Summary of Revisions

We thank the reviewers and the editors for their time and comments that have significantly improved the manuscript.

Major changes include:

- **LCS Data Handling:**
  - Removed all comparisons of LCS data to regulatory thresholds and reframed interpretation as qualitative ("elevated concentrations") rather than quantitative.
  - Separated LCS data from regulatory-grade data (now in Section 3.4.2 and Figure 12) and added clear disclaimers in text and captions.
  - Clarified sensor co-location details and uncertainty estimation.
- **PM Source Attribution:**
  - Added SO_2-based plume filtering method and presented both filtered and unfiltered PM datasets.
  - Incorporated identification and discussion of two summer dust storms (24–29 May and 3–4 June 2021) with supporting HYSPLIT back-trajectory and observations.
  - Expanded discussion on variability in PM ratios and cited relevant Icelandic dust storm studies.
- **Figures and Tables:**
  - Converted Table 3 into Figure 9 for better visualization of PM source "fingerprint" ratios; moved original table to Appendix (Table A1).
  - Updated Figures 3–6 for colour consistency and clarified captions; added explanation for small IQR values.
  - Revised Figure 12 for clarity (visitor data as line graph; LCS data shown as daily max–min range).
- **Textual Revisions:**
  - Corrected introduction claims about PM_1 measurements; clarified novelty as first volcanic PM_1 time series.
  - Rephrased statements on health impacts to avoid implying measured outcomes.
  - Added explanation of alerting process during eruption and clarified visitor exposure limitations.
  - Combined sections 3.2 and 3.3 to avoid repetition and improve flow.
  - Removed most of Section 3.5.2 (3.4.2 in the revised version) per reviewer request, retaining only a short lessons-learned paragraph.
  - Added missing references and corrected typographical errors.

Below we list our detailed responses to the reviewers' comments. The reviewers' comments are in *blue italics* and our responses are in normal font.

Replies to Anonymous Referee #4

AR#4: Thank you for asking me to review this paper. I was not one of the original reviewers so I have reviewed this paper as I would for any review (blind – I didn't read the reviews and responses first). The paper is well written and important, and should be published but, in my view, although the paper is clearly much improved, there are some additional issues that should be addressed. I have separated these into major and minor issues although I hope all can be easily and quickly addressed..

We thank the reviewer for their time and thoughtful and constructive comments, which have improved the manuscript.

Major issues

1.Section 2.2 line 192-196 and other places throughout the paper – While I appreciate the effort taken to explain that the LCS data are not reliable, you then use the data to determine if SO2concentrations are above or below the hourly AQ ID threshold. However, without co-location and calibration, your sensors could have been highly inaccurate. It is only reasonable to give ahigh/low concentration indication for SO2 concentrations, or to give relative values amongst the different sensors (but even this is challenging given that it doesn't seem like the LCS were first co-located together to check their precision). By comparing to a regulatory ('absolute')concentration, you are undertaking a quantitative assessment which cannot be verified. Stating that the assessment is 'qualitative' by just saying whether the exceedance occurred or not, does not make this an acceptable method. I have no doubt that the concentrations would be detectable (line 195) but this is not the point. In my view this analysis should be removed, throughout the paper. In relation to this, Figures 3 (panel a) and 11 (panel b), and section 3.3are particularly problematic. Please see comments, below.

We appreciate the reviewer's detailed comments and have carefully considered their concerns. In the revised manuscript, we have substantially revised the analysis and discussion of the LCS data to further clarify their limitations and ensure that our interpretation is consistent with the capabilities of these sensors. Specifically, we have:

- Removed any comparisons to regulatory thresholds and reframed the discussion to emphasize only qualitative trends and relative differences among sensors.
- In Section 2.2. we clarified that the LCS were co-located in the field during the eruption period and the co-location data were used to estimate the sensor uncertainty, presented as error bars (formerly Figure 3, now Figure 12).
- Removed LCS data from Figure 3 and from Section 3.3. in order to clearly separate its presentation and discussion from regulatory-grade data.
- The figures and sections where LCS are presented have been revised accordingly (Figure 12 and Section 3.4.2 in the revised version.)

We believe that presenting the LCS data remains important because these sensors provided the only near-real-time information available during the 2021 eruption and continue to play a role in the operational hazard assessment for ongoing activity in the region. The revised manuscript presents these data in an appropriate and transparent way, without implying quantitative accuracy.

Minor issues

2.Abstract – I am uncomfortable with the phrasing of the sentence 'This suggests a possible increase in adverse health effects.' given that no research was conducted to confirm this. Instead, I would suggest: 'This suggests the potential for an increase in adverse health effects.'

We agree with the suggested rephrasing and have amended the text accordingly.

3.Section 1.1, 1st sentence 'Much of the existing knowledge on the health impacts of volcanic air pollution comes from epidemiological and public health investigations of the eruptions at Holuhraun in Iceland and Kīlauea in Hawaii.' I do not agree with this sentence. What about all the studies related to ash? Modify the sentence to clarify that you are referring to gases and aerosols.

Thank you, the text has been amended.

4.Section 1.1 line 104 – 'Epidemiological studies in volcanic regions further indicate that children (defined as ≤4 years old)' – please add in the word 'young' before 'children' since, clearly,children can be older than 4 years old.

Thank you, the text has been amended.

5.Section 1.1. line 131-132 – typo - remove one of the 'been's.

Thank you, the text has been amended.

6.Figure 1 – what are the red dots across the main map? Please label them in the caption.

Explanation added to the caption: "Red circles on the main map show the location of populated areas, including the capital area Reykjavík which is represented with a comparatively larger circle. The stations were organised in seven geographic clusters (each shown on the enlarged insets)."

7.Section 2.1 line 171 - 'pulsed fluorescence in the ultraviolet' – isn't this usually termed pulsed ultraviolet fluorescence?

