Atmos. Chem. Phys. Discuss., 6, S2761–S2766, 2006
www.atmos-chem-phys-discuss.net/6/S2761/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of African air pollution on regional and global tropospheric chemistry" *by* A. M. Aghedo et al.

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Received and published: 28 August 2006

This paper employs a global chemistry-climate model to examine the influence of emissions from Africa on regional and global ozone. It focuses on a topic which has not been considered much in previous literature, and which is interesting enough to be able to make a nice publication. Among the interesting results is the large degree to which the influence of African emissions occurs downwind of Africa, and the contrast between the dominant effect of biomass burning emissions over Africa, compared to the dominance of biogenic VOC emissions on influencing ozone outside of Africa. In principle this is a nice start to a potentially good study, but the paper currently fails to really investigate the issue deeply enough to be of sufficient quality for publication in ACP. This can be easily remedied if the authors extend their analysis, especially to consider other gases

besides ozone, trim many of the less interesting details, especially about seasonal variability, and correct a major misconception throughout the discussion. After these improvements the paper should be acceptable for ACP. Detailed recommendations are given below.

Major comments:

1) The title of the manuscript is “influence...on tropospheric chemistry”; however, the focus of the analysis is exclusively on changes in ozone du to removing various emissions. Considering other important components of atmospheric chemistry, such as CO, NOx, and OH, would strengthen the manuscript considerably and fit better with the title.

2) Throughout the paper, the term “contribution” of various emissions to the total ozone burden is used. This is not a correct interpretation of these types of simulations. This does not at all devalue this type of study, it just means a more careful interpretation and more accurate discussion is needed. When emissions are removed, or even slightly perturbed, in a nonlinear chemical system, feedbacks result in a change in other gases which can be either larger or smaller than the actual contribution of the individual emissions. The only way to properly assess actual contributions is through tagged tracers, but that is known to be difficult for ozone. These simulations in which emissions are removed show the net influence of those emissions within the full chemical system (thus, the sum of influences from all emissions could be more or less than 100%). This is still very informative, for instance politically, for understanding how the chemical system would change if certain emissions were increased or decreased, though there are secondary non-linearities which also influence the interpretation. A careful discussion of this with examples for NOx and a quantification of two types of nonlinearities for India and its outflow is given in Kunhikrishnan, T. and M. G. Lawrence, Sensitivity of NOx over the Indian Ocean to emissions from the surrounding continents: nonlinearities in atmsopheric chemistry responses, Geophys. Res. Lett., 31, doi: 10.1029/2004GL020210, 2004. This should be cited in section 3, in the paragraph “We are aware...”, along with

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the seminal works on this by Prather and colleagues, and used to help with properly formulating the discussion.

3) Although I am a strong supporter of the need for proper evaluation of models (just like the characterization of measurements instruments) before they are put to substantial scientific use, the short model "validation" given in section 4.1 is not very convincing of the quality of the model for this study. The free troposphere, as indicated in section 4.1, is indeed often in reasonable agreement. However, the surface, which is the topic of section 4.2, but which is not mentioned in 4.1, disagrees substantially at several locations, including those in Africa (Johannesburg, Cairo, Lagos...), and even has the wrong lower tropospheric gradient over Johannesburg in July. There is no indication given of how all the precursor gases (NOx, CO, etc.) behave, so it would not be possible to diagnose the cause of these deficiencies. I would therefore suggest to remove this brief comparison with the MOZAIC data, and replace it with a short summary of the salient points from the more substantial evaluation which is indicated to be currently in preparation by Rast et al. Note also that the term "validation" is a misnomer which is frequently used in this context; it implies that one believes the subject of validation (in this case, the global chemistry-climate model) is actually correct, and comparisons with observations are being sought to demonstrate that this is so. The more proper term is "evaluation", which implies that one is determining the strong and weak points of the model, indicative of the current state of the science and model development.

4) In the conclusions the advances of this paper beyond Marufu et al. (2000) along with the points from that study which are supported should be summarized (this is mentioned in various places in the text, but it is important to place this in the literature by summarizing this in the conclusions).

5) Why is only biogenic VOC considered, and not biogenic NOx (soils) in the sensitivity run? Further, In Table 3 it appears that other sensitivity runs considering biogenic CO and NOx were indeed conducted, but not discussed properly in the paper (only alluded to in passing once in the results); this should either be incorporated fully into

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the discussion, or removed from the table.

