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Interactive comment on "Volcanic SO_2 Effective Layer Height Retrieval for OMI Using a Machine Learning Approach" *by* Nikita M. Fedkin et al.

Anonymous Referee #2

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This paper describes the implementation of an existing machine learning algorithm to retrieve volcanic SO2 layer height from the OMI satellite instrument. The paper details initial promising results, and I would recommend for publication.

The paper is based on an existing algorithm (FP_ILM) which has been applied for SO2 layer height retrievals in other instruments (particularly Efremenko et al, 2017 and Hedelt, 2019.). While these are clearly reference in the introduction, it would be good to make clearer in subsequent sections (particularly Section 2: Methodology), which sections directly follow the earlier papers approach and reuse existing inputs, and which parts are implemented specifically for OM

It is also recommended that the authors address the following specific points:

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Discussion paper

Specific Comments:

Abstract, line 34-35: The last sentence of the abstract states “This approach offers a promising prospect of using physics-based machine learning applications to other instruments.” However, as this algorithm did not originate in this study or for the OMI instrument, and so has already been demonstrated for a number of other instruments, as phrased here the statement doesn’t seem valid. I would suggest the sentence is either rephrased (if that wasn’t it’s intended meaning), or removed.

Introduction, lines 99-104: The section describes IASI retrievals of SO2 height, and follows it with the phrase (‘For these techniques, extensive radiative transfer modeling is needed. . .). However, in the Clarisse 2014 paper referenced, this describes a fast retrieval scheme, where this statement may not follow. I would suggest checking and amending the text appropriately.

Section 2.2: In the first paragraph, there is a discussion on the impact of the SNR used, with reference to Table 3. First, I would suggest moving some of this discussion to Section 3, where the other performance impacts are discussed, as concepts used here, such as the split between training and test data are not explained until later in section 2. Secondly, it wasn’t clear to me what Table 3 represents. Is this the impact of applying the same varying SNR’s to both the training set and test set of data, or is it the impact of applying a SNR of 1000 to the training set and different SNR’s to the test set? (To me the text reads one way and the caption the other). If it’s the first case, have the authors also looked at the impact of applying the data trained with their chosen (better than reality) SNR to synthetic data which has had the realistic OMI noise applied? When the inversion is applied to real data, this will be the SNR level that the algorithm has to cope with, so would be a valuable indicator of expected performance.

Section 2.4 Line 264-266: “The output is a predicted SO2 layer height based on the input of a radiance spectra and associated parameters, including VZA, SZA, RAA, surface albedo and surface pressure, for a single OMI pixel.” Is this sentence in the right place – I found the flow of the paragraph a bit confusing, as it then jumps back to talking about convolving the irradiance spectra and then applying PCA?

Section 2.4, Line 278: The text assumes that readers will be familiar with the OMI row anomaly, which may not be the case – it would be useful to explain this somewhere.

Section 3: Tables 2 and 3 need more explanation in the text here e.g. RMSE is mentioned, but it's not explicitly stated what this represents anywhere. Also, from their captions, I would have expected the RMSE numbers in Table 2 for VCD > 40 and SZA < 75, to be the same as the RMSE in Table 3 for SNR = 1000. However, the numbers don't agree. What is the reason for the difference?

Section 4: What are the expected uncertainties of the validation data products used – the text talks about reasonable agreement, but there are differences of several km's in some cases, so it would be useful to know if that can be explained by uncertainties in the other datasets as well? In particular, for Kasatochi, the quoted values for prior OMI retrievals are a few km's lower – has the reason for this been looked at in more detail?

Figures 4, 5, 7: These would be clearer if all the instruments were plotted on the same colour scale and lat/lon range. Also, if possible, replot the Caliop data to focus on the relevant region. Similarly for Figure 8, it would be clearer if all the instruments were plotted on the same axes.

In Figures 7 and 8 for the Raikoke eruption, the distribution of values for OMI and TROPOMI seem to be mirrored e.g. OMI has a tail of lower values while TROPOMI has a tail of higher values. Is there an explanation for this?

Section 4.3 – the first paragraph reads as an introduction to section 4.2 too – should the ordering be changed?

Section 4.5 'Discussion of errors'. Have the authors looked in any more detail on the impact of some of their assumptions in the radiative transfer modelling on the retrieval errors? E.g. they mention that using a fixed solar irradiance spectrum will be less accurate than using the OMI solar measurements. Has the expected impact on this been quantified? Is a fuller assessment of these sorts of errors planned as part of their future work?

Conclusion: Line 472 “with absolute errors of up to 1.5km” – This seems to be the first time that number is quoted, and given the uncertainties and difficulties comparing instruments it may be too strong to put a hard number on an absolute error e.g. The section on errors just mentions 1-2km differences. I think if it’s quoted like this, it would be good to back it up with more quantitative information as to where it came from. Otherwise, I would rephrase. (Similarly, the abstract quotes errors of 1-1.5 km’s, which should also be made consistent).

Why are the figures 1A and 2A supplemental, as they’re directly referenced in the text?

Technical corrections:

- Some abbreviations need defining (line 61: VCD, line 93: BUV)

- Some of the figure numbering is wrong (Line 157, 159 – should reference fig 2a,2b,2c)

- Line 164 – ‘absorption’ repeated.

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