Thank you, the text has been amended.

8. Section 2.2 line 178 – 'PM was not monitored with this network due to cost-benefit considerations as PM does not pose as acute a hazard as SO2 for short-term exposure.' I don't think I agree with this sentence. PM, especially acid coated PM, may cause acute respiratory issues just like SO2 can do. Maybe replace 'does not' with 'may not' unless you can provide a robust reference (e.g. meta-analysis) to evidence this point. Also, hyphenate cost-benefit.

Thank you for this suggestion. We agree that the original phrasing could be misleading. In the revised manuscript, we have simplified the sentence to: *"PM was not monitored with this network due to cost-benefit considerations."*. This reflects the primary reason for excluding PM measurements without making assumptions about its relative health impact.

9. Section 2.2 line 181 and section 3.5.2 lines 705-714 – 'The main purpose of the eruption-response network was to alert visitors when SO2 levels were high'. Please explain how this alerting was done.

We have added the following explanations to sections 2.2 and 3.4.2 (formerly 3.5.2):

"The measurements from the sensor network were publicly available in real-time on the EAI air quality monitoring website (airquality.is). The eruption site was staffed by members of the rescue services and/or rangers, who carried handheld SO_2 LCS to supplement the installed network. When any of the LCS reported SO_2 concentrations as elevated (potentially-above 350 μg/m^3) visitors were urged to relocate to areas with cleaner air. During the course of the 2021 eruption and subsequent events (2022–2025), SO_2 measurements from the LCS stations were also used by the IMO to produce hazard maps around the active and potential eruption sites, with hazard zones defined by the distances at which elevated SO_2 was detected (Icelandic Meteorological office, 2025)".

Reference: Icelandic Meteorological office: https://en.vedur.is/volcanoes/fagradalsfjall-eruption/hazard-map/, last access: 14 December 2025.

10. Section 2.2. line 194 – What is 'ID'? It is actually explained later, in line 258. This needs to be moved to first mention.

Thank you for pointing this out. The acronym ID (Icelandic Directive) was originally defined in Section 2.1 (line 168). In the revised manuscript, we have removed the reference to ID from Section 2.2 to align with the reviewer's recommendation that LCS data should not be compared to air quality thresholds. Additionally, we have ensured that the acronym is explained again in Section 2.4 where it is mentioned, to avoid confusion.

11. Section 2.4 line 261 and Section 1.1 line 123 – In Section 1.1, it says 'This study contributes the first regulatory-grade time series and exposure dataset of PM1 from a volcanic source, as well as the first measurements of PM1 in Iceland.' Yet it becomes clear, later, that Iceland has been measuring PM1 since 2020 (line 227-8) and they have a regulatory threshold already in place. Therefore, this isn't the first measurement of PM1 in Iceland. The Introduction should be corrected/clarified.

We have rephrased the sentence in section 1.1 to clarify our intended meaning: "This study reports on the first three years of regulatory-grade PM_1 measurements in Iceland (2020-2022) and represents the first regulatory-grade time series of PM_1 from a volcanic source.". We have also removed a similar statement from Section 2.4 to avoid repetition.

12. Section 3.1 lines 288-289 – 'The proportion of PM1 mass within PM10 increased from 16-24% in the background (standard deviation 7-13%) to 24-32% during the eruption (standard deviation 16-19%);' Are the values ranges of the raw data or ranges of the means? It would make sense if they were ranges of the means at the different stations. If they are ranges of the raw datasets, it is strange to give standard deviation without the means. Also, I presume this is 1 SD? Figure 2 indicates that the data presented in the main text are likely mean + 1 SD but this needs clarifying.

We have clarified this as suggested by the reviewer: "The proportion of PM_1 mass within PM_{10} increased from the average of 16-24% in the background (one standard deviation ±7-13%) to 24-32% during the eruption (±16-19%) …"

13. Table 1 (& Table 2). 'The number of AQ exceedances is the maximum number of exceedances recorded by an individual station within a geographic area.' Is this sentence necessary given that the sentence before already explains what 'ID exceedances' means? Or is the 2nd sentence referring to columns labelled 'Number of AQ exceedances' that no longer exist that are different from the 'ID exceedances' columns? Looking at Table 2, I now think that this is *an issue with the column labelling. In Table 2, there are similar sentences, but both refer to 'AQ exceedances' rather than 'ID/AQ exceedances'. As with Table 1, I don't think that the final sentence is required. Maybe, to cover the point you are making and to avoid confusion, you could combine the sentences as follows (example for Table 2): 'AQ exceedances' denotes the number of times PM concentrations (at any single station within a geographic area) exceeded the following thresholds: PM10 - 50 µg/m3; PM2.5 - 15 µg/m3; PM1 - 13 µg/m3.'*

We thank the reviewer for a good suggestion for how to combine the two sentences and clarify the meaning:

Revised caption Table 1: SO_2 concentrations (hourly-mean, µg/m^3) in populated areas around Iceland during both the non-eruptive background and the Fagradalsfjall 2021 eruption. 'Average' is the long-term mean of all stations within a geographic area ± 1σ standard deviation. 'Peak' is the maximum hourly-mean recorded by an individual station within the geographic area. 'ID exceedances' denotes the maximum number of times SO_2 concentrations (at any single station within a geographic area) exceeded the Icelandic Directive (ID) air quality threshold of 350 µg/m^3.

Revised caption Table 2: PM_{10}, $PM_{2.5}$ and PM_1 concentrations (µg/m^3, 24-h mean) in populated areas around Iceland during both the non-eruptive background ('BG'), the whole eruption period ('Eruption'), and on 'plume present' days only (see Methods for the definition of plume-present days). 'Average' refers to the long-term mean of 24-hour values of all stations within a geographic area ± 1σ standard deviation. 'Peak' is the maximum 24 h-mean recorded by an individual station within the geographic area. 'AQ exceedances' denotes the maximum number of times PM concentrations (at any single station within a geographic area) exceeded the following thresholds: PM_{10} - 50 µg/m^3; $PM_{2.5}$ - 15 µg/m^3; PM_1 - 13 µg/m^3.

