**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Satellite NO$_2$ observations and model simulations of tropospheric columns over South-eastern Europe" *by* I. Zyrichidou et al.

Anonymous Referee #1

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General thoughts: The authors present an analysis of satellite and model NO2 columns over Southeast Europe. Generally the paper is fairly well-written and the figures are clear with sufficient text. Whilst the paper addresses an issue that has generally been overlooked, I hardly think it will set the world alight either. I think where the paper suffers is on two main points: (a) the satellite observations are not used to infer anything about surface NOx emissions and (b) the CAMx model is hampered by the fact it doesn't contain any emissions from biomass burning and lightning. Without a discussion of the likely impact of these sources on the NO2 columns within the studied domain it is difficult to assess the suitability of the CAMx model for interpreting the satellite data. Also, I think there should be more discussion of the NO2 variability at the different sites.

For example, are there any surface measurements at these sites to compare too and what are the likely local NOx sources that have the most (if any) impact on the satellite columns?

I think the manuscript warrants publication in ACP but only after my comments (and those of other reviewers) have been sufficiently addressed and the paper revised. I would advise the authors to look at the work of Martin et al. (JGR, 108(D17), 2003) who used their own GOME retrievals in combination with GEOS-Chem CTM to infer a global inventory of NOx emissions, and to use this work as a lead.

Specific Comments:

Abstract, Line 22: Typo, should be -0.1% not -0.1.

Page 12173, Line 5: A more recent reference in addition to Crutzen, 1979 would be beneficial.

P12173, L9-10: Please insert references regarding the OH radical as the cleansing agent of the troposphere and the toxic effects of NO2 and O3.

P12174, L4: Typo. It is CHartographY.

P12175, L8: Style. Shouldn't this be 'polluted air-masses......Black Sea, leads to an increase in NO2'.

P12175, L26: Please explain why the spatial distribution is not 'proper'. Are there any stations at all in this region?

P12176, L18: What are the criteria for determining an 'unpolluted city'?

P12177, L5 and thereafter: Capitalization. It should be Equator not 'equator'.

P12179, L24: What do you mean by un-flagged? Please be clearer.

P12179, L31: Why were only cross-track pixels 10 to 50 used in the OMI retrieval?

Section 2.3: I found the description of CAMx model somewhat confusing. For instance,

the run is based on a 'coarse grid spacing' which has a spatial resolution of 50 × 50 km2 (i.e., of order 0.5 × 0.5 degrees which is still quite fine for chemistry-transport model). Also I think the lack of lightning and biomass burning NOx emissions within the CAMx model is a significant shortfall. The authors should at least provide some information on the typical periods and intensity of biomass burning and also on the lightning statistics over this region.

Section 3.1: I think a discussion of the variability over 4 locations, albeit that are representative of different conditions, is simply not enough. The main focus of the paper after all is the variability of NOx over this region. For example, showing the time series plots at all locations would make it easier for a reader to gain a better understanding of the NO2 variability.

P12185, L23: Typo. Correct the spacing between: '13:30 to 10:00 UT'. This also occurs in a couple of other places. Pease check the rest of the manuscript carefully.

Section 3.3 If the spatial resolutions of GOME and CAMx are not really comparable, why wasn't the comparison performed with just SCIAMACHY or OMI measurements from the just the TEMIS retrieval? I think adding a brief comparison of the model to these instruments would be beneficial to the paper.

P12187, L24: Typo. I think there is a missing x10ˆ15 at the beginning of the line.

P12188, L3-5: Please define the normalized mean bias. Also there is significant difference between the TEMIS and Bremen retrievals over industrial areas (-4% versus 30%). I think this warrants more explanation.

P12188, L15: Typo. Insert space between '(left).Each. . .'.

P12188, L25-27: The authors state that the EMEP emissions over Istanbul were unrealistically low and subsequently that this location was excluded from Fig. 7. I think this is where the real science of the paper is. Surely, the authors should be using the satellite observations to constrain the model emissions. At the very least they should

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provide a better estimate over Istanbul by perturbing the emissions (in a brute- force approach) to obtain the best match between the model and satellite NO2 columns.

Figure 7: 31 is not the number of 'observations' it is number of the locations. Also I think this figure would be better if 'raw' concurrent measurements were plotted instead of the daily average over 1996-2001 (since the averaging will smooth out the variability and therefore the true performance of the model).