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Atmospheric Measurement Techniques Discussions

Interactive comment on "Development of a neural network model for cloud fraction detection using NASA-Aura OMI VIS radiance measurements" *by* G. Saponaro et al.

Anonymous Referee #1

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The paper "Development of a neural network model for cloud fraction detection using NASA-Aura OMI VIS radiance measurements" by Saponaro et al. describes a method to evaluate the cloud fraction in OMI pixels, based on artificial neural networks, OMI visible radiance measurements and MODIS cloud fractions (as reference outputs). The topic is potentially interesting for AMT readers. However, I have found that the quality of the manuscript is poor in many aspects and it may deserve publication only if the following major and minor comments are properly addressed. Otherwise, I don't recommend publication.

MAJOR AND GENERAL ISSUES

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Interactive Discussion
Discussion Paper

1) The authors base their discussion and their conclusions on the results over the training dataset only, except for one single validation with an independent orbit (Fig. 12). To me, it makes no sense to discuss the outputs of the training datasets, because it is only with an independent validation set that you can test the performances of the algorithm. So, in my opinion, Fig.s 3-11 are not really interesting. I suggest to select only a few Fig.s to report the performances on the training dataset (maybe Fig. 6, 8 and 9), if you really need them to discuss some aspects of your algorithm (the causes of large deviations with respect to MODIS reference data, e.g., as resulting from Fig.s 8-9). And, most important, the authors must provide more validation and discuss the performances of the algorithm based on the validation. The lack of a real validation is the main flaw of this manuscript and I recommend rejection if this aspect is not properly addressed in the revised manuscript.

2) Even if we consider the training dataset only, the results shown in, e.g., Fig.s 3, 4, 7 are not really encouraging. Values of 0-20% for the "Correctly detected pixels" are found for clear sky pixels on some orbits. It is worth nothing that you say that the selected orbits for the training are almost completely cloudy: you just need to chose other orbits to train the NN. So, I recommend to reconsider the training dataset to include more representative orbits and/or to reconsider the design process of the NNs to obtain better performances on the training dataset.

3) The authors need to give more details on the singular value decomposition of the input OMI spectra. Why did you choose 20 eigenvalues? I recommend to add a figure showing the eigenvalues as a function of the eigenvectors to show that cutting off at 20 is an optimal choice.

4) I suggest to modify the title to "A neural network model for cloud fraction detection using NASA-Aura OMI VIS radiance measurements".

5) The definition of MODIS data as "reference inputs" of the algorithm is very confusing. Indeed, the MODIS data are "reference outputs". Please correct throughout the text

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and modify Fig. 2 accordingly.

6) The manuscript is not well organized and very hard to read and understand. The written English needs to be improved. In the following, I suggest some very obvious corrections, but I recommend to ask an English speaker to copy-edit your manuscript. Please make a particular effort to clarify the introduction and the conclusions, as I found them very confusing.

MINOR COMMENTS

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1) L3: "visual"->"visible"

2) L5-7: the sentence "Also...atmosphere" is not clear. You want to say "detection" or "discrimination" instead of "distinction"? "Essential for" instead of "Essential to"?

3) L8: "from NASA-Aura Ozone Monitoring Instrument (OMI) observations"

4) L9: "mathematical" seems not pertinent here, please remove

5) L9-10: "simultaneous application to OMI and Aqua-MODIS data" is very confusing. You wanted to say that OMI and MODIS are inputs/(reference)outputs of the NN? Please rephrase

6) L12: remove "approach"

7) L12: "Highly reflective..."->"However, highly reflective..."

8) L18-19: "...(TOA) reflectance..."->"...(TOA)...", as "reflectance" is said twice

9) L21: "...are discarded.", you need to add a reference here

10) L21: "Commonly"-> "Usually"

11) L21: "...is performed using several tests.", you talk about the methods described at L23-etc? Please clarify

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12) L25: "...with information from..."->"...with additional information coming from..."

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13) L1: "...from large quantities..."->"...from a large quantity..."

14) L2: "...task and, in addition..."->"...task. In addition..."

15) L3: "efficient" doesn't seem the proper word here. You mean "fast"?

