Atmos. Chem. Phys. Discuss., 12, C8366–C8369, 2012
www.atmos-chem-phys-discuss.net/12/C8366/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future impact of traffic emissions on atmospheric ozone and OH based on two scenarios" *by* Ø. Hodnebrog et al.

Anonymous Referee #2

Received and published: 18 October 2012

This is a well written paper further addressing the question of how much different traffic sectors (road, ship and aircraft) contribute to the radiative forcing. All models involved use the same meteorological fields, which presumably make the results dependent mostly on the emissions. There are however some issues that need to be addressed before the manuscript is ready for publication, as discussed below. I will preface my comments by saying that a lot of the details of the manuscript need to be found in other publications. This makes sense, except that I did not have time to go through all the other papers to answer my questions. The authors may consider adding some clarifications in areas where there are questions to clarify, as discussed below. Finally, this is a question for the editors. This manuscript is essentially the same as previous one. The main difference is that (as the authors state), it considers a different emission scenario (A1), and puts the results in the context of other results. Methodology, etc.,

seems to be the same as in previous papers. So my question is whether this is sufficient for a publication? Assessment exercises run a host of different scenarios, and I am not sure that every time there is a new run there is a need for a new publication. 1. One of the main problems I have that needs clarification is how they treat the ship emissions. There has been some work done on this, since diluting the ship emissions to the whole grid size results in producing large amounts of ozone, which disagree with some observations. This also impacts the OH. As a matter of fact, I am curious about what the methyl chloroform lifetime is when the treatment of ship emissions is included (maybe it is in one of the other papers?). In any case, I think that the authors should discuss this uncertainly. A recent paper on this issue is, for example: Vintken et al., Atm. Chem. Phys., 11, 11707-11722, 2011. This is an important issue, since it is the ship emissions that change the sign of the radiative forcing in some of the scenarios considered. 2. Page 20981, lines 14-23. This paragraph makes an important point. However, there is another element that is not described, mainly what is the numerical advection algorithm used by the different models. This could affect the latitudinal and longitudinal distribution of the perturbations (see below). 3. Page 20982, line 23, and Figure 2, etc. There is an inconsistency here: the text says that in the perturbed scenarios the emissions are reduced by 5%, but the figures show increases in NOx, ozone, etc. Which one is it? Also, given the above ambiguity, the authors should clarify what kind of perturbation they get when the results are scaled to 100%: is it taking out the aircraft completely, or doubling the emissions? 4. Figure 2. What are the authors showing here, i.e., what is the difference between the color contours and the white lines (if the white lines is the change relative to the BASE run, what are the color contours?). Also, "the right column shows zonal mean perturbations for all tansport modes". Do you mean for the corresponding sectors on the left-hand column? 5. Again in Figure 2: The ozone perturbations show a maximum over the pole, whereas the strong corridors are at lower latitudes. Other model calculations (see, for example, the results in the IPCC Aircraft assessment) show more of a maximum at around 60N, similar to what the authors see for OH. Any idea why this is? Could this be a numerical transport issue? 6. Page 20985, around line 15. The O3 production increases with NOx up to a center value, and then it turns around and decreases. The implication here is that the NOx is higher than the “turn around” point for the UT/LS conditions? Which is this value, approximately? It may be useful to give the background values of NOx, since they may be different for the different models, and they would impact the non-linearity. Along these lines, what are the different models using for lightning NO production? 7. The robustness of the scaling approach for the RF also implies that the scaling factors were also derived from a complete radiative transfer calculation that had the same cloud field. Is this the case? (I am assuming that these are not clear-sky RF). One could also question whether the scaling would hold for perturbed scenarios such as A1 in 2050. Also, is the RF instantaneous, or is the stratosphere relaxed to equilibrium? 8. Page 20990, lines 19-21. The change in RF per DU of ozone would presumably depend on the altitude profile of the ozone change. Is the factor quoted here for a decrease in the UT/LS, or uniform? How uncertain is this scaling factor? 9. Page 20992, lines 12-15: The statement is made that the results of Lee et al. for the RF are higher “because they removed all aircraft emissions, whereas we used a 5% perturbation approach”. I presume that the RFs shown are relative to a “clean” background, with no aircraft. Now, a statement is made above that scaling the 5% results to 100% is a good approximation, but here the implication seems to be that one would get different results if one did a run with 100% reduction. So which is it? In general, this points to the need to be very clear as to what they are calculating, and consider the dependence of scaling, parameterization, etc., on the background atmosphere. 10. Table 4: What is the meaning of “the history of emissions being taken into account”? This suggests a time-dependent simulation. 11. Tables A1 and A2 are very useful. One of the problems with assessment studies in general is trying to diagnose why model results are different. I would suggest that the impacts on lifetime be further broken down into the categories in Table A2. What is the impact of methane changes on its own lifetime? In addition, my understanding is that the change in ozone due to the change in methane is calculated from the scaling factors given in the text, and the results are given in Table

A2. However, if we scale down the O3 because of the changes in methane, this would also change the OH and thus the lifetime. Is this included anywhere?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20975, 2012.