14. Figure 3 (& Figures 4-6). Firstly, it's not at all clear that Figure 3 shows box and whisker plots. I can't see the boxes or 'whiskers' except the crosses for statistical outliers. Figures 4-6 are much clearer, especially as the box is wider than the crosses. Could you present the data on a logarithmic scale to allow visualisation of the boxes? Each panel is also quite small, so it is hard to see the detail.

We understand this comment and we have experimented with a logarithmic scale as shown below on Figure A; however, we believe that a linear scale is more suitable to display and discuss our data due to the following reasons:

1. The box and whiskers are not visible on Figure 3 because the median value and the interquartile range are very low values (a few µg/m^3). This is because the SO_2 concentrations in the local background atmosphere are virtually zero, and most of the time the background is 'clear' of volcanic air pollution; but when the volcanic plume is advected into the area, the concentrations become very high. One of the key findings in our study is that the average values of SO_2 are not significantly affected by the volcanic eruption, but the SO_2 pollution peaks (i.e. the statistical 'outliers') are much higher during the eruption than during the background period. We have clarified this in the Figure 3 caption "*Note that the IQR is very low in most cases due to the negligible SO_2 concentrations in the local background; as a result, most of the SO_2 pollution episodes are statistical outliers*". On Figures 4-6, the range of PM concentrations in the background are non-negligible, resulting in a visually clearer statistical distribution.

2. A logarithmic y-axis scale does not make it easier to see the median and the interquartile range as shown on Figure A below (because, as explained in the previous paragraph, these values are very small compared to the statistical outliers). The logarithmic scale also has the well-known problem of visually inflating smaller values, meaning that in this case, the difference in the peak SO_2 values between the background and the eruption periods is represented as being relatively small but in reality it was large. Given that the aim with this figure is to illustrate the large difference in the peak values we argue that using a logarithmic scale would be counterproductive for this reason.

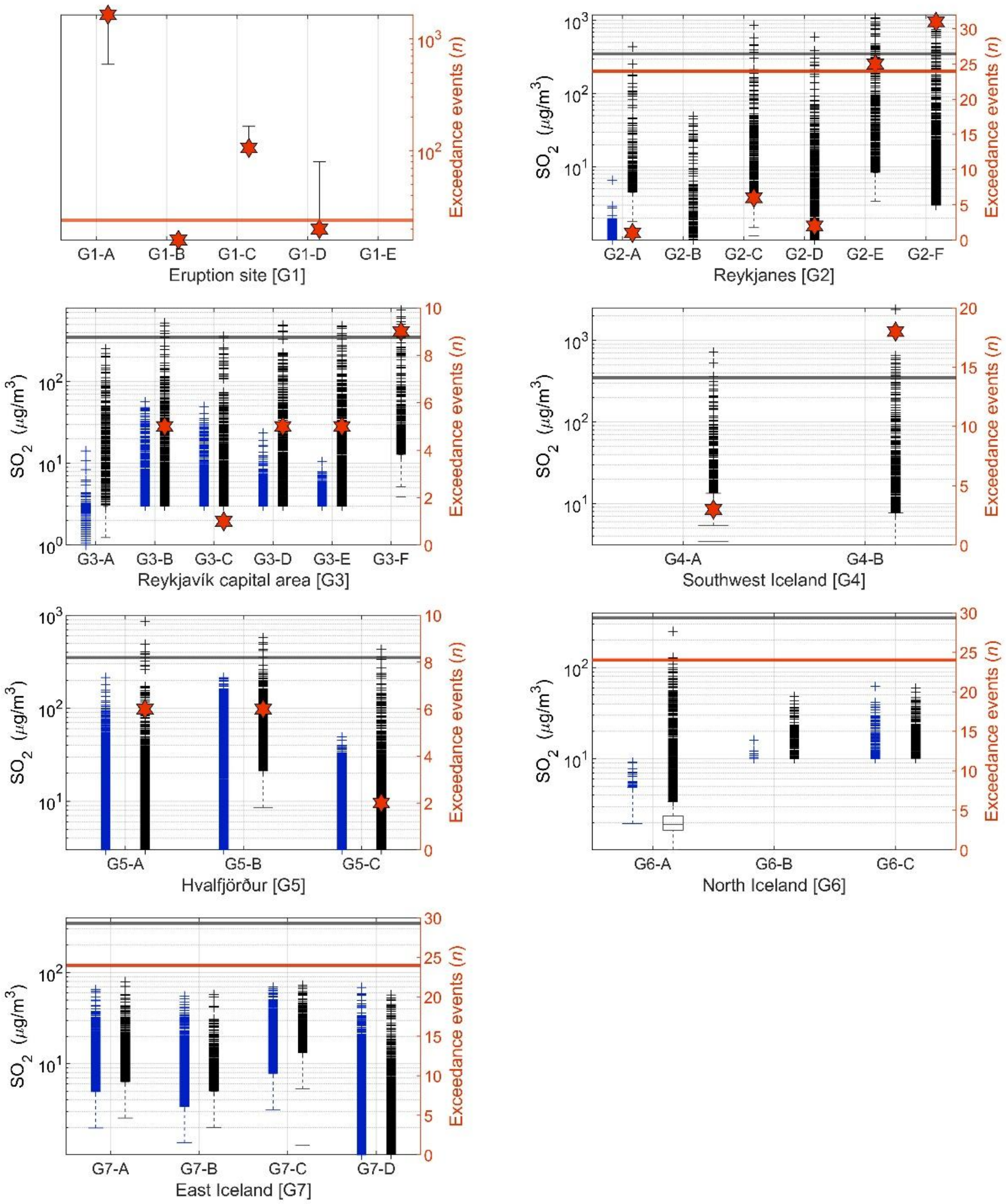

Figure A: Figure 3 with y-axis logarithmic scale

Secondly, it took me a long time to work out that the stars in Figure 3 relate to the number of exceedances (although it does say this in the caption) and, therefore, the right-hand axis. I think it would really help if the stars were orange rather than red, to match the exceedance line and the right-hand axis. It would also really help if the star, line and axis were in the same shade of orange. Same issue for Figures 4-6 – make the filled star the same shade of orange as the unfilled star and right-hand axis (but at least these ones are orange, not red … or at least that's what it says in the captions … it's actually hard to tell!).