16) L6: "...on OMI cloud screening..."->"...on the cloud screening of OMI observations..."

17) L7: you talk about thermal channels: why? You wanted to say that TIR channels are useful for the cloud detection? Please explain and possibly reference to existing literature

18) L9: what do you mean with the word "combines"? Please explain

19) L13: "In this work we propose...", eliminate "In this work" and don't start a new paragraph

20) L14: "VIS" (visible) is not defined. It has been defined in the abstract but it needs to be defines also in the main text

21) L15: you use "OMI" at L6 but you define the acronym only here

22) L18-19: "AQUA"->"Aqua", "AURA"->"Aura"

23) L23: the paper Sellitto et al., 2012 doesn't talk about the prediction of atmospheric parameters. As the authors can easily see from just the title, it talks about ozone retrievals, so please put it at L25, with the other ozone retrieval references

24) L25-26: the references Del Frate et al., 2002,2005a, and Iapaolo et al., 2007 are not the most recent papers on ozone retrievals with NNs. Indeed, here are two references of NNs algorithms to invert OMI radiances that are more relevant for your paper and need to be referenced:

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Sellitto, P., Bojkov, B. R., Liu, X., Chance, K., and Del Frate, F.: Tropospheric ozone column retrieval at northern mid-latitudes from the Ozone Monitoring Instrument by means of a neural network algorithm, Atmos. Meas. Tech., 4, 2375-2388, doi:10.5194/amt-4-2375-2011, 2011

Di Noia, A., Sellitto, P., Del Frate, F., and de Laat, J.: Global tropospheric ozone column retrievals from OMI data by means of neural networks, Atmos. Meas. Tech. Discuss., 5, 7675-7727, doi:10.5194/amtd-5-7675-2012, 2012

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25) L2: eliminate "task"

26) L7: Please specify that MODIS data are taken as "truth reference" or "reference output"

27) L8: if the MODIS pixel is smaller, its spatial resolution is higher than OMI, not smaller. Please correct

28) L8-10: the sentence "Hence the OMI...real OMI data." is not clear. Please rephrase

29) L11: "...the adopted design for the cloud..."->"...the design of the cloud..."

30) L13: "employed"->"used"

31) L14: eliminate "ones"

32) L16: eliminate "separated"

33) L19-20: the authors say "The use of only 4 orbits is not sufficient...": so why you don't use more orbits to train the NNs?

34) L20: what do you mean with "good" solution? Maybe you should mention the concept of "generalization"

35) L24: "...also for the TROPOMI/VIIRS one without..." ->"...also with TROPOMI/VIIRS without..."

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36) L24: please define the acronyms "TROPOMI" and "VIIRS"

37) L24: your OMI/MODIS NN will not work with TROPOMI/VIIRS. You would need to re-train the NN with different data and maybe also change some other aspects of the design. From my point of view, the only thing in common for the two algorithms is that they both are NNs...

38) L25: "...the added benefit of the oxygen a-band..."->"...the added benefit of the presence of the oxygen a-band..."

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39) In the description of OMI it seems that OMI only takes measurements in the VIS. Even if only VIS measurements have been used in this context, the authors need to state that OMI has also two UV channels (UV-1: 270 to 314 nm, UV-2: 306 to 380 nm)

40) L3: why not using the acronym "OMI"?

41) L5: "...in the visible parts of the electromagnetic spectrum..."->"...in the VIS..."

42) L5-6: "...is 13x24 km2 at nadir."->"...is 13x24 km2 at nadir, in the normal global operation mode." (There is also a "zoom mode")

43) L8: "Row Anomaly"->"row anomaly"

44) L8: put a reference for the row anomaly

45) L10: "earth"->"Earth"

46) L10-11: "...is MODIS which..."->"...is MODIS, which..."

47) L13: "...7 minutes the MODIS..."->"...7 minutes, the MODIS..."

48) L18: maybe "variables" is better than "parameters" here

49) L21: I'm not sure that "to detect" is pertinent here, maybe "to measure" is better

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50) L1-2 "...is considered here which is referred to as multilayer perceptron..."->"...is the multilayer perceptron..."

