly to L398) referenced here.

L460-463. "….comes from …. collocated extinction…"?? please rephrase the sentence for clarity.

L461 The article "Vernier et al., 2015" is not listed in the references.

L472. There are three panels in the upper part of Fig. 10.

L472-475. When discussing results, please refer to the corresponding figures and panels.

L480. What is somewhat lost in the correlation plot (Fig. 11, panel b for 940nm) but becomes apparent in the vertical profiles (Figure 10 a, b, and c) is that the positive bias seems to prevail only "in the plumes," while at lower altitudes (< 18-20 km, i.e. outside the plumes), a negative deviation is visible (for 940nm). Please explain this in more detail.

Fig 11. Please have the graphs start at the origin, 0, 0. If the data in this graph, as the caption states, comes from 11 flights (and not from the individual profiles shown in Fig. 10), I wonder whether this quite limited number of data points was averaged over time periods or altitude intervals or filtered in a certain way; if so, this should be specified.

L495. Please, specify "*SAGE III/ISS events*".

L496 and Table 3: I doubt that Aircore and ECC (ozone probe?) are meaningful for comparison with the POPC. If they do not contribute to the analysis, this information could be omitted from a revised table. It is unclear what "CFH" stands for.

L488-489. "....from 4 collocated flights (Table 3) between POPC and SAGE III/ISS...." That reads a little awkwardly. I suggest rephrasing this sentence.

Fig 12. The axis labels are small and partially covered. Specify the units of the values shown in rainbow colours. Please ensure that the colour scheme complies with the Copernicus guidelines on colour coding with regard to colour blindness. Some legends as well as some headlines are cryptic, consider discarding or clarification. Graphed data are partly covered by legends. Nevertheless, the headings seem to suggest that here a flow calibration was applied. This point has not been explicitly mentioned since Section 3.2. Perhaps it should be stated more clearly at the end of this Section or in the data presented afterwards which corrections (flow, η, *RI*, etc.) were made. Axis labels in shorthand style (e.g. "Conc.#/cm-3", "Ext. (km-1)", "Alt(km)" as in a lab notebook) can sometimes be misleading, but in my opinion, they are not suitable for publication.

L503. The text refers to "Kalnajs & Deshler, 2022b", which is not specified in the reference list.

L521-522. Please explain in layman's terms what " *for the matching purposes, two SAGE III/ISS occultations were used on 09/28 "* means and what the implications are.

Fig 13. The graphs are presented in poor resolution. If the main topic of the article is the concept of the "new" POPC-30 system, I wonder why the "Science results & interpretation" presented here in five panels based on the supposedly "old" POPC-06 measurements.

L529. Please specify precisely in what sense "significant variability in the stratosphere" is meant, which variable is referred to specifically? I wouldn't say that volcanic eruptions influence concentration profiles of an instrument. This refers to events whose effects (e.g., additional load on the atmosphere by aerosol material) can be identified based on measurements.

L532. The authors focus exclusively on volcanic influences here. Shouldn't the contribution of the pyroCb events (mentioned earlier) be considered or explicitly excluded here (e.g., for heights < 15 km) to ensure the comparability of measurements from different regions over larger vertical ranges?

L534. Blank space is missing prior to "marks".

L535. Better specify "with a persistent influence of volcanic aerosols in the tropical stratosphere". Is meant that, since this Ambea event, and above 20km altitude the "SR@532nm" value has generally not fallen below about 1.1 (just red out from the colour scale, not sure if robust)? Or that the stratospheric AOD did not fall below 0.005 since 2018. If so, I would emphasize this point more clearly by explicitly referring to Fig. S3. This figure (S3) is particularly valuable in that it more clearly highlights the relative contribution due to the eruption of Mount Pinatubo (as later mentioned in the article – L539).

L539. The article "Vernier et al., 2024 is not listed in the references.

L539-540 The campaign name and the acronym don't quite match up: which word contains the "K"? And is the project's title really "VOlcanic plume Dispersion and Decay in the strAtospheric" or rather "VOlcanic plume Dispersion and Decay in the strAtosphere"?

L541. For non-Americans, one could state, "Hampton, Virginia, USA"

L544 blank space prior to "with" is missing. It should read as "...the number concentration of aerosols peaks...".

L546. blank space prior to "moistening" is missing.

L545. This could be misunderstood. One could write: "... ejected volcanic material into the atmosphere, some of which even reached the mesosphere (Proud et al. 2022) ..."

L552-558. what do the authors have to say about the negative anomalies in panel a (Fig. 13), where concentrations approach 10^{-2} (approximately at 11-15 km in brownish, approximately 7.5–8.5 km in bluish, and approximately 3.5–7.5 km in greenish colours). The same applies to the concentration fluctuations in panel b (greenish: ~7–12 km) or the "feature" in figure d (brownish: at ~14–16 km).

Fig 14. The graphs are presented in poor resolution. Why is the synopsis of the data from POPC-30 campaigns supplemented with a median, but the data from POPC-06 (Fig. 13) is not? Could the authors explain the reasons for this? In which vertical resolution is the median profile provided (may be added in the caption)? Panel d: the median profile of one measurement profile, well...?! Wouldn't it make sense to plot the four median profiles in addition side by side in one graph and compare them in terms of their different launch positions and influencing? According to the headline of panel b, this one is from a BATAL mission - at India? However, no statement about this in the caption.

Fig 15. The graphs are presented in poor resolution. A comparative analysis of the median profiles (see comment to Fig. 14) could thus be expanded to include four additional median profiles and the corresponding discussion.

L575 why is "PyroCb" capitalized here?

L581 This is where the term "wildfires" from the introduction is mentioned for the first time again. In the introduction, "wildfire" was directly linked to the formation of pyroCb. Should "wildfire" be understood here (L581) to mean that pyroCb events have definitely occurred as well? Or may WBCs also be sufficient to transport wildfire emissions all the way to the tropopause region?

L589 why is "PyroCb" capitalized here? The information found online refers to the "Red Earth East Complex Fire" (REECF), which is why the acronym "REEFC" (suddenly appearing in L622, without definition) finally makes sense. Please correct the reference and introduce the acronym.

L604 by the way: Has the number size distribution comparison of various instruments used in a collocated setting, e.g., POPC vs POPS, been presented somewhere herein? Wouldn't that be a valuable addition to the content of this article?

L609. The text refers to "Deshler, et al. 2018", which is not in the reference list.

L613. This should say "volcanic eruption" instead of only "eruption". It should be clarified somewhere that "background conditions" refers to something like the absence of volcanic influences (or similar).

L626 I am still not convinced that it makes sense to speak of "more complex stratospheric sources". Most of the sources under discussion herein are located on the ground, so they cannot be "stratospheric sources", though they could indeed be sources of stratospheric aerosols. Why would these sources become more complex? In what way more complex? More complex than what, more than previously? These are not new sources; however, the magnitude of their contributions may increase.

Supplement: Regarding Section S3 in this document, which refers to "*Retrievals of extinction from COBALD*" I cannot find any reference to it anywhere in the main article.

References:

Vignelles et al. 2027: Any reference to this article is missing in the main text.

Vernier et al. 2023: Any reference to this article is missing in the main text.

Vernier et al. 2022; Any reference to this article is missing in the main text.
