Atmospheric Chemistry and Physics Discussions

Interactive Comment

Interactive comment on "Technical Note: A diagnostic for ozone contributions of various NO$_x$ emissions in multi-decadal chemistry-climate model simulations" *by* V. Grewe

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The Referee #1 raised two specific points, which made clear that some points have to be better explained in the masuscript.

First specific point:

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The referee argued that the box model simulation does not represent an extreme case and had some questions concerning the wah-out. The box model includes wash-out in both boxes, which removes HNO3 partly from the box. An 100% removal of HNO3 is assumed in a part of the box, which is covered by clouds. This coverage is in general below 1 in a climate model. Both boxes of the simple model experience wash-out at

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different times, in order to maximise the different character of the boxes. However, since the reviewer and other colleagues criticised that point, I will add a further figure showing the effect of varying the parameters for Box 2 from 0.5 to 7 days lifetime of NOx and 0 to 100% wash-out per event. The mean difference between the NOx and NOy contributions is less than 10.5% in all cases and less than 5% in the majority of the chosen parameter range. This will be been included in the description of the methodology. Therefore the refere is right that the chosen parameters do not represent the most extreme case, but the deviations are still acceptable since they are in the order of 10% and in most cases far below.

Second specific point

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The referee pointed out that an increase of the emissions by 5% is probably not appropriate.

One cannot expect to get the same results from the two discussed estimates for the contributions. The 5% perturbation simulations changes the background chemistry and therefore always leads to non-linear behaviour of the system, which has often been discussed. The NOy contributions add up to 85% to 110% (see introduction), which in principle inhibits a correct calculation of the contribution. The smaller the emission deviation the more linear should be the response. This would imply that a reduction of the emission perturbation from 5% to perhaps 1% also reduces the non-linear character. But then the calculation of the contributions gets more and more numerical inaccurate, since it is basically the quotient of two differences, where the differences approaching 0 by decreasing the perturbation percentage. Without further proof (since this would again mean a number of simulations and therefore a lot of computer time), I assumed that a 5% change is a not too bad choice. This will be clarified in the text passage, where the referee referred to and also in the introduction. It leads to the conclusion that both estimates have several disadvantages, which were

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discussed in the introduction and that therefore one cannot conclude, which is superior. The main point is that it is not possible to exactly calculate the contributions and that therefore both methods (1) and (2) are equally good or bad. The more important point is that the method is capable to correctly diagnose trends, which is shown in the second error analysis and which has also been pointed out by the reviewer. Another important point is that for a fully coupled climate chemistry simulation all greenhouse gases affect the radiation. Any change in a NOx emission produces a different ozone field, which changes the radiation and therefore meteorology. This doesn't mean that theres a significant change in climate but the sequence of weather patterns has simply been changed caused by the chaos, which is inhere in the climate system (and also modelled climate system). The detection of small ozone changes is then no longer possible, since the natural variability of ozone is superimposed.

Minor comments:

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The difference between the two methods will be more detailed discussed in the Introduction. The apporach (2) assumes that from n molecules produced at any time step $n_i > 0$ molecules can be attributed to the Emission i. The basic idea is that to produce ozone NO_2 has to be photolysed. It doesn't take into account that the source also has other emissions like CO etc., which is currently not included in the model. And all molecules experience the same background chemistry. In the case of an emission reduction the background chemistry changes, so that in principle also negative contributions can occur, because the production efficiency of one N molecule is changed.

The suggestion of the referee that (1) is more appropriate for impact of one source and (2) contributions calculations is a good proposal, which I will includ in the text.

As soon as one source dominates in one region the contributions of the others are very small and small errors in the transport can give large relative errors or even a change in the contribution, which is not possible using this approach (see also new text

in the introduction), but occurs due to numerical diffusion and inexact calculations of the contributions (wil be included in the text). That's the reason why in those regions the data are excluded.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 327, 2004.