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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Satellite retrieval of aerosol microphysical and optical parameters using neural networks: a new methodology applied to the Sahara desert dust peak" *by* M. Taylor et al.

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We are grateful to Dr Noia for kindly posting a review in addition to his interactive comment posted during the public discussion which helped to improve the initial presentation of the new methodology we propose as well as the interpretation of the findings. Here, we formally reply to Dr Di Noia's additional review:

"Dear Authors,

I would like to thank you for the detailed answers to my previous comments, and for taking the effort of producing additional statistics and graphs to support your statements. Overall, I think this study is interesting and your proposed modifications have the potential to improve the manuscript, but based on your reply I think that there are still some points that would benefit from some further clarification. As I will further detail in this report, I think that the paper presents two fundamental critical areas:

1. Since all the data used to quantify the performances of the algorithm were – in a way or in another, as we will see later – involved in the specification of the NN algorithm, we are left without a convincing demonstration that the algorithm really works when applied to a "random" scenario over the Saharan region. My opinion is that such a demonstration should be included in this paper, because this is part of the work that has to be done when a new methodology to perform a given task is proposed. 2. Regarding on the discussion of the performances of the NN with respect to each aerosol parameter, I see the risk that some of the statistics shown may be overinterpreted."

We would like to thank Dr Di Noia for acknowledging that we have produced additional statistics and graphs that support our statements, and that he finds our study interesting and that our modifications have shown a potential for improving the manuscript. We are happy to provide additional clarifications. With regard to point 1), as mentioned in the manuscript and clarified in our response to Dr Di Noia's first interactive comment, the data at the test site Dakar was not used during NN training and therefore was not part of the specification of the NN algorithm. It was reserved purely for random testing. With regard to just how random this site is in the context of the whole Northern Africa region is an interesting point raised by Dr Di Noia and we believe could form the basis on a future in-depth study of the Sahara dust distribution in the area using for example a synergy of global circulation model outputs and AERONET data. Such a large study is beyond the scope of our present study whose aim is to present and evaluate the methodology at a single site in the region of the Saharan dust peak whose aerosol properties are coherent and well understood. In response to point 2), we agreed with

Dr Di Noia's interactive comments and we have changed the presentation of the statistics accordingly.

"Furthermore, I think that the presentation of the results can be improved in at least two respects:

1. In the introduction it should be made clear why MODIS has been chosen for the development of this method, despite the fact that other satellite instruments exist that are known to be better suited than MODIS for the retrieval of the aerosol microphysical properties. 2. Some of the statistics shown in the original version of the paper might perhaps be omitted, because they do not add much to the presentation of the method."

We would like to thank Dr Di Noia for suggesting these two improvements in the context of the presentation of the results. Regarding point 1), we agree and have added the following sentence in the introduction on Page 10958, Line 18 justifying our choice of MODIS for the development of this method:

"In this work, gridded (1x1 degree) data from operational MODIS and OMI instruments was used in order to exploit a long 9-year period of data overlap with AERONET measurements."

Regarding point 2), we respond at the corresponding part of Dr Di Noia's review where the specific statistics are referred to below.

"MAJOR ISSUES 1. Model selection. In your reply to my previous comment you mention that you chose train+validation MSE as the criterion to optimize the NN architecture because it led to a better performance on the Dakar test site with respect to using the validation MSE. If I have correctly understood your reply it seems to me that you performed the model selection exercise twice. Initially, you used the common validation MSE as a metric, which led to a certain solution. Then, you arbitrarily performed the exercise again by using training + validation MSE, and this led you to another solution. Then, you decided that this latter solution was the better one, because it led to

a lower MSE on Dakar. But isn't this equivalent to saying that you chose the best NN architecture by minimizing the MSE on Dakar? If this is the case, I would say that this approach, even with its limitations (Dakar is only a single site, and obtaining the best performance on this site does not necessarily mean that the same would be the case anywhere), is more fair than including the training MSE in the metric for the model selection. However, this also creates another problem. If also the Dakar data have been involved in the NN selection, as appears from your reply, then I do not see any validation of the algorithm on data that have been neither used to train the net nor to optimize its architecture. Therefore, I would suggest you to perform at least one of the following additional tests, that I would not postpone to a future paper:

(a) Validate your NN on another Saharan site that has not been used for training or model selection, if you still have some data available (b) Use a CTM to select a number of different situations (at least 2) that have occurred over the Saharan region in the past, and invert the corresponding MODIS imagery using your algorithm, comparing (even only qualitatively) the spatial distributions of some of the retrieved aerosol parameters to those predicted by the CTM. Are the resulting spatial patterns reasonable? In my opinion, only this type of test can lead us to substantial conclusions about the general feasibility of the proposed approach."

