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Interactive comment on "Aircraft routing with minimal climate impact: the REACT4C climate cost function modelling approach (V1.0)" *by* V. Grewe et al.

Anonymous Referee #2

Received and published: 16 October 2013

This GMDD paper describes a massive research effort to combine various classes of models that collectively evaluate the impact of an aircraft flight routes on climate. The mix of atmospheric chemistry and transport, aircraft simulators, and radiation-climate codes is impressive, clearly a result of extensive collaboration of the co-authors on the REACT4C FP7 project. The connection across models is new and highly valuable to the community. In my view the paper should proceed to GMD, but there are a number of worries that I really wished the authors had dealt with. Some are trivial fixes, but some would require much more extensive (and intensive) analysis of this modeling system that may not be possible – I leave that decision up to the editor. Either way, it would be helpful to address the major issues of closure, conservation, time scales of

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perturbations, and uncertainties.

Can GMDD or the authors please use continuous line numbering as it is almost impossible to keep track of and type in the endless page number here?

4346-3 I thought that most persistent (ie, important) contrails were formed by the cold snap in temperatures in the wake vortex and NOT by the added H2O. In fact the added H2O does nothing unless we are supersaturated w.r.t. ice.

4346-23ff The sensitivity analysis and sanity checks have failed here ot convince me of closure (ie, that the entire along-flight impacts are correctly integrated) and that we have learned anything about the uncertainty of the results. Since we are talking about small differentials in climate per alternate routes, it seems that the uncertainty is critical and must be estimated. While some is systematic and may cancel across routes, others are specific to different altitude processes or different chemistry (O3 vs CH4) and hence do not cancel when comparing routes.

4347-8ff It would seem appropriate to reference the IPCC 1999 chapters on chemical modeling and climate in this intro since they really started the major EU initiatives.

4349- It seems the community is moving to "effective RF" – can this be harmonized? If not, OK< but why not?

4346- Very nice point on the ozone production efficiency

4350-2 This discussion of a sanity check was very disappointing. There are many such checks that should be done and were not even discussed. For one, there are Oslo co-authors who could calculate aviation effects independently (not optimized with the complete system, but at least for some test cases to compare two alternate routes). The EMAC model could have been run to completion itself with different flight routes to compare with the climate RF calculated with the Langrangrian and limited squares analysis. The lack of a 6 month or longer integration to get the CO perturbations correct and thus the O3 and CH4 perturbations is most worrisome. For example the

Lagrangian integrations cannot address this, and the breakdown of the chemistry re-actions and partitioning is not convincing.

4356- A big question here, and one that certainly could affect the results, is what is the size of the EMAC grid (not mentioned) and the size of the Lagrangian plume that the exhaust is diluted into. This resolution appears to be very different from that shown in Fig 3 & 4. The authors do a very good job on the issue of sampling the flight (Fig 7, # of trajectories), but there is also uncertainty /error from the grid resolution of EMAC and the plume model.

4357- Very good sensitivity analysis here, but there is no propagation of uncertainty – i.e., just how different in climate impact do two alternate routes have to be to be significant?

4358- It is not clear what this 2.5 degrees shift means – is it an estimate of error because of the grid size? How does it relate to the EMAC grid?

4358-17ff This detailed chemistry and breakdown of the linearized attribution is still only an approximation. It needs to be tested and demonstrated here that the results are the same as a perturbation run. This is particularly important as the perturbations to O3 and CO (not really a player here) need to be transport throughout the troposphere (for more than 2 months) to derive the overall chemical impact.

4359- The reactions and partitioning for production and loss of O3 does not appear to take into account that O3 affects its own loss frequency by affecting OH and HO2 levels, and that a full linearization is needed. Basically, I doubt that these equations actually give the correct O3 P-L in the limiting case where a perturbation should be derivable form these partitioning coeff's.

4359- As noted earlier, I do not see the transport of O3 and CO coming into play here. And, the CO budget is incomplete.

4376- Yes, Verification is an important item here. The comparison with other published

studies is excellent, but it fails to address the major questions about closure and consistency. Are these partitioned, model-handoff-to-model with totally different scale and scope conserving and integrating the impacts properly? Why not just run EMAC and compare the chemical perturbations – that is what looks most worrisome here. Are the total effects being accounted for. The current paper does not give confidence in this matter.

4404-Fig6 Cannot figure out what is important here – it just repeats the chemistry with different colors? What's up?

4406-Fig8 This set of perturbations is confusing. Is this global? Is the CH4 feedback consistently done within EMAC and is it similar to the results from other published studies, including those from Oslo CTM ? What is conserved here and why/why not? Where are the perturbations occurring – otherwise the odd timings make no sense. When was the pulse? Dec 20? Strange timing.

4411-Fig13- This figure makes little sense as it would seem that the RF from absolute H2O would depend entirely on WHERE that H2O was. It would certainly not be linearly if the injected water were below 500 hPa where it would be swamped by the background. So scaling to total H2O perturbations makes little sense.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4345, 2013.