51) Fig. 1 (caption): "...feedforward topology..."->"...feedforward structure..." (the figure is general, while the topology depends on the number of inputs/hidden/outputs neurons, for your specific problem)

52) Fig. 1 (caption): x_n actually represents the last neuron, not the generic one (I would use another index, e.g. "i" to refer to the generic index), in this figure. Has this figure been produced by the authors? Otherwise please cite the reference of the source.

53) L4: "...comes in at the..."->"...is collected by the..."; you can also eliminate "of the network" (it is obvious)

54) L7: "into"->"to"

55) L8-11: the sentence is very confusing, please rephrase. In general, the authors never talk about the "neuron" (they use the word "unit") or "activation function", which is very confusing

56) L12-13: with "the model of each node" you mean "the activation function of the neurons"?

57) L15: "...neural networks theory..."->"...neural networks..."

58) L16: "...multilayer perceptron network..."->"...multilayer perceptron..."

59) L18: "...back-propagation, algorithm..."->"...back-propagation algorithm..."

60) L26-28: if you say "The training phase of a NN...", the sentence seems more general than a specific discussion on the back-propagation. The authors wanted to be more general here?

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Interactive Comment

61) L9: this sentence is very general: in this subsection weren't you specifically talking about the back-propagation training algorithm? (and here you finally mention the "activation function", but without defining it)

62) L10: "adopted"->"used"

63) L12: define symbols of Eq. 1

64) L13-17: now you're talking about your algorithm? Is subsection 3.1 a general description of the back-propagation learning algorithm? I personally think that this subsection must be completely reconsidered

65) L23: "Extreme"->"extreme"

66) L25: I missed where the authors introduced the "bias term" in the general description of the NNs

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67) L3: you use again "activation function", which is not defined

68) L5: again, the authors need to define the biases before (when they describe the general concepts of NNs)

69) L14: "provides"->"requires"

70) L14-16: you say that the extreme learning machines reach smaller errors and are faster, so why using the back-propagation?

71) L18: "resolved from"->"found with"

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72) L2-4: the sentence "The drawback...randomly" is not clear. What do you mean with "scaling parameters"? Please clarify

73) L9: "observation uncertainties" seems not proper here, since you don't use uncer-

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tainties as inputs of the NN

74) L17: "unity"->"one"

75) L17: calculating the reflectances does not assure to fully exploit the dynamical range of the input neurons (0-1). The authors should report the maximum and minimum value for the spectral inputs or rescale the inputs between 0 and 1.

76) L22-23: do you mean that the small-pixel data is provided at one single wavelength?

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77) L17-18: the authors don't mention at all the beneficial effect of a dimensionality reduction for the generalization capability of a NN (limitation of the overfitting effect and less local minima in the mapping function). This is important and must be discussed

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78) L11: what do the authors mean with "initial testing"?

79) Sub-section 4.3: please specify that the MODIS cloud fraction data are reference "outputs"

80) L14: you may want to say "...to be matched with the methodology described by Stammes et al. (2008)"

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81) L8-9: isn't the sentence "The neural...Fig.1" a repetition?

82) L14: why "Extreme Learning Machine" has initials in uppercase?

83) L17-18: you say "For the back-propagation algorithm, 25 hidden nodes guarantee a good performance either in terms of training accuracy or training time", so you tested bigger NNs, with more hidden neurons? So the training time is not an issue? Trade-offs of hidden neurons is usually searched based on the accuracy and the generalization

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capability, by analyzing the results over an independent dataset (usually referred to as "test dataset", not to be confused with the "validation dataset"). I recommend to discuss the choice of the hidden layer dimensionality in terms of these quantities and not the training time.

84) L22: why "Extreme Learning Machine" has initials in uppercase?

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85) L1-10: there are lots of repetitions. Please be more concise

86) L11: "Data was..."->"Data were..." ("data is plural")

87) L14: the authors talk about the results of Fig. 3 but they don't describe the figure in the text. Please first introduce the figure and then comment it

88) Fig. 3: as said in my major revision 2), the algorithm does not work (e.g., 0% clear sky detected pixels for the third orbit).