We are grateful to Dr Di Noia for this detailed comment. However, his interpretation is not correct. While Dr Di Noia is correct in reiterating that best results were obtained by using a combined training + validation error (as we explained in some detail in our reply to his interactive comment regarding the choice of statistic used to identify the optimal NN from the grid), he has mistakenly connected this with testing at Dakar. During the training phase, the NN was presented with data from Northern Africa sites (excluding Dakar). As such, Dakar data was not involved in the NN selection. To make sure that this point is clear to the reader, we wish to reiterate that the combined training + validation error gave the best results with regard to validation of the training data (i.e. comparison between the NN model outputs with AERONET inversions on the non-

Dakar dataset). The same NN was then applied to Dakar as an independent test case. It is important to repeat here that the combined training + validation error was not used because it gave the best results at Dakar, but because it gave the best results on the training dataset from which it was calculated. In short, the NN performance over the whole region is captured by 2 measures: 1) the combined training + validation error for the training dataset (excluding Dakar), and 2) the CASE 4 NN comparison statistics (the median absolute error as a measure of dispersion and the use of the Pearson's as a measure of correlation) at Dakar. We believe this is an adequate proof of the validity of the method, at least in the region of the Saharan dust peak in Northern Africa. Regarding Dr Di Noia's suggestions a) and b), choice of a different test site will mean a new partitioning of the Northern African dataset. By this we mean that if, for example, we retained Agoufou as the test site, then the training dataset would exclude Agoufou data but would include Dakar. The results would be slightly different to those presented here due to the fact that the mean values would be slightly different. One could argue that to be even more thorough and pedantic, such a process could be repeated (N-1)! times where N is the total number of AERONET sites in the region and then the results of this bootstrap approach could be analyzed for self-consistency. We did not do this in this work for two reasons: 1) to avoid a lengthy and repetitive analysis and 2) because we believe that this interesting idea which is beyond the scope of the present work, could be a useful concept for a separate study by itself. Similarly, in response to suggestion b), in our reply to Dr Di Noia's interactive comment we presented initial results from work in progress where we applying this methodology to global clusters of AERONET sites (with a view to investigating application of this approach on the global domain and in synergy with chemical transport models). However, this is the subject of a separate work in progress to be submitted shortly to another journal.

"2. As I suggested in my previous comment, you tested the mean absolute values of the NN-AERONET differences against the same statistic for the difference AERONET training set mean. The table shows that the mean absolute (NN-AERONET) difference is lower than the mean absolute (AERONET-mean) difference for all the aerosol pa-

rameters. Ok, but how significant are these differences? It must be kept in mind that the AERONET estimates of the aerosol microphysical parameters are not error-free. For instance, in the case of desert dust aerosols, the standard error on the real part of the complex refractive index is quantified by Dubovik et al. (2000) in 0.04 for AOD(440 nm) larger than 0.5 (and presumably larger for smaller AODs), whereas for the imaginary part a relative error of 50% is reported. If this is the case, then I would doubt that, for example, the numbers you report for CRI-R(440) – 0.041283 vs. 0.040864 – are indicative of a real difference between estimating CRI-R(440) using the NN and estimating it using its average value on the training set. A similar line of reasoning applies to many of the retrieved aerosol parameters, the only noticeable exception being the coarse mode volume, for which it is clear that the NN estimate gives us more knowledge on this parameter than its simple average on the training set does. In other words, it seems to me that many of the aerosol parameters are not really retrieved by the NN, and this fact also relates to what I wrote in my previous comment about which aerosol parameters should be included in the NN output vector: my opinion is that, if you include all these parameters in the output vector and deliver them as a product without any sort of flagging, a data user might erroneously think that their values reflect the evidence of a real situation (e.g. spatial distribution of refractive index, or SSA, etc.) going on over the Saharan region, while perhaps they are not much different from random variation around a "climatological" mean. Maybe in this paper you should leave the output vector as it is, but I would at least suggest you to present this work more clearly as an exploratory investigation aimed at verifying which aerosol parameters can be inferred from MODIS AODs over the Saharan region and which aerosol parameters cannot."

