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Atmospheric Measurement Techniques Discussions

Interactive comment on "The CU 2-dimensional MAX-DOAS instrument – Part 1: Retrieval of NO_2 in 3 dimensions and azimuth dependent OVOC ratios" *by* I. Ortega et al.

Anonymous Referee #1

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This paper suffers from two major deficiencies, which need to be addressed before publication 1) There is no independent validation data presented to show that the profile and geographic distribution results represent a real atmosphere 2) The spectral fitting does not demonstrate uniqueness with respect to the wavelength window chosen by the authors. For example, the window for HCHO is a frequently used spectral window, but experience has shown that quite different answers can be obtained with a slightly wider window. The paper needs to independently address the uniqueness issue using the results from the new fairly coarse resolution (0.75 nm) MAX-DOAS instrument.

General Comments

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Interactive Discussion
Discussion Paper

The paper is very well written with generally clear figures. The technique presented is new, and appears to have significant promise for ground-based measurements.

âĂć I think the term solar stray light is meant to be solar scattered light. âĂć A minor note: Aside from the apparent compactness, this instrument is not “novel”. For example, the Washington State University MF-DOAS instrument has all of the same capabilities. At a minimum, this earlier instrument should be referenced. âĂć The OMI measurements of NO2 do not qualify as validation, since OMI always underestimates NO2 amounts in polluted regions. This is true in the case presented in your Figure 11. âĂć What is the signal to noise ratio for the various measurements? Was the 30 second exposure an average of multiple measurements or just a single exposure? âĂć What is the FOV for direct-sun measurements? âĂć What is the slit function shape as a function of wavelength? âĂć Does the fiber optic cable move? If so, what is the sensitivity to motion? Motion effects would seem particularly important for the large range of motion for this instrument. âĂć The detector is 1340x400 pixels, suggesting that there are more than enough pixels ($\sim$7 and 9) to determine the slit function as a function of wavelength. What was the slit function and how did it compare to the assumed slit function in the retrieval program (usually Gaussian). âĂć NO2 and HCHO are highly variable in time. How does the 13-14 minute duration affect the azimuthal distribution results? What kind of error is introduced? What is the variation in time for continuous measurements in one direction? âĂć How sensitive are you to the assumed P and T profiles (US Standard Atmosphere) compared to the real P and T profiles at the observing site? Equation 7 should be quite sensitive to the P and T profile assumptions. âĂć Figure 6. . .”The PBL height is estimated as the 1/(2e) decrease of the near surface NO2 VMR at each wavelength.” Does this mean that you have assumed an exponential profile shape? Frequently this is not the case. How does your assumption affect the PBL height calculated? âĂć Figure 3C. . . It is not clear that the fit to CHOCHO is good except for the peak near 455 nm âĂć Figure 1B. . . What keeps the direct-sun light from scattering off the edges of the prism after coming out of the integrating sphere? Are there lenses and a collimator that are not shown? Equations: Equation 7 (line

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404) does not seem to make sense. . . .First, what does the symbol O4 mean? When combined with eqn. 6 the Leff seems to be just the ratio of the differential AMFs times the vertical PBL height. I do not see how this is related to the horizontal effective path length. The authors need to explain this in more detail.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11653, 2014.