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.

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 SO2 concentrations 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 a high/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 3.3 are particularly problematic. Please see comments, below.

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.'

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.

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.

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

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

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

8. Section 2.2 line 178 – 'PM was not monitored with this network due to costbenefit 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.

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.

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.

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.

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.

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.'

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.

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!).

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).

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.

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.

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).

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

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

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').

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.

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.

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.

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.'

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

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

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.