We thank the reviewer for pointing out the colour inconsistency in the figure and have fixed it on all relevant figures (Figures 3-6).

Note my (major) issue with the validity of comparing LCS data to regulatory thresholds, and therefore the inclusion of panel a of Figure 3. If this is to remain in the paper, i) please explain what the error bars refer to, in the caption; and ii) in the main text be absolutely clear that these values are extremely indicative and should not be taken as definite exceedances. Currently, panel a isn't discussed in the text at all (on p12).

We have made the following changes to the manuscript to address the reviewer's comments and suggestions:

i) The error bars on panel A of Figure 3 in the original manuscript represented the sensor uncertainty based data from sensor co-location in the field. We have clarified in section 2.2. what the error bars on LCS data represent (now presented on Figure 12 in the revised manuscript):

   Section 2.2: "The absence of a regulatory-grade field calibration significantly limits the accuracy of LCS dataset, particularly at lower concentration levels. To partially mitigate this, two LCS units were co-located at station G1-B between 6 and 22 June 2021 to quantify inter-sensor uncertainty. The co-located sensors were of two types used in this study: Crowcon XGuard (deployed at G1-A throughout the monitoring period and at G1-B until 22 June) and Alphasense SO_2-B4 (deployed at G1-B from 22 June and at G1-C, D, and E for the entire period). The measured concentrations showed a strong linear correlation ($r^2 = 0.70$), but Alphasense reported lower values relative to Crowcon, with a correlation coefficient of 0.38 (Fig. A2). This coefficient was used to estimate the measurement uncertainty for the two sensor types, represented here as error bars on relevant figures. While the colocation experiment was useful for identifying uncertainty between sensor brands, it did not quantify variability among sensors of the same brand.

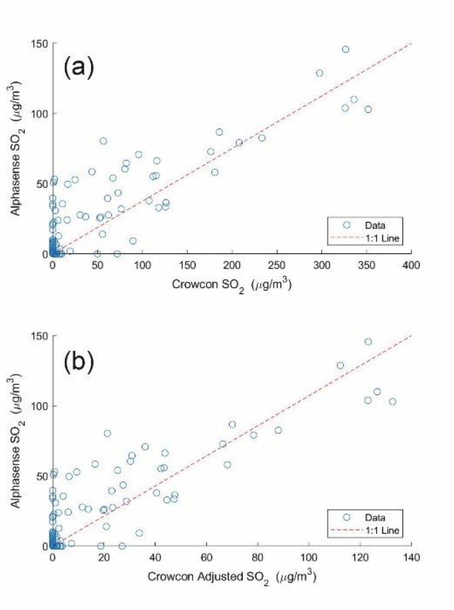

Figure A2: SO_2 concentrations measured by two types of lower-cost sensors (LCS) used in this study—Alphasense SO_2-B4 and Crowcon XGuard—during a field colocation at the eruption site (6–22 June 2021). Measurements from the two sensors showed a strong linear correlation ($r^2 = 0.70$), but Alphasense reported lower values relative to Crowcon, with a correlation coefficient of 0.38. Panel (a) Correlation of raw data points from the two sensors. Panel (b) Correlation after Crowcon data were adjusted using the correlation coefficient

ii) We have made the following changes to ensure clarity:

a. **Separated the datasets visually** by presenting LCS data in a new figure (Figure 12), distinct from the regulatory-grade measurements, to avoid any implication of equivalence. The revised Figure 3 includes only the regulatory-grade measurements.

b. **Removed references to air quality thresholds** when discussing LCS concentrations in Sections 2.2 and 3.4.2 (previously 3.5.2). We now refer only to "elevated concentrations" rather than exceedances.

c. **Revised text in Section 2.2** to explicitly state the indicative nature of the LCS data:
"Given the calibration and co-location limitations, we do not report quantitative SO_2 concentrations from the LCS network. Instead, the data are presented as a qualitative indicator of whether concentrations were likely elevated—defined as exceeding 350 μg m^{-3} hourly mean—within the uncertainty of the sensors. This threshold is approximately two orders of magnitude above the manufacturer-reported detection limit, making it reasonable to assume that such levels were detectable. However, these values should be interpreted only as indicative; 'elevated levels' do not represent confirmed air quality exceedances."

d. **Added a clear statement in the caption of Figure 12**:
"The LCS data should be interpreted only as indicative; 'elevated SO_2' levels do not represent confirmed air quality exceedances."

15. Section 3.2 lines 345-361 – one thing that is not discussed is that PM in Iceland can also be influenced by dust mobilising events from the island's interior. Did you check wind conditions, and other evidence, to ensure that none of the increased PM during eruption periods was, *coincidentally, from different crustal sources? Or potentially other acute sources (fires/construction etc.)? The way it is written at the moment, it is assumed that there is a causation in the correlation! However, in section 3.3, this is briefly discussed – in general terms – but whether the data collected solely relate to increases due to volcanic pollution is not discussed.*

To address the reviewer's concern about non-volcanic PM sources, we revised the manuscript as follows:

- **Section 2.3 (Data Processing):** Added a detailed explanation of how volcanic PM was distinguished from other sources using SO_2-based plume identification, while acknowledging limitations (e.g., dust storms, smelter emissions, mixed-source days). Both filtered (plume-present) and unfiltered PM datasets are now presented for transparency.