After giving some thought to it, we agree that we need to emphasize in the paper that the satellite retrieval provides only a moderate estimate of the total concentration of the coarse mode of dust. As such, we proceeded with an overall review of the text to ensure that this point has been made consistently throughout. With regard to Dr Di Noia's suggestion for the use of "flagging" to help end-users, we would like to remind the reader that this is precisely the point behind our construction of Table 7 and our attempt at specifying some categorical break-points: “Very Poor”, “Poor”, “Moderate”, “Good” and “Very Good”. The exact break-point values have been subjectively chosen and have been revised in the text and in Table 7 of the revised manuscript. Dr Di Noia’s suggestions relating to such categorizations under “MINOR COMMENTS” are correct and we have incorporated these changes in the revised manuscript (please see below).

“PRESENTATION ISSUES 1. I think that in general it is fair to show how using additional information in the NN input vector improves the retrievals. However, I would suggest to restructure the paper so as to emphasize in advance that this “empirical sensitivity analysis” is one of actual goals of this work. But on top of that, is the full analysis of the CASE 1 NN (with all the statistics shown in Tables 2 and 3) really informative for the reader? Is it not trivial that trying to estimate 7 uncorrelated quantities from only one input variable cannot lead to useful results? My personal opinion is that discussing the results of the CASE 1 NN in detail distracts the attention of the reader, because poor performances are, in a sense, to be expected in advance for such a NN. Would it not be enough to say that a PCA applied to the AERONET AODs only gave rise to a significant principal component, and that this is clearly inadequate to perform the task you are aiming at? Why go ahead, train a dedicated NN for such a case and discuss its results? I must say that also the best NN, that should be the CASE 4, suffers from this problem, but at least in that case you have 3 uncorrelated components in the input, so in that case the discussion of what type of information about the aerosol parameters you can retrieve becomes more interesting. I think that also for that case, the scope of your experiment should made clearer in the paper, by explaining that there is no hope to retrieve all the 7 principal components of the aerosol parameters from a neural model that is driven by only 3 uncorrelated (that does not necessarily mean independent) inputs. With respect to that, it might also be interesting to see what would happen if you apply a PCA to the set of the aerosol parameters retrieved by the NN, and compare the number of significant principal components (perhaps using the same criterion of 98% variance) to that of the AERONET values (7). I have never tried this

myself, but I suspect that this number should be less than or equal to 3, and should give you a rough estimate of the actual "degrees of freedom" in your retrievals (i.e. how many parameters, or combinations of parameters, you really retrieve).”

We are happy that Dr Di Noia agrees with our approach of showing how using additional information in the NN input vector improves the retrievals. It is precisely for this reason that we adopted a systematic approach to input variable combinations whereby the complexity of the input space was gradually built up – in order to identify a combination of available satellite inputs that could be used to retrieve aerosol microphysics and optical parameters. We agree with Dr Di Noia that the approach followed could be called “empirical sensitivity analysis” and we have decided to include this useful naming convention in the text in the introduction on Page 10968 Line 7 as follows:

“This approach is essentially a form of empirical sensitivity analysis applied to the input data.”

We also revised the text in order to further emphasize this point. Moreover, we emphasized the fact that for CASE 1, the input vector contains limited information, and that we don’t expect this approach to give good results.

As Dr Di Noia points out, the CASE 4 NN does not suffer as much from the problem of limited principal components (PCs). This has been emphasized in the revised manuscript. Following Dr Di Noia’s suggestion, we reapplied PCA to the NN outputs and indeed, 7 PCs account for 98% of the variance – i.e. the NN output gives the same number of PCs as the AERONET target data at Dakar as expected. We do not believe it is important to include this in the revised manuscript as it may distract the reader.

