

## ***Interactive comment on “Detecting volcanic sulfur dioxide plumes in the Northern Hemisphere using the Brewer spectrophotometer, other networks, and satellite observations” by Christos S. Zerefos et al.***

### **Anonymous Referee #1**

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This paper investigates Brewer measurements following 5 volcanic eruptions since 2005 searching for an so<sub>2</sub> signal in columnar spectrophotometric measurements. In all volcanic eruptions shown, a signal is found at the right time for the stations determined to be under the plume, based on trajectory modeling. In comparison no such signal is observed in those stations which are not under the plume based on the trajectories. By itself this is reasonably convincing of the reliability of the Brewers to detect so<sub>2</sub> plumes from somewhat significant volcanic eruptions. This results is, however, tempered by the noisy so<sub>2</sub> signal from the Brewers, and by difficulties in corroborating the Brewer so<sub>2</sub> signal using satellite data, particularly the OMI data. The authors consistently

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overlook this difficulty, making only a modest attempt to explain it. But then in the final case considered there is clear agreement between the Brewers, GOME-2, and OMI. The authors seize on this agreement, which is already clear, and perform a Pearson's statistical test leading them to claim in the conclusions, "The comparison with satellite measurements shows statistically tested agreement between the Brewer network and collocated measurements of columnar SO<sub>2</sub> from OMI and GOME-2." But in fact this only applies to one of the eruptions considered, and if the same approach were used for the other four comparisons the conclusion would be significantly different. The authors need to temper their conclusions significantly and be more careful in explaining the comparisons.

While overall the results will be useful and the paper should be published there are some serious deficiencies which must be addressed. The overall tone in various places is disturbingly confident of the so<sub>2</sub> measurements when there are glaring issues. The authors make claims starting with the first sentence of the abstract and ending in the conclusions (with the statement above), which are not correct. In addition there are several other serious issues which should be addressed.

The most serious issue is why there is such poor correlation of the satellite data, particularly OMI, with the Brewer data for 4 out of the 5 eruptions compared, and then such good agreement with Kasatochi? Was there something different about Kasatochi? When there is such poor agreement I don't see the point of quoting averages of the satellite data which appear to this reader to be in the noise of the measurements.

As the authors are aware, and I have become aware, the problem with so<sub>2</sub> columns from Brewers is where to set the zero point. There appears to be a lot of noise around this value leading at times to negative so<sub>2</sub> columns even in the satellite data. What do these negative columns mean? Can the authors provide a rough explanation of how to interpret them? Are they real or noise about a baseline of zero? Is a baseline determined for each of the instruments used?

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On a similar theme, the non-perturbed baseline from, for example, the European Brewers has various values. The authors quote 0.46 DU as the mean of all the examined bi-monthly Brewer stations in contrast to means of -0.02 for OMI and 0.09 for GOME-2. How should this 0.46 DU be interpreted? Is this real or a bias in the data? If a bias should it be, or is it, subtracted? Do the authors know how the baseline is handled from their individual data sources and does it matter?

Considering Fig. 5, the 0.46 offset to the Brewer data looks reasonable as that appears to be about the non-perturbed baseline in all stations. But then in Fig. 10 for Taipei, the baseline appears in the 2-3 DU range and again OMI shows no correlation with the Brewer. The 0.46 DU offset also applies to Figs 14 and 15 b). In contrast in Fig. 15 a), c) the average is much closer to zero. What changed for these stations outside the plume, whereas within the plume the offset appears? Finally in Fig. 16 the offset appears to be near zero for the US/Canada and Taipei stations.

Aside from these major questions I provide the following specific comments, some provide more specifics on the themes above.

1.41-42. Have increased compared to what? That so<sub>2</sub> columns increase following somewhat large volcanic eruptions is not new and has not depended on this paper to show that. Nor is it new that such columns increased following the five eruptions considered here. This sentence needs to be rephrased or deleted. I would begin the abstract with something like.

Following the five largest volcanic eruptions of the past decade in the Northern Hemisphere, a strong positive SO<sub>2</sub> signal was detected by all the existing networks either ground based (Brewer, EARLINET, AirBase) or from satellites (OMI, GOME-2). This study particularly examines ...

But after reading the paper even this sentence has issues. A strong signal was not detected in OMI and GOME-2 data according to the results shown here in several cases. Thus the statement that a "... a strong positive SO<sub>2</sub> signal was detected by

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all the existing networks either ground based (Brewer, EARLINET, AirBase) or from satellites (OMI, GOME-2) ..." is not correct for the satellite data for all cases.

1.41. Why are the increases described as significant? Significant in what way? The so<sub>2</sub> increases following Pinatubo and El Chichon were significant, but these are on a different scale than the eruptions considered here.