  Section 2.3: "The importance of non-volcanic sources of PM in Iceland meant that PM concentrations during the eruption period may have been elevated independently of volcanic activity. To identify the volcanic contribution to PM levels, we processed the data following a similar approach to Ilyinskaya et al. (2017). PM data were filtered to include only periods when SO_2 concentrations exceeded the non-eruptive background average; these periods are hereafter referred to as 'plume-present days'. Stations G3-G and G3-H did not monitor SO_2 and were filtered using SO_2 data from stations located within 2 km distance (G3-A and G3-E, respectively). This plume-identification approach has inherent strengths and limitations. First, it is effective at sites with negligible non-volcanic SO_2 sources, which applies to most of the monitored locations in Iceland; however, its reliability decreases near aluminium smelters, which represented a minor yet locally important SO_2 source at stations G5-all, G6-C, and G7-all. Second, it may exclude periods when the volcanic plume was present with low SO_2 but elevated PM, as can occur when the plume is chemically mature (Ilyinskaya et al., 2017). Third, it cannot distinguish between days when PM is predominantly sourced from an eruption and days when volcanic PM is strongly mixed with another PM source, such as dust storms. To address these uncertainties, we present both filtered and unfiltered PM datasets and compare them in our discussion"

- **Section 3.2:** Included analysis showing that some of the major PM_{10} and $PM_{2.5}$ peaks during the eruption period were linked to dust storms, confirmed via HYSPLIT back-trajectory and crowd-sourced observations (Figure A14). We have discussed confidence levels for plume-day identification and highlighted that PM_1 is strongly correlated with SO_2, suggesting that the volcanic eruption was the most important source.

- **Figures and Tables:** Updated Figures 2, 4–6 and Table 2 to show plume-present days, background, and whole-eruption data for comparison.

16. In section 3.3, it's not clear if the number of exceedances (line 420) includes those at the eruption site (G1) from LCS. If so, I would remove these values (or discuss them separately). Given that the following sentence says there were 16,000 exceedances at a G1 station, my guess is that the 0-31 value does not include G1 stations (!) but this should be clarified. At this point, there should be a clear discussion to highlight the reliability of the data indicating 16,000 exceedances, if you are going to keep these data in the paper.

We have clarified the separation between datasets to address the reviewer's concern. In the revised manuscript:

- **Regulatory-grade data** are discussed exclusively in Sections 3.1–3.3.
- **LCS data** are discussed separately in Section 3.4.2.

17. In general, section 3.3 seems repetitive of earlier sections and those parts that are not could have been incorporated into earlier discussions, and Table 3 would be much better visualised as a figure, with the main text being more explanatory (line 456: 'suggests distinct 'fingerprint' ratios' – so, what are these? It's hard to work this out from the table).

To address the reviewer's suggestion, we have:

- **Merged Sections 3.2 and 3.3** (now section 3.2) to reduce repetition and improve flow.
- **Converted Table 3 into a figure (Figure 9)** for clearer visualization of the 'fingerprint' ratios.
- **Stated the fingerprint ratios in the text of section 3.2:** "This comparison suggests distinct 'fingerprint' ratios for the different PM sources: volcanic plume periods show the highest PM_1/PM_{10} ratios (mean range 0.3–0.9), dust storms the lowest (mean range during storm peaks 0.04–0.05, mean range during the whole storm 0.1–0.3), and background conditions intermediate (mean ~0.2)."
- **Moved the original Table 3 to the Appendix (Table A1)** to support Figure 9 with detailed timings of the visualised pollution events, and station-specific mean pollutant concentrations.

18. Section 3.4 – repetition re. Pfeffer et al. 2024 model limitations in lines 486-491 and 516-521.

We have rephrased the relevant parts of this section to avoid repetition and make the discussion of the model limitations clearer:

"Supplementary Figures S1 and S2 show animations of the simulated dispersion of volcanic SO_2 at ground level during the two pollution episodes discussed in this section, 28-30 May and 18-19 July 2021. The simulations were produced by a dispersion model used operationally for volcanic air quality advisories during the eruption by the Icelandic Meteorological Office (IMO) (Barsotti, 2020; Pfeffer et al., 2024). As discussed by Pfeffer et al. (2024), the model had a reasonable skill in predicting the general plume direction but relatively low accuracy in simulating ground-level SO_2 concentrations for the 2021 eruption (Pfeffer et al., 2024).The model results are included here for qualitative purposes—as a binary yes/no indicator of potential plume presence at ground level. The sharp ground-level movement and boundaries of the plume during the 28–30 May episode were captured reasonably well by the model (Supplementary Figure S1), but the larger episode on 18-19 July was not reproduced by the model. This highlights the challenges of accurately simulating ground-level dispersion of volcanic emissions from eruptions like Fagradalsfjall 2021, as well as other small but highly dynamic natural and anthropogenic sources (Barsotti, 2020; Pfeffer et al., 2024; Sokhi et al., 2022). High-resolution observational datasets, including those presented here, can support improvements in dispersion model performance."

19. Figure 9 – there is an issue in the caption. Instead of panels c and d, the caption says panels g and h.

Thank you for pointing this out, corrected to c) and d). Please note that this figure is now Figure 8.

20. Section 3.5.1 line 638 – do you think there is a cumulative impact on air quality from different eruptive events? I do not think so. There is more than sufficient time for the air quality to return to background concentrations between episodes. I would remove this (but keep in the cumulative effect on public health). And I would say that the health effects could be chronic 'as well as' acute (instead of 'rather than').

