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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of urban NO_2 concentrations and their diurnal and seasonal variations observed from space (SCIAMACHY and OMI sensors) using in situ measurements in Israeli cities" *by* K. F. Boersma et al.

Anonymous Referee #1

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This paper provides validation of tropospheric NO2 columns from the OMI and SCIAMACHY satellite instruments. In situ surface measurements from molybdenum converter instruments have been used for validation. Surface measurements benefit from long-term data that allow to examine monthly variation in satellite measurements as presented in the manuscript. The work (validation) like the one presented here provides the user community with an insight into the overall data quality and therefore is certainly useful. However, using surface NO2 measurements from molybdenum converter for the validation of tropospheric NO2 columns is complicated by (a) interference

in surface measurements and (b) the need for inferring column field from volume mixing ratio measured at the surface. The authors need to convince the readers that the approach they followed is robust and conclusions are valid. The current version of manuscript is weak in these two aspects and will require additional work. I recommend to publish the manuscript in ACP after the following comments are addressed.

General Comments:

It is not clear what motivated authors to focus just on 8 surface sites from Israel, while there are many stations in North America and Europe. The number of coincident measurements is still very small in contrast to what motivated authors to use surface measurements. I believe that authors would be able to find many measurement sites in developed countries. Why not extend the validation excercise to larger domain if they are looking for a large statistical data set for validation?

Authors rely entirely on the quantification of interference in molybdenum converter analyzers from the field campaign at Mexico City to correct for interference in molydbenum converter measurements at Isreali cities. This raises a number of questions regarding how robust the correction approach is. For example, the field situation at Mexico City can be very different than at Isreali cities. The MCMA field campaign was held in the month of April, and Dunlea et al., 2007 have not explored the interference in other months. The authors first need to investigate if they could apply the correction approach as described by Dunlea et al., 2007 to the measurements at Isreali sites. There are a number of possibilities- (a) Use simultaneous photolytic and molybdenum converter measurements from Europe and North America (and other regions if available) to examine if the correction based on ozone concentration matches with the difference between photolytic and molybdenum converter measurements. (b) The Isreali measurement sites likely provide the measurement of NO, which along with O3 could be used to calculate the photostationary steady state NO2. That would allow to infer the interference in molybdenum converter measurements. There are other approaches like the one used by Lamsal et al., 2008. Authors must be aware that insuffi-

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cient/inappropriate correction in the surface measurements would have large impact in the scientific intent of the manuscript.

In situ molybdenum converter measurements have been used to validate tropospheric NO2 columns from GOME (Ordonnez et al., 2006 and Schaub et al., 2006) that has similar overpass time to SCIAMACHY. They report interference in the range of 20-50% with smallest interference in winter and largest in summer. This is in contrast with the present study saying the lack of interference for morning interference (during SCIAMACHY overpass time) following Dunlea et al., 2007. This further suggests that the authors should consider reviewing their correction approach.

Authors assume vertical uniformity in the boundary layer and negligible concentrations above the boundary layer. It appears to me that the latter motivated the authors to compare tropospheric NO2 columns with BL columns. As a reader I would be interested to know what fraction of tropospheric column is in the boundary layer (and in the free troposphere). Can columns above boundary layer be really neglected? If that is the case it would then be logical to compare tropospheric and boundary layer columns. Moreover, assumed constant profile shape within the boundary layer could yield different conclusions. The manuscript would benefit from some sensitivity studies using the GEOS-Chem model, which they have used to understand diurnal and seasonal variation of NO2 columns, to examine the effect of these assumptions.

Specific Comments:

Title: Suggest removing "(SCIAMACHY and OMI sensors)" from the title and put the information in abstract. Alternatively, remove "space" and make "from the SCIAMACHY and OMI satellite instruments". Make "in situ surface measurements" instead of "in situ measurements".

Page 4304, line 28: Please include Winer et al., 1974 and Grosjean and Harrison, 1985 in the citation.

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Page 4307, line 15: What is the detection limit of the instrument (molybdenum converter)? Would the instrument be able to detect in the ppt level? Some measurement sites cap the lower limit to 1 ppb whereas NO2 mixing ratio in summer could be lower than 1 ppb.

Page 4308, line 8: What is the coincidence criteria that you generally apply? Your statement in Page 4309, line 2 implies that the coincidence criteria is different than 0.1 deg. How would your result change if you consider only the observations with surface sites located within OMI/SCIAMACHY pixels? Do you include all OMI/SCIAMACHY pixels? Did you examine how your results would change if exclude the OMI pixels at larger viewing angles? What are your criteria for cloud?

Page 4308, line 18: Would the correction of in situ measurements lead to increased correlation?

Page 4308, line 20: Annual average of 8% seems too small based on earlier studies. Relative contribution of other reactive nitrogen species is expected to be higher in summer leading to higher interference in summer. How do the corrected surface concentrations change with season versus uncorrected concentrations?

Page 4308, line 22: The number reported here (0.7 ppb) may be weighted by winter concentrations. It will be useful to provide the range of values classified by month or season.

Page 4309, line 13-14: BL NO2 column is surface concentration multiplied by boundary layer height. Would you expect to have different correlation between BL columns and tropospheric NO2 columns versus surface concentrations and tropospheric NO2 columns?

Page 4309, line 26: Apparently there is no grey dashed line in Figure 3.

Page 4310, line 6: Would not this seasonal variation caused by the change in NOx emissions?

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References: A.M. Winer, J.W. Peters, J.P. Smith, J.N. Pitts Jr., Response of commercial chemiluminescent NO-NO2 analyzers to other nitrogen containing compounds, Environ. Sci. Technol., 1974

D. Grosjean and J. Harrison, Response of chemiluminescence NOx analyzers and ultraviolet ozone analyzers to organic air pollutants, Environ. Sci. Technol., 1985.

Page 4316, line 5-13: Make 2008a and 2008b for the references of Boersma et al

There are few references in the list which are not cited in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4301, 2009.

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