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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 #1

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The paper presents a comparison of time series of tropospheric NO2 VCDs derived from 4 European MAX-DOAS stations to an ensemble of 5 regional models. The horizontal and vertical resolution of MAX-DOAS observations fits in general well to those of the regional models. Thus such a comparison is well suited to evaluate the performance of the model simulations (and also the quality of the MAX-DOAS retrievals). In this respect, the results of this paper are of high importance, and are well suited for publication in ACP. However, I have three major concerns with respect to the evaluation and presentation of the results in the present version of the manuscript, which should be addressed before final publication:

a) One of the main advantages of MAX-DOAS observations is that profile information

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Discussion paper

for the lowest layers of the atmosphere (below about 2km) can be obtained. Profile information is crucial to assess the performance of the model simulations (and to understand deviations from observations). It is a pity (and completely unclear to me), why the authors do not make explicit use of the profile information derived from MAX-DOAS. One – rather simple – way to make use of the profile information (and to compare MAX-DOAS results and model simulations) would be to determine a characteristic layer height (e.g. the layer, below 70% of the total tropospheric column resides) from both the MAX-DOAS observations and the model results.

b) The authors compare the MAX-DOAS results to model ensembles. Although in the appendix, also the comparison results to the individual models are shown, no attempt is made to systematically asses the performance of the individual models with respect to the MAX-DOAS results. The authors should at least provide a table with some key indicators (e.g. correlation coefficient, slope, bias, etc.) for the individual model comparisons. These indicators should be provided for a) the complete time series, b) for the seasonal variation, c) the diurnal variation, and d) the weekly cycle.

c) The discussion of the deviations between the model simulations and the MAX-DOAS results is weak, and only rather general explanations for the disagreements are given. The paper would benefit a lot if the possible reasons for disagreement would be investigated in more depth. In particular, from the two points mentioned above, useful information could be obtained, which processes (e.g. transport, emission inventories, chemistry) might be most important reason for discrepancies for individual situations and/or models.

Minor points:

Page 1, line 1: Replace NO2 by NOx

Page 1, line 8: ‘measurements are available during daylight’. To me it seems that this is not an advantage but rather a disadvantage (measurements are not available during night)

Introduction: It should be made more clear, that the quantity of interest is NOx, but only NO2 can be measured

Page 2, line 30: The statement 'using zenith measurements as intensity of incident radiation' is unclear to me. Do you mean incident solar irradiation? Then I would disagree. Please clarify.

Section 2.1: What is the spatial resolution of the models? How does it compare to the horizontal sensitivity ranges of the MAX-DOAS results?

Section 2.2: The retrievals are described in an inconsistent and partly incomplete way. For example, for KNMI the retrieval procedure is completely unclear. Was a profile inversion performed or not? This section should be harmonised and completed. The effect of the different inversion procedures on the NO2 results should be briefly discussed.

Section 2.2: It is stated that for Uccle, cloud information was retrieved. Was this information also used for the selection of the measurements? What about the retrieval of cloud information for the other stations?

Section 2.3: How does the wind data compare to the wind fields used in the models? What about wind data for KNMI?

Page 8, line 22: 'Only those model values closest to the measurement time are used'. Why is no interpolation in time of neighbouring model output values performed?

Page 9, line 10: What is the vertical extension of the lowest measurement layer?

Page 9, line 12: 'comparisons of profiles'? No comparison of profiles is shown in Figs. 1 and 2.

Page 10, line 5: 'As the sensitivity of MAX-DOAS retrievals is largest in the boundary layer' Is this also true for the 'de Bilt measurements'?

Page 10, lines 23,24: 'On average, observed NO2 partial columns are higher in the

lowest observation layers during cloudy conditions compared to clear-sky conditions' I guess that no clouds are considered in the MAX-DOAS forward model. How reliable are then the MAX-DOAS NO2 results under cloudy conditions?

Page 11, line 3: What is exactly meant with 'correlation'? r or r squared?

Page 11, line 12: How consistent are the wind data from the weather stations with the wind fields used in the models? Can you show a similar plot as Fig. 6 based on the wind fields from the models?

Page 13, line 15: 'However, many validation points arise from the MAX-DOAS based comparisons which could improve model performance substantially.' This sentence is not clear to me. Please clarify.

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