We agree and have amended the text accordingly: "Although each event has been relatively short-lived—ranging from several days to several months—their cumulative impact on public health may be chronic as well as acute, and thus warrants comprehensive investigation.".

21. Figure 11 – I strongly disagree with using 'hours above SO2 ID threshold' for the reasons stated in the major comments, above. It would be much better to use the raw data to give indicative air quality concentrations (time series) at each station, allowing qualitative comparison among stations with no reference to air quality thresholds.

Additionally, the graph is very hard to read with bars being used both for daily visitors and SO2 concentrations (for multiple stations – I can only differentiate 2 or 3 because the greyscale is similar and complicated by the overlayed blue bars). Also, to the left of the orange dashed line (note, the O in SO2 label looks subscripted), the bars look both blue and grey. I don't understand this if sensor installation hadn't happened yet. Time series line graphs for SO2 concentrations would improve graph visualisation and interpretation.

Please see our response to the reviewer's major concerns above. In the revised manuscript, we have removed all comparisons of LCS data to regulatory thresholds. We maintain that reporting actual SO_2 concentrations from the LCS network would be misleading given the high sensor uncertainty. Instead, we present the LCS data in the most appropriate format—as a qualitative indicator of whether the SO_2 concentrations were likely elevated.

Revised section 2.2: "Given the calibration and co-location limitations, we do not report quantitative SO_2 concentrations from the LCS network. Instead, the data are presented as a qualitative indicator of whether concentrations were likely elevated—defined as exceeding 350 $\mu g\ m^{-3}$ hourly mean—within the uncertainty of the sensors. This threshold is approximately two orders of magnitude above the manufacturer-reported detection limit, making it reasonable to assume that such levels were detectable. However, these values should be interpreted only as indicative; 'elevated levels' do not represent confirmed air quality exceedances". Please also see the revised caption of Figure 12 (formerly Figure 11): "The LCS data should be interpreted only as indicative; 'elevated SO_2' levels do not represent confirmed air quality exceedances."

We have revised Figure 12 (formerly Figure 11) to improve clarity and address the reviewer's comments:

- Visitor data is now shown as a line graph rather than bars.
- The LCS data from all five stations are presented as a single bar type, representing the daily max–min range across all five stations, rather than multiple overlapping bars.
- This approach removes the confusing greyscale scheme and ensures that the figure is easier to interpret.

- We also corrected the SO_2 label formatting and ensured that no bars appear before sensor installation.

22. Section 3.5.2 lines 670-680 – another point is that you don't know if individuals visited multiple times, therefore increasing their exposure.

We have added a sentence on this "In addition, there is no data on whether people visited the eruption multiple times and were therefore potentially cumulatively more exposed."

23. Section 3.5.2 lines 697-714 – this section is weak. You have not conducted any evaluation of the efficacy of the LCS network because neither the precision or accuracy have been measured. Therefore, drawing conclusions of its meaningfulness and utility is overstepping the remit of this paper, especially given that there is also no information on how alerts were disseminated and whether they were responded to or not. Please remove this section. This aligns with my other concerns about the LCS data reported in this paper.

We have removed most of Section 3.5.2 (3.4.2 in the revised version) in accordance with the reviewer's recommendation. However, we retained a short reflective paragraph because it is intended as a lessons-learned statement and a call for action in future emergency monitoring campaigns, rather than an evaluation of the LCS network's performance. The retained text reads:

"In conclusion, the deployment of the LCS network at the eruption site for the purposes of alerting people to potentially-high SO_2 concentrations was likely valuable given the high frequency of elevated SO_2 concentrations and the large number of visitors in a confined area. However, the absence of regulatory-grade calibration prevented any quantitative assessment of individual exposure to hazardous pollutants. To obtain high-quality datasets with LCS, regular and frequent field calibration against regulatory instruments is essential. However, such calibration is typically feasible only during short-term campaigns at reasonably accessible locations. In this crisis-response scenario, the challenging terrain and limited accessibility of the eruption site precluded field calibration. The primary concerns associated with uncalibrated LCS in emergency contexts are false negatives—where the sensor underreports concentrations that exceed health thresholds—and false positives—where the sensor overreports concentrations that are actually below threshold. False negatives pose a problem by failing to alert individuals to hazardous conditions, while repeated false positives may undermine public trust and reduce compliance with safety advisories. "

24. Conclusions line 720 – 'These results suggest that the Fagradalsfjall eruption may have contributed to measurable adverse health effects, warranting further public health investigations.' As already discussed, health effects were not measured, so such wording needs to be carefully chosen. I would suggest something like: 'These results suggest that the Fagradalsfjall eruption generated sufficient air pollution that it could have triggered negative health responses, which should be investigated retrospectively or during future events.'

We agree and have amended the text according to the reviewer's suggestion.

25. Figure A1 caption – 'The cover was custom-made from Plexiglass with the sensors are recessed…' Remove 'are'.

Amended.

26. Figure A2 and A5 captions – ‘The stations were not in operation before the eruption an therefore’ Replace ‘an’ with ‘and’.

Amended – thank you for the careful proofreading.

27. Figure A4 – panel 3A has multiple horizontal dashed lines. I think this is because the Y axis scale has more tick marks than the other panels despite using the same scale as the other panels. Please remove. Figure A7 has the same issue but at least it is in all the panels.

Figure A4 (A5 in the revised version) has been amended. We did not revise Figure A7 (now A8) as the ‘extra’ grid lines make it easier to read the values on the log-scaled y-axis in this particular case.