89) L15: "observed"->"considered"

90) L22-24: the authors say "This can be explained by considering the chosen orbits: most of the pixels are fully covered by clouds, thus not enough information is provided to the NN for the training." To me this statement sounds strange. Cloud free and cloudy pixels are complementary (if a pixel is cloudy it is not cloud free, and viceversa), so if you have information on cloudy pixels you have also information on cloud free pixels. Please explain or correct your statement

91) L26: "...analysed the same data but trained the learning algorithms with separated..."->"...analyzed the outputs of similar NN trained with separated..."

92) L28: "efficiency" seems not pertinent here

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93) L2-5: the authors say that worse performances over ocean than land are unex-

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pected, and I agree. In any case, please attempt to investigate or at least to propose possible reasons for this unexpected behaviour

94) L6-7: I think that the sentence "To supply...phase" is a repetition

95) As said in the major revisions 1-2), do you really need to show all your tests if only this latter NN, with 60% threshold, works?

96) L13-14: "fully clouded", you mean that there not even 1 pixel with CF<100%? Maybe histograms with MODIS reference cloud fractions would help in this context

97) L14: put a space between "representative" and "for"

98) L16: "ground pixel coverage type" seems not the best way to define your "Land/Water" flag

99) L18: "predicted"->"estimated"

100) L20-21: "The color scale...points", is it the number of points or the density?

101) Fig. 6: please annotate the units next to the colorbars, here and in every figure

102) Fig. 6 (caption): again, "predicted"->"estimated"; "A good correlation...and ocean", this must be said in the main text and not in the figure's caption

103) L26: the authors talk about biases, but where are they reported? Not in the figure, as they claim here

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104) L18: delete "signifies"

105) L23-24: you actually don't show "good performances" in Fig.s 10-11, but "MODIS geometrical cloud fraction...etc...for two orbit characterized by better performances than those shown in Fig.s 8-9..." or something similar (please explain the figures in the main text)

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106) Fig. 11: there are regions with about 100% mismatch, so this is not characterized by "good performances" over the whole orbit. Please clarify and attempt to explain for these marked mismatches

107) Fig. 9: please change the extreme values of the colorbar (c) to 0-100, as done for Fig.s 8, 10 and 11.

108) Fig. 8: "...MODIS image shows the presence of dust as the reason of failure...", actually MODIS shows the presence of dust and you suppose that this is the reason of mismatch (that's not shown by the MODIS image, it's your supposition): please rephrase accordingly

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109) L2-3: "The resulting predicted cloud fraction is rather inaccurate", so the only validation you provide shows that your algorithm doesn't work/doesn't generalize from the training?

110) L9-10: "However...Fig. 12", the sentence is not clear, please explain better

111) L14-17: in my opinion, the statement "Moreover, they rely on auxiliary data only during the training and they are independent from the instrument platform which makes the approach portable to other combinations of instruments such as TROPOMI/VIIRS" is not true, because a NN for TROPOMI/VIIRS will need re-design and re-training. Please modify or eliminate the sentence

112) L18: this is not a "comprehensive" study. I would say, on the contrary, that this is a "preliminary" study

113) L23-25: you say more than once that the back-propagation is extremely time consuming with respect to ELM: please quantify

114) L25: you say that all the orbits are almost totally cloudy, again, so why not using other orbits?

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115) L3-5: you claim that "the spectral features alone can discriminate cloudy from clear pixels", but this is not true, because you show that cloud free pixels are often not correctly detected

116) L10: "learning"->"training"

117) L9-12: your description of future work is somewhat mysterious... If you wish to talk about it, please give more details

118) L25-26: again, this is not really portable to TROPOMI: you'd need to re-design and to re-train the NN. In addition, this sentence is a repetition: please delete

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119) L1: please give some more details on how do you want to use the oxygen A-band in your future work with TROPOMI

120) L5-7: "These demands are outside the scope of the method description and initial testing described here but they will be addressed in future testing and validation", I strongly disagree on this statement. In my opinion, if you don't provide a validation of your NN and you don't demonstrate its generalization to data not present in the training dataset, your work is not at publication level.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 1649, 2013.