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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud and aerosol classification for 2 $1/2$ years of MAX-DOAS observations in Wuxi (China) and comparison to independent data sets" *by* Y. Wang et al.

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In the manuscript "Cloud and aerosol classification for 2 1/2 years of MAX-DOAS observations in Wuxi (China) and comparison to independent data sets" the authors Wang et al. provide a nice overview of classification features that can be derived from MAX-DOAS measurements to detect clouds and aerosols. They also describe how those features could be improved compared to an earlier classification scheme by Wagner et al. 2014. Since the topic is relevant for ACP and could be of interest for other MAX-DOAS projects, I recommend publications after some revisions. My concerns which should be addressed before the final publication are as follows:

The overall objective of this study is not very clear. I assume that the goal was to improve the classification scheme by Wagner et al. 2014, so that it can be used for the MAX-DOAS measurements as well, but no clear measure to quantify this improvement is presented.

There are lots of open questions, e.g.: What is the percentage of incorrectly classified scenes (that you know of)? How did this value improve compared to Wagner et al. 2014? Do your new classification parameters also improve the Cabauw retrieval? Under which conditions can your classification be used for other measurement campaigns?

You could use a classification scheme based only on AERONET and satellite data as ground truth and compare how often the MAX-DOAS algorithm leads to the same classification. Some scenarios might not fall clearly in the given categories, but you can filter those out, since you have a very long time series.

Another problem of your study is that you use additional data to improve the thresholds and add additional indicators (e.g. in Sec. 2.3.3 you describe that add an indicator to detect the presence of continuous clouds to match MODIS observations, and also in the supplement you mention that you use scenes with specific sky conditions as selected based on visual images from MODIS to select the thresholds), but then you compare your classification results to those additional data sets. If you use the comparison with AERONET and satellite data to improve the algorithm's thresholds, the better agreement between the MAX-DOAS classification algorithm and AERONET and satellite data is not the result of your study (as described in Sec. 4), the comparison is rather the tool to derive improved thresholds which are then your main results. However, then you need an independent way to assess the quality of the algorithm, e.g. a scientific explanation of the thresholds, or some equations for calculating the values depending on certain parameters (instrument characteristics, location dependent parameters, etc.). Then you could apply your improved classification to the Cabouw data set and check if it improves the classification for that data set as well. You describe that you ad-

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justed the thresholds for different quantities according to properties of the MAX-DOAS instrument (p. 4664 l. 4), but I cannot find a description of that dependency (the supplement is referenced here, but there I can only find a description of how MODIS and AERONET data were used to select threshold values). The dependency on instrument characteristics might be of interest to readers who would like to adjust the classification scheme to their own instruments.

You plotted a lot of different combinations of classification results and several other parameters and describe the results, but what is the significance of the results? For example, when you write "Overall the results from MAX-DOAS are mostly consistent with the AOD data from MODIS." (p. 4675 l.16), that's very vague and more information is needed to allow the reader to see this consistency as well. As mentioned already, a clear criterion is needed to quantify the improvements compared to Wagner et al. 2014.

In the following I will list some comments about minor concerns:

In the abstract (p.4654 l.24) and in Sec. 3.2.4 (p.4678 l.20) you write that "the satellite cloud products contain valuable information on aerosols", but it's more an unwanted contamination, right? (it sounds a little like "it's not a bug, it's a feature" thing, but in fact it is a bug).

In the list of different MAX-DOAS retrievals (p.4655 l.12f) you could add Hartl, A. and Wenig, M. O.: Regularisation model study for the least-squares retrieval of aerosol extinction time series from UV/VIS MAX-DOAS observations for a ground layer profile parameterisation, Atmos. Meas. Tech., 6, 1959-1980, doi:10.5194/amt-6-1959-2013, 2013.

In Sec. 2.2.2 one could first get the impression that you are measuring SCDs and dSCDs. You write that "the FRS used in our analysis is also taken from the MAX-DOAS measurements" (p. 4662 l.15), so you only have dSCDs but no SCDs, is that correct? You write "Because of the systematic variations we did not use the O4 absorption

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measured in zenith for the identification of optically thick clouds", but isn't the zenith measurement used for the dSCD? That should be made clearer.

In Fig. S1, can you get rid of the strong temporal variation of the O4 dSCDs if you adjust the reference spectrum according to the FWHM from the high resolution solar spectrum fit?

Fig. 10 b is not really needed, since it only adds information about the number of measurements per month and is difficult to interpret. It doesn't show the number of sky conditions in each month but rather the number of detected sky conditions, so it depends on the running time of the instrument.

Fig. 11-17 can be replaced by a more meaningful quality measure (see comment above) which can probably be shown in a single plot or table. This plot or table could also compare the classification results using thresholds from Wagner et al. 2014.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4653, 2015.