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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of tropospheric NO_2 columns from MAX-DOAS retrievals and regional air quality model simulations" *by* Anne-Marlene Blechschmidt et al.

Anonymous Referee #2

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In "Comparison of tropospheric NO2 columns from MAX-DOAS retrievals and regional air quality model simulations," the authors provide a nice overview of 1) long-term MAX-DOAS records of NO2 in northwest and southwest Europe, 2) a description of regional air quality models used in the CAMS ensemble, and 3) a description of past comparisons of regional CTMs and MAX-DOAS with in situ and satellite data. The comparison of the model ensemble and the four MAX-DOAS NO2 datasets showed general agreement in a broad sense. The authors highlight when and where there are discrepancies between ensemble median model results and MAX-DOAS observations (e.g., seasonal cycle, diurnal cycle), but do not offer ideas on potential approaches for disentangling the causes of these discrepancies.

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Discussion paper

I felt that the paper lacked a final synthesis, written in more general language, of how future simulations and MAX-DOAS deployments like these can isolate effects from individual processes. I hope that the authors consider adding a broader synthesis of their results to the end of section 4, offering possible paths forward for future analyses: what common and distinct attributes of these four sites share? How might these differences and similarities be exploited to investigate chemistry? Emissions? Meteorology? Where might the authors propose future MAX-DOAS instruments be located? Should one expect an ensemble median to capture hourly NO2 variations? Monthly averages? What is the native scale of NO2 spatial variations at the MAX-DOAS sites inferred from the time scale of NO2 variation and wind speed?

Comments: P2, L10-11: NO2 lifetime is much longer in the upper troposphere, primarily because its chemical family, NOx, is mostly present as NO at high altitudes, which has far fewer permanent sinks.

P3, L29: “focusses” – typo

P4: There is no discussion of model resolution. The NO2 lifetime is a function of model resolution. Also, median values may be biased towards coarser models as those with finer resolution may produce highs when a plume passes and lows when not.

P5, L7: These sites, with exception of OHP, seem to be in very similar physical settings, with likely similar meteorology (e.g., vertical mixing characteristics). If so, this fact should be mentioned. Please also consider including a map of the region with sites indicated on a backdrop of satellite-based tropospheric NO2 column measurements. Minor comment: I did not see Lat/Lon values reported for Uccle.

Page 6, Line 29: Has there been any side-by-side operation and comparison of these two instruments? If so, please provide the reference.

P9, L5: “As the typical error on MAX-DOAS retrieved VCDs is around 20%” – please describe this statement in more detail: at what time scale? Random or systematic

uncertainty? Based on measurement inter-comparisons or fitting statistics? Page 9, L17-21: See comment on “page 4” above. NOx lifetime depends on model resolution, and NO2 maxima will be diluted in coarser models. Model resolution needs to be better reported.

P10, L5-17: I have a hard time following the language and reasoning behind this conclusion. Please consider clarifying. Is this because the a priori profiles are generated from similar models as those included in the comparison? Are any systematic effects buried below random sources of uncertainty?

Page 10, L21-31: This analysis and discussion is tangential to the broader scope of the paper and should be removed, as earlier noted by the authors “The impact of clouds on MAX-DOAS retrievals is described in detail by Vlemmix et al. (2015)” I do consider the comparison of model and MAX-DOAS NO2 columns under different cloud conditions to be an interesting topic for its own manuscript.

P10-11, L34-11: How much of the correlation is determined by seasonal and weekly cycle? Consider isolating correlation at one time of day, one season and one set of weekdays (e.g., M-F)

P12, L35: Consider a reference to Beirle et al. (2003). I think that this paragraph could be expanded. Day-of-week effects, over the long-term, are independent of meteorology and driven entirely by variations of emissions and chemistry. Future day-of-week comparisons would be one means of providing systematic approaches to quantify the many processes affecting NO2 (emissions, meteorology uncertainty, chemistry, observational uncertainty)

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