“2. I would strongly recommend to remove Section 4.4 about the statistics of the compliance between your retrievals and the accuracy requirements by Mishchenko et al. (2007). As I said in my previous comment, I think that the test set you are using is too case-specific to claim that, and that “small difference from AERONET” does not mean “certainty”, especially if these differences are mostly evaluated in “average” situations.”

We thank Dr Di Noia for his suggestion. We agree with his view and we have removed the section from the paper and the corresponding Table . For completeness, in the Supplementary Material, we present the daily-averaged AVSD for all test data at Dakar as retrieved with the CASE 4 NN together with the AERONET retrieval and with accompanying estimated errors on the AERONET data.

“3. I would suggest to include in the revised manuscript the analysis of the mean absolute error you presented in your reply to my previous comment. I think that this is an important part of the verification process. However, as I said above, in interpreting the differences between (NN-AERONET) and (AERONET-mean) I would recommend you to acknlowledge that also AERONET aerosol parameters often have non-negligible uncertainties (based on Dubovik et al., 2000), so that the significance in those differences should not be overinterpreted. For the same reason, I would suggest you to avoid to use many decimal digits to represent the mean absolute errors, because I am quite sure that the least significant digits do not have a real meaning.”

Thank you. As mentioned in our reply to Dr Di Noia’s previous comment in the interactive discussion, we are happy to include the results of this analysis in the revised manuscript and also to emphasize that AERONET aerosol parameters often have non-negligible uncertainties (based on Dubovik et al., 2000). Sections 4, 5 and 6 in the revised manuscript have been altered accordingly. We have also added plots in the Supplement of the trend in the MAE and MARE obtained at Dakar over timescales ranging from 1-dy to 1-yr.

“MINOR COMMENTS

-Abstract, P10957, L1. I do not think it is correct to say that the aerosol parameters were “previously inaccessible” from space, because methods for the retrieval of the aerosol refractive index and the aerosol size distribution have been previously reported for POLDER.”

We would like to thank Dr Di Noia for kindly pointing out that algorithms for the re-

trieval of aerosol microphysical properties from space have also been developed for the POLDER instrument. We were unaware of 4 out of 5 of the papers Dr Di Noia brought to our attention (see below please). Having checked the papers by Deuzé et al. (2000, 2001) and Hasekamp et al. (2011) and Waquet et al. (2014) we agree that this assertion is not correct and have removed the words “previously inaccessible”. Thus, in the revised manuscript, we have changed the sentence in the abstract on P10957, Lines 1-2 from:

“offer some potential for moderately accurate daily retrieval of previously inaccessible aerosol parameters from space.”

to:

“offer some potential for moderately accurate daily retrieval of aerosol parameters from space”.

“-Introduction. P10597, L10. Perhaps you might replace the reference to IPCC (2007) with the more recent one of the 5AR (IPCC, 2013).”

We agree. The citation has been changed from “IPCC (2007)” to “IPCC (2007; 2013)” in the revised manuscript and have added the corresponding reference:

IPCC (2013), "Climate change 2013, the Physical Science Basis: Working Group I Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change", Cambridge University Press, New York, 2013.

“-P10597, L13. I would move the reference to Dubovik and King (2000) from after “robust inversion algorithms” to after “retrieval of aerosol parameters”.”

We agree. The citation has been moved in the revised manuscript.

“-In the introduction it should be mentioned that algorithms for the retrieval of the aerosol microphysical properties from space have been developed for the POLDER instrument on the platforms ADEOS-1 (Deuzé et al., 2000, 2001) and PARASOL

(Dubovik et al., 2011; Hasekamp et al., 2011; Waquet et al., 2014)."

We would like to thank Dr Di Noia for bringing 4 of these 5 important papers to our attention (we were aware of the Dubovik et al 2011 paper which was included in our list of references). On P10958, Line 17, we have added the sentence in the revised manuscript:

"Importantly, a statistically-optimized inversion algorithm applied to multiangle photopolarimetric measurements has recently demonstrated that aerosol properties are obtainable from the POLDER instrument on the platforms ADEOS-1 (Deuzé et al., 2000, 2001) and PARASOL (Dubovik et al., 2011; Hasekamp et al., 2011; Waquet et al., 2014). These methods have not yet been independently validated."

