Geosci. Model Dev. Discuss., 5, C632–C635, 2012
www.geosci-model-dev-discuss.net/5/C632/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "Tagged ozone mechanism for MOZART-4, CAM-chem, and other chemical transport models" *by* L. K. Emmons et al.

M.J. Evans (Referee)

mat.evans@york.ac.uk

Received and published: 3 September 2012

This paper describes a method for 'tagging' the O3 produced from the emissions of NO from a variety of sources. It appears to offer a flexible and comprehensive approach to this problem. Given the important of trans-boundary air quality problems and the attribution of O3 related radiative forcing to different emission sources this is a very useful and powerful tool in the armory of diagnostic techniques for atmospheric chemistry models.

The paper should be published, however, there is still in my view, a bit of a mystery as to why the perturbation experiments give a much lower value for the long range transport of O3 than the tagging approach adopted here. My suggestions for the paper would be a little bit more investigation of this problem and a slight reduction in the number of

figures.

Overview

The attribution of O3 to different NO emissions sources in a non-trivial activity as the relationship between NOx concentrations and O3 production in non-linear. Previous efforts to target this problem have used either a series of perturbation studies or have tagged the O3 produced over a certain region. Both approaches offer significant limitations. This paper describes an approach that allows the O3 produced from the emissions of NOx to be tagged directly. This is a significant advance in our ability to diagnose models both for science but also for policy activities. The authors present the a generally good description of their approach and show extensive figures of their diagnostics. However, I would suggest they think about whether they can reduce the number of figures, as there appears to be an overwhelming number.

Major Comments

Non-linearities The most interesting aspect of this work is the inconsistency between the O3 produced from a certain NO source calculated by this approach and the O3 calculated by the equivalent perturbation study. This difference has significant policy implications. This approach suggests a much more significant fraction of the O3 being due to trans-boundary transport than the perturbation studies suggest. This is a very important result and one that needs to be disseminated to the science and policy community.

Assuming that there this not a bug in the coding, the explanation for this discrepancy is hard to understand. The authors suggest that this is due to the non-linearity of the chemistry. However, previous literature seems to suggest that may not be the case. Wild et al., [2012] (Modelling future changes in surface ozone: a parameterized

GMDD

5, C632–C635, 2012

approach Fig 3) suggests that the 20% changes should be effectively linear and much larger changes are needed to get a significant non-linear response. It may be that the model used here has a more-significantly non-linear response than the one in the Wild study. The model has a more complex hydrocarbon oxidation scheme but it is not obvious that this would cause this behavior.

The fact that the addition of the individual tracers NOx tracers gives the same result as the case for the simulation without the tagging suggests that there isn't a major problem with the methodology.

A set of perturbations with smaller and larger perturbations (say 1,5,10,50%) would show how non-linear the model response is and would significantly strengthen the author's claims that the non-linearity's are the explanation for the discrepancy. At the moment I don't find this explanation convincing on the evidence presented.

Whatever the case this work has a significant implication for public policy. I am slightly concerned that it will get lost within a journal that emphasizes model developments rather than more traditional science. The authors should ensure that they have a good strategy for publicizing this result to both the science and policy community as this has significant results for both.

Description of the methodology It would be possible for the tagging to be implemented in 2 ways. The 1st would involve running the standard chemistry and the tagged chemistry in parallel with the standard chemistry impacting the tagged chemisty but not vice versa. The 2nd way would involve only having tagged tracers which interacted with each other and the wider chemistry scheme. As I understand it the methodology uses the 1st approach. A couple of sentances to make this clear would be useful.

Minor comment

There appear to be random numbers after each of the references in the reference list

GMDD

5, C632–C635, 2012

Interactive comment on Geosci. Model Dev. Discuss., 5, 1949, 2012.

GMDD

5, C632–C635, 2012