Replies to Referee #5: Pavla Dagsson Waldhauserova

Comments to the Editor and Authors

This study presents a valuable investigation into the impacts of recent volcanic eruptions on the Reykjanes Peninsula, Iceland, on air quality and the long-range transport of volcanic emissions across the country, particularly to densely populated areas such as Reykjavík. It is an important contribution that combines real-time measurements with data on resident exposure and visitor numbers at the eruption sites. The study also provides evidence that fissure eruptions are one of, or potentially the most, important sources of PM_1 *in Iceland. However, more comparisons with dust storms and biomass burning events are needed.*

The paper is clearly written, and the figures effectively support the analyses. I highly recommend this work for publication in EGUsphere, pending minor revisions as outlined below.

We would like to thank Pavla for the review, which has greatly improved the manuscript.

General comments:

Fissure eruptions are emphasized here as an important source of PM_1 *in Iceland. Could you provide a more detailed description of all the potential* PM_1 *sources associated with an effusive eruption? You mention ash and sulphate particles directly emitted from the eruption, but it is clear that burning mosses around the eruption sites significantly increased* PM_x *concentrations, for example, in August 2023, with a clear signature of black carbon.*

We have added text to the Introduction and Conclusions to explain that eruptions can trigger wildfires, but also to clarify that this was not the case in the 2021 eruption and is therefore outside of the scope of this particular study.

In section 1.1 "Some eruptions (e.g. at Kīlauea, Cumbre Vieja, and several recent Reykjanes episodes) cause extensive lava-ignited wildfires, which are also a source of PM.".

In section 1.2 "The 2021 eruption did not trigger significant wildfires; however, several subsequent episodes have caused extensive fires (primarily of vegetation but also some urban structures), warranting a dedicated investigation into their effects on air quality and related health outcomes.".

In Conclusions: "the high frequency of eruptions, and eruption-ignited wildfires in this region since 2021 raises the possibility of chronic exposure, which should also be examined, particularly given that the ongoing Reykjanes Fires'eruptions may continue for several generations."

It might also be worthwhile to include long-range transport of biomass smoke from Canadian and U.S. fires, which have significantly increased PM_1 *levels in Iceland, in Table 3. Table 3 is a particularly strong part of the study.*

We thank the reviewer for recognising the importance of Table 3 and for suggesting the inclusion of North American wildfires. However, we argue this falls outside the scope of the present study, which is focused on PM sources that are very frequent and local to Iceland. Our group is preparing a separate paper dedicated to wildfire air quality impacts, including lava-ignited wildfires from the Reykjanes eruptions. As both this manuscript and the forthcoming paper are led by PhD students, merging these topics would compromise the independence and integrity of their respective research efforts. We appreciate the reviewer's and editor's understanding.

Long-range transport of SO_2–PM_x plumes outside Iceland is not addressed. Existing studies have documented such transport to Ireland (e.g., Ovadnevaite et al., 2009). Moreover, emissions from the Reykjanes Fires have been detected in Svalbard and elsewhere, but this paper does not include information on that.

Ovadnevaite J., Ceburnis D., Plauskaite-Sukiene K., Modini R., Dupuy R., Rimselyte I., Ramonet R., Kvietkus K., Ristovski Z., Berresheim H., O'Dowd C.D., 2009. Volcanic sulphate and arctic dust plumes over the North Atlantic Ocean. Atmospheric Environment 43, 4968-4974.

We have added the following discussion to section 3.2:

"Historically, larger Icelandic fissure eruptions (>1 km^3 of erupted magma) have caused volcanic air pollution episodes far beyond Iceland—across mainland Europe during the 2014–2015 Holuhraun eruption (Schmidt et al., 2015; Twigg et al., 2016) and potentially even farther during the 1783–1784 Laki eruption (Grattan, 1998; Trigo et al., 2009). Simulations indicate that associated health impacts in Europe could have been substantial (Heaviside et al., 2021; Schmidt et al., 2011; Sonnek et al., 2017). During the recent Reykjanes eruptions (2021–2025), elevated volcanic SO_2 was detected at ground level by UK regulatory-grade stations on at least one occasion, in May 2024, exceeding previously documented levels at this distance (UKCEH, 2024). This suggests that PM concentrations may also have been elevated beyond Iceland during these events. Assessing the impacts of recent eruptions on air quality and public health in European and potentially more distant communities is therefore an important priority for future research."

We did not include Ovadnevaite et al 2009 in this discussion because it does not report on volcanic plume transport during an eruption, but rather on diffuse emissions in Iceland during a period of no eruptions. The dynamics of plume transport will likely be very different and not directly comparable. Instead, we have referenced studies which focus on emissions during Icelandic fissure eruptions and report direct observations from regulatory-grade stations in Europe: Schmidt et al., 2015; and Twigg et al., 2016.

Regarding the potential detection in Svalbard: We were unable to confirm which publication the reviewer was referring to, as no further details were provided. The study that most closely matches this description appears to be:

Wu, K., Luo, Y., Li, Q., Zhou, H., Xi, L., and Si, F. (2025). Arctic haze induced by an Icelandic volcanic eruption: Evidence from China's highest-resolution trace gas monitoring. The Innovation Geoscience, 3(2), p.100131.

This is a potentially interesting report based on satellite observations of the 2024 eruption; however, we had concerns about its scientific authenticity that precluded citing it. Specifically, Figure 1 appears to include an unrealistic, AI-generated image (although use of GenAI is unreferenced), and the statement that *"On August 23rd and 24th, the SO_2-rich air mass spread to the south and the east under the influence of prevailing winds, affecting Scotland and Ireland for a time"* seems unsupported by data or references.

The reference list is incomplete, as several cited works are missing. The discussion should include more references to in-situ studies conducted in Iceland that are currently not cited.

Specific comments:
L121 – Tomášková et al. (2024) is missing from the reference list.

Thank you for noticing this, this has been amended.

L446 – Dust from local Icelandic deserts does not need to be "resuspended," similarly to desert dust elsewhere. These are primary dust sources, often originating from beneath glaciers, meaning the material cannot be considered resuspended.