Accordingly, the following 4 references have been added in the revised manuscript.

Deuzé, J. L., et al. (2000), "Estimate of the aerosol properties over the ocean with POLDER", J. Geophys. Res., 105(D12), 15329–15346, doi: 10.1029/2000JD900148

Deuzé, J. L., et al. (2001), "Remote sensing of aerosols over land surfaces from POLDER-ADEOS-1 polarized measurements", J. Geophys. Res., 106(D5), 4913–4926, doi: 10.1029/2000JD900364

Hasekamp, O. P., et al. (2011), "Aerosol properties over the ocean from PARASOL multiangle photopolarimetric measurements", J. Geophys. Res., 116, D14204, doi: 10.1029/2010JD015469

Waquet, F., et al. (2014), "Retrieval of the Eyjafjallajökull volcanic aerosol optical and microphysical properties from POLDER/PARASOL measurements", Atmos. Chem. Phys., 14, 1755-1768, doi: 10.5194/acp-14-1755-2014."

"-P10959, L24-26. I do not think that AERONET aerosol microphysical properties retrievals are based on multivariate regression. As far as I know, such retrievals are performed by iterative fitting of a radiative transfer model using Tikhonov regularization, as described by Dubovik and King (2000)."

We would like to thank Dr Di Noia for this clarification. This change has already been made following the comment from Anonymous Reviewer #3 whereby in the revised manuscript on P10959, Line 26 we have changed the sentence from:

"by performing multivariate regression - which must be performed for each measurement."

to:

"by peforming a a numerical inversion of the observations - which must be performed for each case"

"-P10959, L25 till P10960, L1. What does it mean that NN retrievals can be performed "without having to recalculate each day"?"

We agree. In the revised manuscript on P10959 Line 28 to P10960 Line 1, we have cut the potentially confusing ending to the sentence as follows:

from:

"...CRI, SSA, and ASYM for the entire data sample in a single step – without having to recalculate each day."

To:

"...CRI, SSA, and ASYM for the entire data sample in a single step"

"-P10960, L3. I would replace "inverse function" with something like "a nonlinear regression function yielding an estimate for the atmospheric state given the measurement vector". In fact, the forward function is usually not invertible, therefore the concept of "inverse function" does not really apply here."

Thank you. We agree. In the revised manuscript on P10960 Lines 2-3 we have changed:

"To be more specific, the NN calculates the inverse function (applying to all cases covered by the training space),. . .”

To:

“To be more specific, the NN calculates a nonlinear regression function yielding an estimate for the atmospheric state given by the measurement vector (applying to all cases covered by the training space),. . .”

“-P10960, L6-7. The reason why the calculation of the inverse function (or of the regression function) takes time is not the need for running a grid of NNs, because this step is not strictly necessary (you decided to do it, but not everyone does), but simply because the training times can be very long (also depending on how many training data are used).”

We thank Dr Di Noia for this observation. We have changed the sentence to:

“The calculation of the inverse function may require considerable time in the case of a NN since, depending on the training data, NN training can be long; but, once complete, the retrieval using the trained optimal NN is instantaneous.”

“-P10973 and so on. While I find it legitimate to say that a correlation coefficient higher than 0.95 indicates a “very high” precision, I find it a little bit arbitrary to say that correlation coefficients smaller than 0.5 indicate a “moderate” agreement. Why is this “moderate” and not “poor”?”

We thank Dr Di Noia for this comment. We agree. In our answer to his Point 2 under “MAJOR ISSUES” we commented that such categorizations are subjective. In a further comment (below) Dr Di Noia suggests changing the categories from:

“Very Poor” R(d)<0.2, “Poor” 0.2≤R(d)<0.3, “Moderate” 0.3≤R(d)<0.4, “Good” 0.4≤R(d)<0.5 and “Very Good” R(d)≥0.5.

to:

“Very Poor” R(d)<0.2, “Poor” 0.2≤R(d)<0.4, “Moderate” 0.4≤R(d)<0.6, “Good”

0.6≤R(d)<0.8 and “Very Good” R(d)≥0.8.

in accordance with a linear scale. This seems more sensible to us also and we have made this change in Table 7. Furthermore, in the revised manuscript in our presentation of the results from P10973-P10981 Line 9, we have removed all appearances of the terminology “moderate” in this section, and have just stated the range of values of the regression coefficient without making any subjective assessment.

