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Atmospheric Chemistry and Physics Discussions

Interactive comment on "TRADEOFFs in climate effects through aircraft routing: forcing due to radiatively active gases" *by* F. Stordal et al.

Anonymous Referee #3

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This is an interesting paper dealing with potential climate effects from changes in air traffic routing. The paper is worth publishing after revision. Unfortunately, the main conclusion of the paper is that "we are not yet at a point where we can include non-CO2 effects of aviation in emission trading schemes". This is not a surprise considering the large differences between the models, already shown in the evaluation papers published earlier in Atmos. Chem. Phys. by Brunner et al. (referred to in the present paper).

The somewhat depressing fact that models are disagreeing about climate effects from air traffic does not mean the paper is useless but I think it could be improved significantly (and become more useful) if some things are looked at in a bit more detail.

Four models are included in the paper. As far as I can tell, only one of them, the Oslo CTM2 model, is really fully "fit for purpose". All the others have some, more or less serious, drawbacks for the, rather complicated, issue of investigating impacts of air traffic at different altitudes on ozone and methane concentrations. The LMDz/INCA model only includes background tropospheric chemistry (missing VOC-chemistry for "polluted" parts of the atmosphere and containing no stratospheric chemistry). The ULAQ model has a very coarse resolution (both vertically and horizontally). The TOM-CAT model lacks stratospheric chemistry (as far as I can tell) and no radiative forcing calculations were performed using the results from this model.

Calculations have been performed for a base case scenario (year 2000) and three different alternative routing scenarios (+2000ft cruising altitude, -6000ft cruising altitude and increased polar routing; the polar scenarios where only investigated by two of the models).

For the base case scenario the models are in reasonable agreement, for aviation impacts on ozone columns and methane lifetime, but for the alternative routing scenarios the deviation is large. The results are not even in qualitative agreement with each other. I think that the authors have not managed to explain the reasons for the differences in an acceptable way yet.

For example, for the Lower cruise altitude scenario three of the models predict a decrease in ozone column while the fourth (ULAQ) gives increased ozone columns in the whole Northern Hemisphere. For the Higher cruise scenario the ULAQ results (for the O3 column) are not so very different from the OSLO CTM2 results. When it comes to explaining the results (for the Lower cruise scenario) the authors only state that: "Like in LMDz/INCA the ozone change is negative in Oslo CTM2 and TOMCAT as expected, due to a shorter lifetime for NOx as well as ozone. However it is positive in ULAQ, which is more difficult to explain." I think that the authors should at least try to explain the results. It would be very interesting to know at what altitudes the ULAQ increase in ozone occurs and not only showing the total ozone column effect. In general I think the

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study would be more valuable if some more information about the vertical distribution of the effects were presented. The models have fairly different resolutions (both horizontally and vertically) and it may be interesting to do some test calculations of how the vertical transport differs between the models (that is some type of passive tracer simulation).

I think that the Polar routing scenario should be removed from the paper since only two models (Oslo CTM2 and ULAQ) performed these calculations and since the scenario is very poorly described. There is, as far as I can see, no description of which routes are increased and decreased in this scenario! (There is only a statement that "some flights are moved from these [middle latitudes] to higher latitudes") Also, it seems strange to assume no changes in CO2 emissions when increasing the polar routing. I thought one of the main ideas behind increased polar traffic is to cut transport distances and thus cutting fuel consumption? It is also very difficult to draw any conclusions from the Polar scenario results in the paper since the differences are very large between the Oslo CTM2 and the ULAQ results. The ULAQ results shown in Figure 3 are quite remarkable with almost as large effects in the southern hemisphere as in the northern. When it comes to explaining the differences between the two models the authors only suggests: "The other model (ULAQ) has a coarser spatial resolution that most probably explains the model difference." I can not understand how this explains the qualitative differences, which are very large (see Figure 1 and 3). Is it possible that some of the more elaborate heterogeneous chemistry in ULAQ could be influenced by the aircraft emissions and lead to more ozone losses? I think polar routing is a very interesting, and important, issue for aviation research but I believe that the authors should remove this part from the present study and "redo" the work, preferably with more than two models, and prepare a separate paper on this subject.

Some other points:

Did you only consider NOx (and CO2) emissions from air traffic or were emissions of sulphur, soot and/or VOC also included?

Could you specify in a little bit more detail how the changes in methane were derived from the changes in OH? That is, what CH4 data were used and what assumptions were used regarding temperatures etc. for calculating the OH-reaction rate?

Regarding the LMDz/INCA GCM (2.2) It is stated: “A zonally and monthly ozone climatology is prescribed above the tropopause based on Li and Shine (1995)”. This seems rather strange. Does it mean that all ozone impacts in the stratosphere are neglected in this model?

Regarding the ULAQ CTM (2.3) This model seems to be developed specifically for studies of aerosol species. The text indicates that apart from O3 also H2O, BC and sulphate fields were used for the RF calculations. It would be interesting to know how large the air traffic impacts on RF through H2O, BC and sulphate are, compared to the O3-effect.

Section 4.1 Base case 2000 There is no mentioning of the ULAQ results in the first section. The other three models are mentioned and there is a statement: “In the three models the lifetime change is in the range”. I get the feeling that the study started with only three models included and that ULAQ was forgotten when this part was written.

Section 4.4 Higher cruise altitudes. 6th sentence: “the model that predicted ozone decrease in the previous case” Should be increase instead of decrease

Reference 10 (Brunner et al.): correct starting page number is 1609 (not 1606)

In the figures the TOMCAT model is denoted UCam. This is not stated anywhere in the text or figure captions and it could be a bit confusing for the reader.

There seem to be some inconsistencies between Table 2 and Figure 1. For the Base case the UCam model seems to have the highest impact on ozone columns in Figure 1 but the TOMCAT model has the lowest impact in Table 2. (E.g., the UOsl model clearly has a lower ozone column impact at all latitudes than the UCam model in Fig1 but still the value in Table 2 is higher for the Oslo model.) Perhaps there is a misprint in the

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Table? There may be a similar error for the Higher cruise altitude. The LMDz/INCA model has the highest impact in the table but in Figure 1 it looks as if the UCam model has a much higher impact at all latitudes $<$ ca 50N and from the figure I guess that the global average for the UCam model should be considerably larger than for the LMDz model.

Table 6 can be removed since all of the data are included in Table 7.

In Table 7 (and of course Table 6) it can be misleading to state that the RF resulting from CO2 in the additional polar route scenario is 0.00. As I already mentioned, this is a strange assumption and if this figure is given in the Table it should be with a footnote that this was prescribed in the scenario. It would have been more interesting if the authors had made an estimate of how much the CO2 emissions could decrease due to increased polar flights, to be able to say something about the possible climate effects of using these routes.

It would be interesting to include the results from the TOMCAT model (for the ozone column) in Figure 4. The differences between the other models are so large that the addition of the TOMCAT results would be valuable. Does the seasonal variation in TOMCAT look like any of the other models? And what about the inter-hemispherical transport? Figure 1 indicates that TOMCAT has larger impacts in the southern hemisphere than in the northern! I think this should be discussed in the paper.

Why is there no “Figure 5” showing the impacts on ozone columns from the Higher cruise altitude scenario in the same way as for the Lower cruise altitude in Figure 4?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10733, 2006.