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Atmospheric Measurement Techniques Discussions

Interactive comment on "Inter-comparison of MAX-DOAS measurements of tropospheric HONO slant column densities and vertical profiles during the CINDI-2 Campaign" *by* Yang Wang et al.

Anonymous Referee #2

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This manuscript evaluates results from intercomparison exercise of HONO observations during CINDI-2 campaign performed at Cabauw, the Netherlands. Previously HONO intercomparison was conducted during MAD-CAT campaign but was limited to dSCD comparisons. Here the exercise, with more participants, was extended to vertical profiling. Tasks were carefully designed, where first the retrievals for HONO and aerosols were conducted with individual groups' own observations/processing for which protocols were provided, and where the HONO dSCDs, and then the aerosol quantities additionally, were constrained to common values in order to compare the performance of profiling algorithms, separately from the influence from diversity in HONO dSCDs. Basically high degree of agreement was found particularly for selected in-

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Discussion paper

struments/groups. The random and systematic discrepancies were evaluated, mainly around the median quantities. With the state-of-the-art instruments, the relative random and systematic discrepancies for dSCDs were about 15 and 30% for low elevation angles, and were about 20% for both for VCDs and near-surface VMRs. The evaluation is important not only for providing adequate a priori errors for the MAX-DOAS retrievals themselves (as the authors mentioned) but also for adequate validation of satellite observations of HONO using MAX-DOAS. Additional findings that atmospheric variability is important for the random term and that basic agreement is reached with active DOAS measurements are also important and interesting. Basically the analytical methods and logics are sound. However, clarification is needed for some important points. First, I am afraid that the true VCD values (including discrepancies) mentioned in text are 10 times lower (10ˆ15 must be 10ˆ14.) The authors need to check all values very carefully as such mistake is fatal. Secondly, the authors need to state why systematic errors quantified to be 30% for dSCDs could diminish to a systematic error of 20% in VCDs. Thirdly, the standard deviation is calculated over both days and instruments/retrievals (e.g., Fig. 2 and 6). While the authors conclude the variability derived from instruments/retrievals, day-to-day variability may severely affect. As a whole major revision must be made before being considered for publication.

Specific comments

1. In Abstract, the authors should highlight what was newly done with CINDI-2, beyond MAD-CAT. I believe VCDs and near surface VMRs were intercompared for the first time.

2. Page 1, line 53, 10ˆ14 instead of 10ˆ15? Please check also for Lines 518, 563, 571, 572, 573, 574, 715, 716, 742.

3. In Abstract, better to mention that systematic and random discrepancies were determined against median observations basically.

4. Line 135. CINDI-2

5. Lines 254. Better to tabulate differences among tasks T1a, T1b, T2a and T2b. Aerosols in T2b are same as those in T1a and T2b? Are "aerosol retrievals at 340 nm given in Tirpitz et al. 2020" same as "aerosols retrieved from the O4 delta SCDs" mentioned in line 263? Table 3 mentioned 360 nm.

6. Lines 346-349. Is this statement valid when variability is studied including days with different concentrations?

7. Line 369. How less were the photons could be quantitatively discussed.

8. Line 407. Not only "codes" but instrumental characteristics, such as how well the slit function is represented during DOAS fit might affect?

9. Line 413. Define what is "mini". This statement should only be valid for Cabauw or cleaner sites.

10. Line 416. Comma should be period

11. Line 448. Did the difference occur on selected days? Or for most of the days of observation?

12. Line 467. I believe the quantities are not "in general" but for selected high-performance instruments.

13. Line 471. Why DOAS fit error is discussed within systematic term here, while it was discussed under random term before?

14. Section 3.3.3. Influence from different FOV angles for individual instruments can be ignored?

15. Line 531. The better result in T2a (compared to T1a) might be partly from the fact that only high-performers took part in the T2a exercise?

16. Line 561. 0 to 0.2 km, 0.2 to 0.4 km, and 0.4 to 0.6 km (4 km in the figure; be consistent)

17. Line 566. What are the "modelled" quantities? Fitted dSCDs for some elevation angle? Readers may even wonder if they are from chemical transport models.

18. Line 603. I did not clearly see the systematically low values at high altitudes from LMU in Fig. 8.

19. Line 617. Was any similarity found for NO2, a potential precursor of HONO?

20. Line 618, 625, lifetime

21. Section 5.1 Was constant Sa used for the general comparison exercise previously described? Any feedback comment from this exercise?

22. Section 5.1. Need to discuss negative values found with profile 1 for AUTH and INTA.

23. Line 721. Define "good" spectrometer.

24. Line 767. Are the uncertainties for dSCDs?

25. Figure 3. Scatterplots should be presented (maybe in supplementary) to show to how low dSCDs agreement is found.

26. Figure 4, Put labels (a), (b) and (c). What are the "subplots" mentioned in the caption?

27. Figure 5. The scale only starts from zero and thus negative values are not shown. AUTH sometimes went to negative range here, as shown in Figure 9?

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