“-P10983, L20-22. I would remove the adjective “new”. Determining the NN architecture via exhaustive search over the space of NN architectures is a standard technique, and there is a large number of works where this approach is discussed or at least mentioned. Here are just a few references:

Curry, B., and P. H. Morgan (2006), “Model selection in Neural Networks: Some difficulties”, Eur. J. Oper. Res., 170, 567-577, doi: 10.1016/j.ejor.2004.05.026

Gorr, W. L., D.Nagin, D., J. Szczypula (1994), “Comparative study of artificial neural network and statistical models for predicting student grade point averages”, Int. J. Forecasting, 10, 17-34, doi: 10.1016/0169-2070(94)90046-9

Lawrence, S., C. L. Giles, and A. C. Tsoi (1996), “What size neural network gives optimal generalization? Convergence properties of backpropagation, Technical Report UMIACS- TR-96-22 and CS-TR-3617, Institute for Advanced Computer Studies, University of Maryland, College Park, MD, United States

Stathakis, D. (2009), “How many hidden layers and nodes?”, Int. J. Remote Sens., 30(8), 2133-2147, doi: 10.1080/01431160802549278”

We are extremely grateful to Dr Di Noia for kindly drawing these papers to our attention. We are happy that the method we used is well established. We believe it is important to emphasize this in the text. In the revised manuscript we removed the word “new” on P10983 Line 20 and have added the following sentence at P10983 Line 22:

“While such an approach is well established in the scientific literature (Gorr, Nagin and

Szcypula, 1994; Lawrence, Giles and Tsoi, 1996; Curry and Morgan, 2006; Stathakis, 2009), this is the first time it has been applied in the development of an atmospheric measurement technique."

Accordingly, we have added these 4 additional citations in the reference section. Furthermore, in Section 1.2, we have expanded our description of contemporary studies slightly to also increase reference to the following relevant works:

Albayrak, A., Wei, J., Petrenko, M., Lynnes, C., & Levy, R. C.: Global bias adjustment for MODIS aerosol optical thickness using neural network. Journal of Applied Remote Sensing, 7(1), 073514-073514, 2013.

Ristovski, K., Vucetic, S., & Obradovic, Z.: Uncertainty analysis of neural-network-based aerosol retrieval. Geoscience and Remote Sensing, IEEE Transactions on, 50(2), 409-414, 2012.

Sellitto, P., Di Noia, A., Del Frate, F., Burini, A., Casadio, S., & Solimini, D.: On the role of visible radiation in ozone profile retrieval from nadir UV/VIS satellite measurements: An experiment with neural network algorithms inverting SCIAMACHY data. Journal of Quantitative Spectroscopy and Radiative Transfer, 113(12), 1429-1436, 2012.

"-P10999, Table 7. As I said previously, I find the "mapping" from a value of the correlation coefficient to an adjective (good, moderate, poor, etc.) a bit arbitrary (e.g., the statement that anything above 0.5 is "very good" might be questionable). Wouldn't it be better to just fill the table with the correlation coefficients? Keeping the color scale might also be a good idea to make the results more immediate, but I would use a different scaling (perhaps a scaling that is linear with respect to R might make more sense, given the statistical meaning of R, e.g. R>0.8= dark green, 0.6-0.8=light green, 0.4-0.6=yellow, 0.2-0.4=orange, <0.2=red).

We thank Dr Di Noia for persevering with this issue. We agree. As mentioned above, we have changed the categories and have made the corresponding changes to (the

newly numbered) Table 6 in the revised manuscript. We attach an image version of this table and the additional Figures 8 and 12 and the revised Figure 11 (due to removal of reference to the Mishchenko uncertainties). On P10984 Line 14 we have also added the phrase:

"with a categorization based on a linear scale of values of the regression coefficient."