Amended to: "The dominant non-volcanic PM source in Iceland is natural dust from highland deserts, with dust storms occurring frequently throughout the year with significant regional and seasonal variability (Butwin et al., 2019; Dagsson-Waldhauserova et al., 2014; Nakashima and Dagsson-Waldhauserová, 2019)."

L447 – "..elevated levels typically occurring during the drier summer months." Other studies show different long-term patterns in the frequency of dust storms in Iceland. In the southern half of Iceland, the highest frequency of dust storms occurs in late winter and spring, with the lowest in summer. Winter dust storms occur frequently in Iceland. Please consider the following works:

Nakashima, M. and Dagsson-Waldhauserová, P., 2019. A 60 Year Examination of Dust Day Activity and Its Contributing Factors From Ten Icelandic Weather Stations From 1950 to 2009. Frontiers in Earth Science 6, 245-252. DOI:10.3389/feart.2018.00245

Dagsson-Waldhauserova, P., Arnalds, O., Olafsson, H., 2014. Long-term variability of dust events in Iceland. Atmospheric Chemistry and Physics 14, 13411-13422. DOI:10.5194/acp-14-13411-2014.

We have rephrased this section to state the importance of dust storms as a year-round PM source, with significant seasonal and regional variability: "The dominant non-volcanic PM source in Iceland is natural dust from highland deserts, with dust storms occurring frequently throughout the year with significant regional and seasonal variability (Butwin et al., 2019; Dagsson-Waldhauserova et al., 2014; Nakashima and Dagsson-Waldhauserová, 2019).

L 452 – There are already an existing study dealing with PM1 proportions during Icelandic dust storms and the reported values differ substantially from yours. See here:
Dagsson-Waldhauserova P, Magnusdottir AÖ, Olafsson H, Arnalds O 2016. The spatial variation of dust particulate matter concentrations during two Icelandic dust storms in 2015. Atmosphere 2016 7, 77.
Dupont, S., Klose, M., Irvine, M., González-Flórez, C., Alastuey, A. Bonnefond, J.-M., Dagsson-Waldhauserova, P., Gonzalez-Romero, A., Hussein, T., Lamaud, E., Meyer, H., Panta, A., Querol, X. Schepanski, S. Vergara Palacio, Wieser, A., Diez, J., Kandler, K., and Pérez García-Pando, C., 2024. Impact of dust source patchiness on the existence of a constant dust flux layer during aeolian erosion events. Journal of Geophysical Research: Atmospheres 129(12), e2023JD040657

We thank the reviewer for pointing this out and have revised section 3.2: "Different PM_1/PM_{10} ratios (~0.4–0.5) were reported for two dust storms affecting Reykjavík in 2015 (Dagsson-Waldhauserova et al., 2016), suggesting variability among these events and the need for further research.".

The Dupont et al. (2024) study is a very important contribution to our understanding of Icelandic dust storms; however, we believe a direct comparison between PM size fraction ratios is not appropriate because their measurements were taken at dust-source locations, whereas our data were collected hundreds of kilometers away in populated areas.

L 452 – It is excellent that you compare your ratios to Icelandic dust storms, which are an extremely important source of PM in Iceland, but there are no permanent measurements around the local deserts of an area of 44000 km2. However, you state here you used the summer period due to eruption timing but then you chose two storms in November? These storms are not even visible on satellite images. Why not use clearly detectable dust storms instead? The ratios would likely differ.

We thank the reviewer for this valuable suggestion. In the revised manuscript, we have included analysis of two dust storms that occurred during the summer of 2021 (24–29 May and 3–4 June), aligning with the eruption period. Data from these events are now incorporated into Figure 9 (which supplements Table 3 in the revised manuscript) and Table A1 (formerly Table 3).

We have also expanded the discussion in Section 3.2 to include these storms:

"Some of the highest PM_{10} and $PM_{2.5}$ peaks in Reykjavík capital area (G3) during the eruption occurred on non-plume days (Fig. 4), notably in the periods 24–29 May and 3–4 June 2021. These two events accounted for most threshold exceedances— for example, five of seven for PM_{10} and four of six for $PM_{2.5}$ at station G3-A—and were recorded across all G3 stations, suggesting a diffuse distal source. The dominant non-volcanic PM source in Iceland is natural dust from highland deserts, with dust storms occurring frequently throughout the year with significant regional and seasonal variability (Butwin et al., 2019; Dagsson-Waldhauserova et al., 2014; Nakashima and Dagsson-Waldhauserová, 2019). We used back-trajectory analysis (HYSPLIT) and crowd-sourced observations to confirm that the PM_{10} and $PM_{2.5}$ peaks in Reykjavík on 24–29 May and 3–4 June were consistent with dust storms (Fig. A14)."

L 485 – These 'sharp edges´ are also often identified during dust storm measurements in Iceland. Great that you are emphasizing it here.

Very interesting observations; more research into the dispersion dynamics is clearly warranted.

L 672 – Is this (PYRO-Box, Eco Counter, 2021) also a reference?

We have improved the formatting to clarify that this is an instrument reference rather than a literature citation. The revised sentence reads: "These counters (PYRO-Box by Eco Counter) have a reported accuracy of 95% and a sensing range of 4 meters."

L 724 - 'Iceland's exceptionally dense network'. I would argue here that the Icelandic AQ network is insufficient outside urban and industrialized areas. Thus, Iceland may be incorrectly described as one of the cleanest-air countries in the world. Please clarify by specifying: "...in the Capital Area and Reykjanes Peninsula," so it does not imply coverage of the entire country.

We have changed the phrasing according to the reviewer's comment.

Reference list missing references: Janssen et al., 2013; McDonnell et al., 2000; Tomášková et al., 2024

We apologise for this error. The missing references have been included.