

Response to Anonymous Referee #2

We would like to thank the reviewer for their thorough review and helpful comments which are addressed individually in the response below. The reviewer's comments are included in blue and italics.

This manuscript is a well written and extensive intercomparison between UV-Visible spectrometers during a field study with a highly refined strategy. The work demonstrates very good agreement between slant column densities of the gases mentioned in the title during the campaign. These efforts are necessary for understanding agreement between instruments and for use in subsequent profile retrievals and satellite validation. The work is clearly relevant to Atmospheric Measurement Techniques and I recommend that it be published with minor revisions. Below are general and then specific comments.

General comment:

The manuscript goes through extensive procedures that were designed to synchronize measurements to be of the same volume of air at the same time. This synchronization has been improved as compared to the prior campaign, and results are improved. This result indicates that there are significant variations in the actual slant column densities at the same elevation angles if viewed at even slightly different times. The result is not surprising for short-lived pollution gases that probably have a variety of nearby sources, but it indicates that subsequent inversions to vertical concentration profiles and vertical column densities may have challenges due to variations in the vertical concentration profile that occur during the measurement profile. This point is discussed on page 12, lines 21-31, but is not given as much importance as is necessary for this finding.

To emphasis this finding further, we have added a brief summary of what has been discussed in Section 3.7 (former page 12, lines 21-31) to the conclusions as part of the 1. bullet point.

On the other hand, it seems that this point may be the origin of the "conclusion" on lines 13-14 of page 17 that the design "was not fully adequate for profile inversion experiments". This conclusion should be removed or reworded because the present work does not show inversion experiments and thus cannot conclude on them. If the point was meant to be that variability in space and time is observed, then that is a conclusion. Please make clear both the important point of variability in time and space and discuss relevance for inversions, but do not conclude about inversions that are not shown here.

We have changed the sentence as suggested and added more discussion to this first bullet point (partly also covered by the response to the comment above).

Specific comments:

Page 3, line 34. It should be discussed here that when the instruments that measure profiles sequentially at un-synchronized field studies (as they will typically be used after CINDI-2) that the variability during the profile will affect profile inversions. Potentially the Boesch et al. (2018) AMT paper could be cited.

This is an important point and one of the CINDI-2 companion papers on profile retrievals, 'Intercomparison of MAX-DOAS vertical profile retrieval algorithms: studies on field data from the CINDI-2 campaign' by Tirpitz et al. (see also entry in the reference list) which has just been submitted to AMT, would be the more appropriate publication for this discussion. A brief discussion has also been added in Section 5, 1. bullet point under 'Despite these achievements, a few critical points were identified that deserve more attention in future deployments.'

Page 4, line 24. The Apituley et al. manuscript to be submitted to AMT is really important to the present publication. Is this manuscript submitted? If it is not submitted by the time of this manuscript being decided upon, details should be added here.

Since Apituley et al. is not yet submitted, we have added some information re the measurement site (CESAR) and the CINDI-2 campaign in general:

‘In short, the CESAR site at Cabauw is overall a rural site, with only a few pollution sources nearby, but the wider vicinity of Cabauw is densely populated, with the cities of Utrecht, Amsterdam, The Hague and Rotterdam less than 60 km away and a dense highway grid within 25 km, so that the site experiences recurring pollution events, e.g. such as from the daily morning and afternoon rush hours.

The MAX-DOAS instruments were also complemented with a suite of in-situ, profiling and mobile observations which are described in detail by Apituley et al. (to be submitted to AMT, 2019). In particular, a long-path DOAS measuring near surface mixing ratios of NO₂ and HCHO but also a range of other species such as HONO and SO₂ (see e.g. Merten et al, 2011, for a description of the technique) was operated at the CESAR site for the period of the campaign. Several mobile MAX-DOAS measurements were also made around Cabauw, and between Rotterdam and Utrecht (e.g. Merlaud, 2013). in addition to the static ones. NO₂ profiles were measured with NO₂ sondes (Sluis et al, 2010) and lidar (e.g. Volten et al., 2009), as well as through in-situ observations using the Cabauw meteorological tower. Extensive aerosol information was also gathered using Raman aerosol lidar and in situ samplers.’

Page 5, lines 9-10. The suggestion for future studies should be in the discussion rather than here. Potentially giving an indication to "see section N.M" would be appropriate.

We agree and since this suggestion is also discussed as part of the previous Conclusions section, now part of the newly added Section 5 (Recommendations for network operation and future campaigns), at the end (2nd bullet point), we have deleted this sentence.

Page 6, line 31. Please give the approximate solar zenith angles of these UTC cutoffs so that they can be more easily translated to other work.

This information has been added.

Page 7, line 14. The text says "atmospheric noise", but this effect is not noise but variability given later analysis. Reword.

This has been reworded as suggested.

Page 10, lines 23-29. It may be appropriate to note that retrievals using a zenith reference spectrum within the same elevation sequence (rather than a fixed noon reference) often reduces difficulty in fitting, and thus more instruments could get useful HCHO data if other analysis methods were used.

We agree with the reviewer that using a sequential reference spectrum can potentially reduce instrumental effects, or the impact of misfits to strong absorbers like O₃. However, this has not really been true for CINDI-2 and the agreement seems worse, most likely because noise is added due to the fact that not all instruments are able to capture the sequential reference exactly in the same way.

Page 12, line 24. The word "noise" is used, but this effect is not noise, but rather "variability" due to viewing different airmasses (in time or space).

We have changed the wording from ‘noise’ to ‘difference between the individual data sets’.

Page 12, line 39. Replace "keeps larger" with "remains larger".

Done.

Page 13, line 16. Change "dependency" to "dependence".

Done.

Figure 7 needs a color/symbol key

This has been added as requested.

Table A1. The reference to Vandaele et al. (1998) is not in the references. The paper that I believe is cited seems to indicate the spectrum is at 294K rather than 298K. Please clarify this citation and temperature. This citation and temperature occur in other appendices. Please assure that all sources are fully cited in these appendix tables.

We agree with the reviewer and have added the reference and corrected the citations.