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Interactive comment on "Comparison of ozone retrievals from the Pandora spectrometer system and Dobson spectrophotometer in Boulder, Colorado" *by* J. Herman et al.

Anonymous Referee #2

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This paper presents mainly a comparison of the total ozone data derived from the DOAS-based Pandora system and the standard Dobson spectrophotometer that operates in Boulder CO. Such DOAS-based systems may have the potential to be included in the global total ozone observing network, following a thorough investigation of their quality and comparability with the existing standard Dobson and Brewer instruments. The Pandora systems can supplement the existing network of the standard Dobson and Brewer total ozone instruments, and is essential to have thorough analyses of their performance. In this context this paper could be an essential contribution towards this goal. In addition, the paper presents comparisons among total ozone data de-

rived from satellite-borne instruments, but these are discussed only briefly. Although interesting results are presented showing, generally, a good comparison between the Pandora and the Dobson, parts of the paper would need further work in order to prove the quality of the Pandora data.

My major concern for this paper is the use of different absorption cross section data when comparing the two systems. Despite the general agreement that the Bass and Paur (BP) data are outdated and would be probably be replaced in the near future, still the official total ozone data reported by the standard Dobson and Brewer instruments are based on BP. Since the aim of the paper is to compare the total ozone retrieved by the two instruments, the comparison should be based on the same spectroscopic data.

While the paper has started very nicely with adequate discussion of the Pandora and Dobson comparison, later on, a lot of figures are included addressing very briefly different topics, not all relevant with the title and aims of the paper. For instance, the cross comparisons of the satellite data cannot be considered a significant contribution to the paper. Similarly, Figures 10-12 show some examples of daily variations of radiance under clear and cloudy conditions and of total ozone. I don't think that are necessary. I am sure that some of the figures could be omitted to the benefit of the paper.

Finally, the discussion of the 2 last paragraphs of section 4 is rather simple and I do not consider it as a significant contribution to the paper. If the diurnal pattern of total ozone were to be discussed, I would expect a more thorough statistical analysis, where uncertainties in the measurements are taken into account, at least, quantitatively.

To conclude, I think that although the paper is a good contribution to the emerging network of Pandora spectrometers, I believe that the authors should improve the discussion of the weak sections, and possibly reduce the number of figures.

Specific comments by page and line number

3054 3: Please include the units for the resolution (500000!). How the normalization was done? 5: Why the Malicet cross sections have been used, considering that the Dobson is based on BP? The effect from the different cross sections should be discussed. 8: What is the uncertainty of the applied slit function, which usually is wavelength dependent? What is the resultant uncertainty in the ozone retrieval? 11: How much is the stray light reduced? How much remains and what is the effect on ozone retrieval? Does the UV340 filter block wavelengths above 525 nm (e.g. in the solar infrared) that can be also a source of stray light? 14: What is meant by "weighting system"? 16-17: If I understand correctly, 4000 spectra in 20s correspond to an integration time of 5 milliseconds. Have you tested the linearity of the system at such low integration time? Is the signal to noise ratio sufficiently large? 23: What is the degree of the polynomial used in practice? Is it the same for all solar zenith angles? 24: How much the total ozone is overestimated under such conditions? Is that a problem for locations with significant aerosol loads?

3055 10: When Pandora TCO is compared with the Brewer, what absorption cross-section dataset is used? Here again the Brewer TOC is based on BP, while the Pandora TOC is based on Malicet. I would expect an additional effect, even seasonal.

3056 18: The systematic difference between the two systems cannot be attributed solely to differences from the effective temperature, since differences should appear also due to the use of different absorption cross sections. Although the differences in the measurements match those calculated from the change in the stratospheric temperature, there is an almost 2-month shift in the maximum of the annual patterns between figures 1 and 2. In addition, after the temperature correction (Fig 3), there is still a residual seasonal pattern. 20: "The seasonal difference is significant at the level of 1 standard deviation 5DU of the observed data about the Loess(0.5) curve (Fig. 1b)." I don't understand this statement. To my understanding, the seasonal difference is described by the loess curve which spans between -5 and +10 DU. On top of this there is day-to-day variability (noise ?) which is the order of $\pm$5DU. Please clarify. 24-25: I

do not understand why in Table 1 the effective temperature for each month is given as a function of total ozone. The effective temperature is usually calculated by weighting the temperature profile with the ozone profile. The total ozone does not enter into this calculation. Please explain in more detail the procedure for the calculation of the ozone weighted effective temperature. 26-27: "The tables are given as a function of latitude, ozone amount, and height for each month". Please make clear if these are the tables from Labow (2011) and Wellemeyer et al. (1997).

3057 24: It is not clear how the Dobson data were corrected; using the constant temperature coefficient as mentioned in line 13, or using Table 3?

3058 4: I suggest redrawing figures 4a and 4b, with axes of equal length. (Same for Figure 7) 5: In this case the high correlation arises from the seasonal variation of TOC. The scatter is more important for establishing the level of agreement between the two systems. 24: I assume that the satellite TOC is based also on the Malicet absorption cross section, and this removes the seasonal dependence that was seen in the Dobson comparison. I think this should be discussed.

3059 1: Comparison of the TCO data from the 2 satellites is shown but results are discussed very briefly, without any background information, even in the Introduction. I do not see strong relevance with the aims and title of the paper. Generally, this section (3) of the paper is very short with a lot of figures.

3060 21: It would be good to provide some more details on the method that tis used to weight the different wavelengths according to their noise. Two lines later it is said that the retrieval range is shifted. Which of the two methods is actually used? 27: It would be interesting to plot on fig 14 the data from the Dobson. How many Dobson measurements are taken on average every day?

3061 1-24: The last paragraphs of section 4 are very brief, again with lot of figures, addressing different topics, not very much related to the aims of the paper.

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