We would like to express our gratitude to Dr Di Noia for offering his expertise and advice a second time, and we hope to have helped to clarify and/or answer his methodological and presentational concerns. We have made a special acknowledement to Dr Di Noia for his contribution to improving the quality of the final manuscript which we have changed from:

"Acknowledgements. M. Taylor was supported by a FP7-PEOPLE-2011-IEF grant for the project "AEROMAP: Global mapping of aerosol properties using neural network inversions of ground and satellite based data". M. Taylor would like to thank the members of IERSD-NOA for excellent training in the field, and for their kind and ongoing hospitality."

to:

"Acknowledgements. M. Taylor was supported by a FP7-PEOPLE-2011-IEF grant for the project "AEROMAP: Global mapping of aerosol properties using neural network inversions of ground and satellite based data". M. Taylor would like to thank the members of IERSD-NOA for excellent training in the field, and for their kind and ongoing hospitality.

The authors are especially grateful to Dr Antonio Di Noia for his open and constructive comments and reviews which have substantially helped to improve the submitted manuscript. We would also like to thank the handling associate editor Professor Alexander Kochanovsky for his excellent choice of scientific experts in this field."

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/6/C4886/2014/amtd-6-C4886-2014-supplement.pdf

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 10955, 2013.

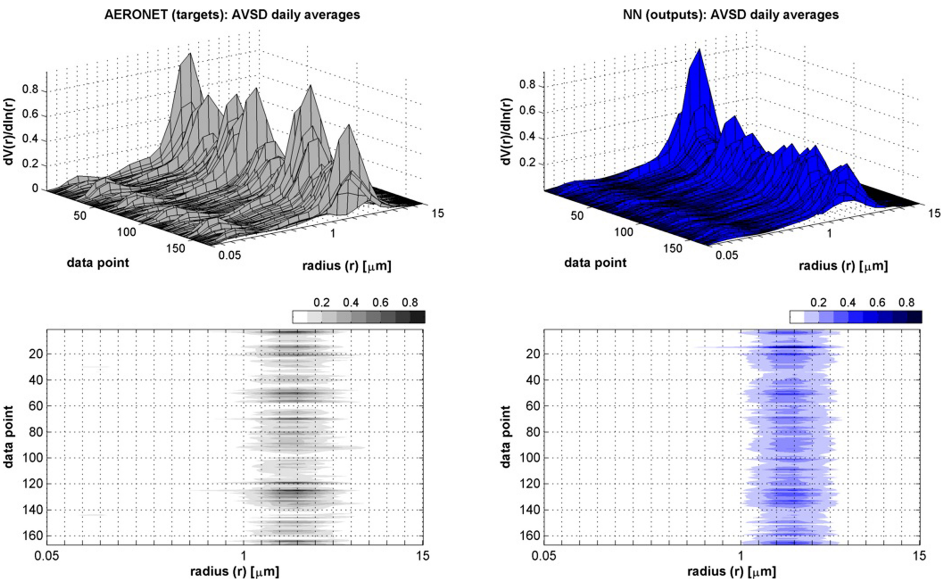

Fig. 1.

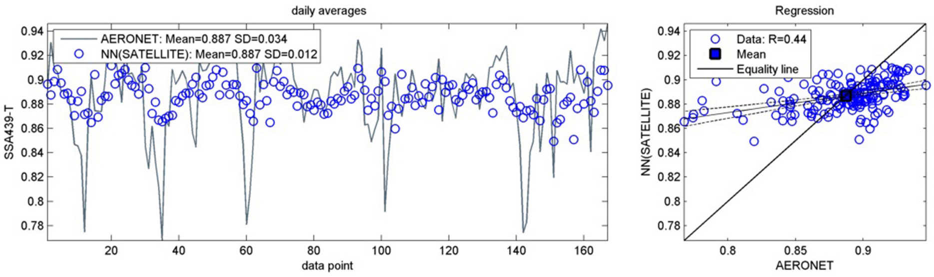
daily averages
AERONET: Mean=0.887 SD=0.034
NN(SATELLITE): Mean=0.887 SD=0.012
SSA439-T
data point
Regression
Data: R=0.44
Mean
Equality line
NN(SATELLITE)
AERONET

Fig. 2.

