Atmos. Chem. Phys. Discuss., 4, S125–S128, 2004
www.atmos-chem-phys.org/acpd/4/S125/

Atmospheric Chemistry and Physics Discussions

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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Received and published: 24 February 2004

1. General comments

This paper suggests a new and computionally efficient method for the calculation of ozone contributions from various NO_x sources. This is required for long multi-decadal simulations with chemistry climate models, for which existing methods are computionally too expensive. This new method is based on the assumption that each source contributes proportionally to the total NO_x and NO_y concentrations. For each NO_x source the contribution to the ozone concentration can then be estimated from the chemical ozone production rate and the ratio of the amount of NO_y from the specific source over the total NO_y concentration. This method constitutes a simple tagging of

NO_y and ozone only and is consequently very efficient.

This proposed methodology is innovative and is indeed very helpful, if not necessary, for long chemistry-climate simulations. Moreover the authors present their method along with a adequate error analysis that demonstrates to what extent the method is valid. Nevertheless, there remain two issues that need further clarification, which I will discuss in the next section.

2. Specific comments

First issue concerns the basic assumption. Although is intuitively acceptable that the assumption is approximately true, the two-box model example does not convince completely. Figure 1. shows very high relative contributions that steadily increase with time without very strong fluctuations. To my opinion the 2-box model does not represent an extreme case, since this would not be determined by the chemical lifetime of NO_x alone, but also by other processes of which wet removal is likely to be very important. Incidents of strong washout can almost completely remove all nitric acid, thus possibly destroying the required fixed ratios. This depends whether the relative contribution on nitric acid is also close to that of NO_x. If the ratio is detroyded, it is not clear if such an event compares to the initial conditions of the two-boxmodel example. If so, one could argue that the assumption still holds for such cases, since deviations of 10% are acceptable.

Last and most important issue concerns the comparison of the new methodology with a method based on emission increases of 5%. The 2nd error analyis, according to Eq. 9, does convince that the new method is compatible with the 5%-increase method. However, the first part of the error analysis rises the question whether the 5%-increase methodology is adequate at all. This doubt stems from the fact that only points with scaling factors of more than 2 and less than -0.5 are removed from the analysis. This

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means that large non-linearities are kept in the analysis and that perhaps much smaller emissions increases are needed to obtain approximate linearity. So if the 5%-increase method is not valid for most/large regions of the atmosphere, then the usefulness of the new methodology remains questionable. Moreover, the author seems to admit that this is the case (page 333, lines 23-26).

3. Minor comments

The 5%-increase method is introduced as a form a method (1). However, this not entirely consistent with the discussion on the differences between methods (1) and (2). The small increases of 5% aim to keep the non-linearities at a minimum in order to obtain approximately the same results as with method (2). Perhaps this could be mentioned in the introduction for clarity.

Also some words about how the differences between the two traditional methods should be interpreted, would be useful. For instance, one could argue that method (1) is more suitable for calculating the impact of one specific source, since the impact of a source is also determined by the induced change in chemical regime. On the other hand, method (2) seems more appropriate for assessing the contributions of different sources for a 'real' (current or future) atmosphere, since all sources together determine the chemical state. However, this is not entirely straightforward, as the chemical state is not only determined by NO_x, but also by CO and hydrocarbons. For instance road traffic, in contrast to lightning and air traffic, emits large amounts of CO and NMHCs.

It is not clear to me why regions with negative contributions or dominant stratopsheric ozone productions have to be omitted. Although it is a fact that all studies indicate that monthly mean contributions are positive, one cannot be sure that this will be the case for any future scenario. How do dominant stratospheric contributions affect the analysis?

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4. Technical comments

Table 1 should not only contain symbols but also what they represent. I assume that in the final version this table will be in the appendix, otherwise the reading of this part of the paper requires a lot of going forward and backward through the text. Perhaps it is a good idea to use the same conventions for the 2-box model as used elsewhere, namely that NO_x is x and NO_y is X.

Abstract, line 11: I (still) think that it must be relative contributions.

Abstract, line 15: absolute.

Introduction, page, 329, line 19: analysis of the errors.

Throughout the text indices such as n and i should be placed in italics.

Error analysis, page 331, line 16/17: suggestion to replace 'performed, where ...' by 'with the emissions slightly increased by 5%'.

Error analysis, page 332, line 14: underestimate.

Conclusions, page 336, line 13: local.

Table 1: exchange (T).

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