6) Most of section 4.4 could be dropped in the interest of an improved analysis elsewhere; starting around p. 5812 it is mostly descriptive without much interpretation, and the sizes of the influences (tenths of Tg) are small enough that the seasonal variation in them is not really relevant compared to the other issues discussed in the paper; only the last paragraph of the section becomes more interesting, but it would need to be supported by a meteorological analysis (e.g., vector plots of wind patterns) to make a significant statement. For the rest of the section, there are indeed a few interesting points, but the summary that is given in the conclusions would be enough to bring these across well.

7) The inclusion of interannual variability to the study is, as pointed out by the authors, an important advance over previous work. This section should be made more in proportion to its importance. In particular, characterizing the reason for the large interannual variability of the influence on southern Asia would be very interesting. I suspect this is related to shifts in the southward excursions of the ITCZ, and to the transport in plumes in the trade winds during the monsoon transition periods, so that this could be nicely related to the satellite observations discussed in Kunhikrishnan, T., M. G. Lawrence, R. von Kuhlmann, A. Richter, A. Ladstätter-Weißenmayer, and J. P. Burrows, Semi-annual NO2 Plumes during the Monsoon Transition periods over Central Indian Ocean, Geophys. Res. Lett., 31(8), doi: 10.1029/2003GL019269, 2004. Also, the last paragraph discussing the interannual variability in emissions is interesting, but given the results in the rest of the study, it would be useful to indicate how this might translate into effects on ozone.

Minor comments:

1) In the abstract, it would be nice to give the relative amounts of African ozone which the changes due to each of the emissions represents (e.g., about 8% for biomass burning, etc.)

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2) Abstract: “about 70%” - I calculate exactly 80% for the values in the table

3) Abstract: Rather than listing Russia and other countries (and missing many of the former Soviet states), it would be better to say “northern North America, northern Asia, and Europe” (here and elsewhere)

4) The choices of what to discuss in the introduction seem a bit scattered to me, e.g., why mention methanol but not other emissions (e.g., butane)? It would help to tighten down the introduction and really focus on what is relevant for Africa (and indicate why where possible).

5) P. 5801, l. 21-22 “is henceforth referred to as Rast et al. (2006)ˆ2” is not needed, since this is just the normal citation with footnote.

6) Section 2.4: has the lightning distribution been evaluated for these particular runs? (it can change a lot between different horizontal and vertical resolutions, for example in terms of land/sea contrast)

7) Section 4.2: “This corresponds with the relatively high contribution of these countriesĚ” - how large is the contribution of each country? Is the relative effect on ozone (and other gases) disproportionately small or large (e.g., due to the concentration of emissions in one region)? This is an example of where deepening the analysis would help make it much more interesting.

8) Section 4.2: the last paragraph seems to be the opposite of the the sentence on p. 5808, l. 21; these should be synchronized.

9) Why assume the tropopause at 200 hPa? This is a bit oversimplified and it is straightforward to do it better (although it might not make too much difference, but with the effect of biogenic VOC being so strong in the TTL on the other hand it might).

10) Section 4.3: “Over the continental regions outside Africa, the African biogenic contribution to the ozone burden is two times that of the biomass burning” - why is this? Adding analysis of the precursors might give some insight.

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11) P. 5811, l. 19-23; these are partially redundant with the previous paragraphs, should be merged.

12) P. 5816, l. 7: Lawrence et al. (1995) gave a range of 1-8 Tg(N)/yr, not an upper limit of 20; if the citation is moved to after "too high", then the sense of the sentence is accurate; another recent study giving strong evidence of an upper limit nearer to 10 Tg(N)/yr is Labrador, L. J., R. von Kuhlmann, and M. G. Lawrence, The effects of lightning-produced NOx and its vertical distribution on atmospheric chemistry: sensitivity simulations with MATCH-MPIC, Atmos. Chem. Phys., 5, 1815-1834, 2005.

13) Grammar (which is generally excellent): replace "at" with "in" anywhere that a region of the troposphere or a hemisphere (NH/SH) is mentioned, e.g., p 5800, l. 6, or p. 5808, l. 11

14) P. 5801, l. 5: "conditions" (plural)

15) P. 5816, l. 17, add "emissions" after "anthropogenic"

16) Table 1 and Table 2 would be nicer merged into one table, with the relative contributions (in percent) of the African emissions to the global emissions being given.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5797, 2006.