1.45-47. This statement is incorrect for the reasons given above, particularly for OMI. The correlation is better for Brewer and GOME-2, but I doubt even this would be statistically significant at the level claimed if all cases were considered. See Figs. 5, 12, 15. Again how are the columnar so<sub>2</sub> amounts significant? What do the authors intend to imply with this word?

3.9-14. The authors need to be more careful about their claims concerning the "five" volcanic eruptions. In the abstract it was the 5 most significant eruptions since 2005. Now here it seems to be the five eruptions which produce the most so<sub>2</sub> over Iceland, but only 4 eruptions are shown. Not surprisingly 3 of these eruptions were in Iceland, although most of these eruptions are not on the list of the 5 eruptions since 2005 with the greatest atmospheric impact. Here the sentence needs to indicate up front that these are selected based on their so<sub>2</sub> columns over Iceland. So ... Five cases of high SO<sub>2</sub> over Iceland from volcanic ...

Yet this sentence goes on to say that these are the five eruptions to be compared in this study. So I am confused, are the eruptions the 5 most significant since 2005 or the 5 with most significant so<sub>2</sub> over Iceland. According to the Smithsonian Global Volcanism Network, Bárðarbunga has a VEI of zero, so undetermined.

Table 1. It is significant that 4 of the 5 eruptions are at high northern latitudes, while the lone tropical eruption had its plume picked up in the Asian monsoonal circulation to bring the so<sub>2</sub> over Europe, so an important but poorly stated criteria seems to be the opportunity to measure the plume over Europe.

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Clearly there is enough confusion here that the authors need to rethink the criteria used for the selection of the 5 eruptions and to explain it clearly.

4.13-21. Confusing. I had to re-read this several times. First the authors state . . . the Brewer spectrophotometer is additionally used to derive the SO<sub>2</sub> column. . . Then they say . . . The existing Brewer network could deliver frequent SO<sub>2</sub> measurements as well, but the Brewer instruments are less able to accurately provide SO<sub>2</sub> measurements . . . So which is it? Don't claim that it is used and then say it can't be used. Please rewrite this to be clear.

7.2-4. Doesn't this also suggest a bias for the Brewer data?

7.35-36. From Fig. 5 only the GOME-2 measurements corroborate the Brewer results, but even then only in timing, not in magnitude. Is there an explanation why no signal appears in OMI data and why the Brewer and GOME disagree in magnitude to the extent shown?

8.23-30. Aside from GOME-2 it seems pointless to quote these numbers for OMI. The OMI data do not indicate anything out of the ordinary for 20-25 September, neither the TRM or PBL. In fact there are bigger excursions of the SO<sub>2</sub> column at other times. The GOME-2 data are better and a case can be made that some SO<sub>2</sub> was observed, but even these data could be questioned.

8.33-35. Thus the statement, "In all cases, however, the observed . . . were always higher . . ." is simply incorrect, as demonstrated with the numbers just above, and should be removed.

9.1-5. Why is there so much inconsistency between Figures 5 and 7. Fig. 7 shows OMI measurements of 1-4 DU across large regions of Europe, yet Fig. 5 indicates almost all OMI measurements < 1 DU and most measurements < 0.5 DU.

Figure 9. The differences between the colored lines are not obvious.

10.15. What is meant by both methods?

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Fig. 15. Why is the Brewer baseline at 0.2-0.3 DU for the stations under the plume, whereas for the 10 outside stations the baseline is closer to zero?

11.38. Does an average SO<sub>2</sub> plume of 0.1 DU mean anything when earlier the averages of the Brewers without influence by volcanoes was on the order of 0.4 DU? It does not help the authors' argument to be calling out numbers in the text which are in the noise of the measurements. The authors also never explain what a negative DU measurement means. What causes this? Are the negative numbers a real measurement?

Fig. 16. Why is a 7 day running mean now added to the measurements? Does it show something missing in the simple averaged daily data shown up to now?

12.31-13.6. A calculation of Pearson's correlation coefficients is not necessary to convince the readers that the Brewers, GOME-2 and OMI are all in agreement at least over Europe. Is the Taiwan station included in the correlation coefficients? If so, does the fact that there is virtually no correlation there get masked because it is only one station? What is telling about this paragraph, and the corresponding Table 5, is that such tests were not used in any previous comparison, most certainly because the results would have been much worse, see Figures 5, 12, 15.

13.16-18. This statement is based on only the Kasatochi results and does not hold for 4 of the 5 eruptions studied, thus the statement either has to be removed from the conclusions or dampened considerably by pointing out all the other times when no correlation or a poor correlation was found.

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