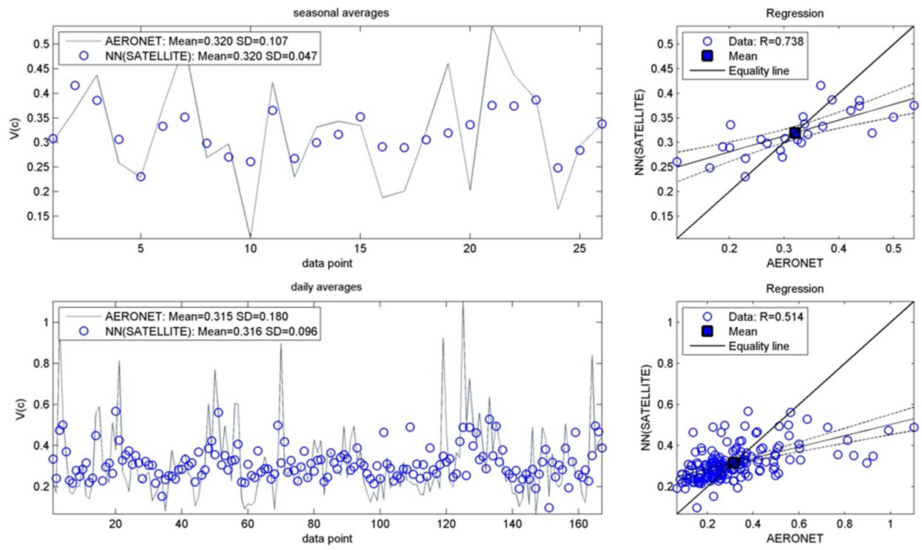
seasonal averages
AERONET: Mean=0.320 SD=0.107
NN(SATELLITE): Mean=0.320 SD=0.047
V(c)
data point
Regression
Data: R=0.738
Mean
Equality line
NN(SATELLITE)
AERONET
daily averages
AERONET: Mean=0.315 SD=0.180
NN(SATELLITE): Mean=0.316 SD=0.096
V(c)
data point
Regression
Data: R=0.514
Mean
Equality line
NN(SATELLITE)
AERONET

Fig. 3.

ASSESSMENT		AERONET			SATELLITE
		CASE 1	CASE 2	CASE 3	CASE 4
Microphysics	V(f)	Very Poor	Poor	Very Poor	Poor
	V(c)	Very Good	Very Good	Very Good	Moderate
	η	Moderate	Moderate	Moderate	Moderate
	Radial Bin 15	Very Good	Very Good	Very Good	Moderate
	CRI-R(440)	Poor	Poor	Poor	Poor
	CRI-R(675)	Very Poor	Poor	Poor	Poor
	CRI-R(870)	Very Poor	Poor	Poor	Very Poor
	CRI-R(1020)	Poor	Moderate	Poor	Very Poor
	CRI-I(440)	Moderate	Poor	Poor	Poor
	CRI-I(675)	Moderate	Moderate	Very Poor	Poor
	CRI-I(870)	Moderate	Moderate	Very Poor	Poor
	CRI-I(1020)	Moderate	Moderate	Very Poor	Poor
Optics	SSA(440)	Poor	Poor	Poor	Moderate
	SSA(675)	Moderate	Moderate	Poor	Poor
	SSA(870)	Moderate	Moderate	Poor	Poor
	SSA(1020)	Moderate	Moderate	Poor	Poor
	ASYM(440)	Moderate	Moderate	Moderate	Very Poor
	ASYM(675)	Moderate	Moderate	Moderate	Very Poor
	ASYM(870)	Moderate	Moderate	Poor	Very Poor
	ASYM(1020)	Poor	Moderate	Poor	Very Poor

Very Poor	$R(d) < 0.2$
Poor	$0.2 \le R(d) < 0.4$
Moderate	$0.4 \le R(d) < 0.6$
Good	$0.6 \le R(d) < 0.8$
Very Good	$R(d) \ge 0.8$

Fig. 4.