Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-498-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The role of plume-scale processes in long-term impacts of aircraft emissions" *by* Thibaud M. Fritz et al.

Anonymous Referee #2

Received and published: 2 January 2020

The study introduces a new model called APCEMM, which is designed for simulating chemical processes in aircraft plumes and also considers the effect of contrails on plume chemistry. My impression is that authors do not have a strong background in atmospheric physics. This becomes apparent in quite a few passages of the manuscript. I recommend that the author team strengthens their expertise in atmospheric physics before revising the manuscript and redesigning the APCEMM. The manuscript could become publishable only after major revisions.

General comments

As already stated in the summary above, I doubt that the implemention of the various

atmospheric processes is done correctly. Moreover, sometimes processes or phenomena are included, that are irrelevant and only pretend to increase the level of detail in the model. Comments on the physical soundness of your apprach are listed in the section "Specific comments".

Here, only several general comments on terminology and language are made.

- Even though often written and read, it is wrong: Temperature is not cold or warm. It is low or high and tells us if something is hot or warm. Please check the whole manuscript.
- E.g. formula (2), (4)
  I find it awkward to provide units for each quantity. This somehow pretends that the formulae are only valid in conjunction with exactly those units. This is certainly not the case. I understand that supplying units helps the reader to make a first check of the correctness of the formulae. But the way it is presented, it is misleading.
- Aerosol is a gas with suspended particles. If you refer to the particles only, better use the term aerosol particle. Sometimes you use the term aerosol even for ice crystals (in particular, last paragraph of section 2.2.3). I would make a clear distinction between aerosol particles and ice crystals.
  http://glossary.ametsoc.org/wiki/Aerosol
- Please use the terms deposition, sublimation, condensation and evaporation consistently.
- p.4. l.31: those THAT were emitted
  See https://www.wisegeek.com/what-is-the-difference-between-that-and-which.htm
  Please check the whole manuscript.

- Concerning statements in the abstract like “evaporate ˜9% faster and are 14% optically thinner”
  Given the accuracy of the (still) simplified treatment, I would prefer to leave out such precise numbers (in the abstract). How much are they worth? If you use another definition of optical thickness or define the time of evaporation slightly differently, I am sure you can get anything between 5% and 20%.
  Table 7 and 8
  Given the uncertainties, it is not meaningful to provide numbers like -5.35% with two decimal places. Please round them to a reasonable precision. Similarly the value 1.2581 in Table 4 it "too" precise. Please go through the whole manuscript.

Specific comments

p.2, l.39:

what are “local aerosol clouds”?

p.4, l.5:

The ambient temperature at cruise altitude is not 280K. Climate change is not that fast :-)

p.4, l.14:

Your statement implies that the coherent vortex flow field is just turbulence which is not the case. Please better describe how the vortices break up. Paoli and Shariff (2016) is a good source of information for contrail-specific processes and phenomena.

p.4., l.16:

Schumann, 2012 is a long paper. Which formula do you use? Do assume that the vertical motion is constant over time during the vortex phase?
Note that in a stably stratified atmosphere, large parts of the vertically displaced plume rise back to the original emission altitude after vortex breakup due to buoyancy. As some portion of the ice crystals (or some other tracer) remains at lower levels, the vortex sinking causes a strong and fast vertical plume expansion (compared to time scales of natural processes). It seems that this effect is not considered in your model.

p.7, l.45:

Who is the user in this case and is supposed to choose a value for D_h?

p.8, first paragraph:

The way you include the effect of radiation is not correct. Contrail parts with the highest IWC are usually heated the most. This heating causes an uplift of those contrail parts during which the air cools adiabatically (again proportional to Γ_d). Assuming the atmosphere is stably stratified, the local uplift is sustained as long the ambient temperature is below the temperature of the contrail patch. So for typical stratification values, the initial heating actually translates in a cooling of the contrail! As the heating in the contrail fall streak is usually not that strong, radiation leads to a contrail vertical stretching.

See introductory textbooks on lifting condensation level for the general physics (unlike to warm clouds however, the latent heating effects are not that important in ice clouds and the moist-adiabatic lapse rate Γ_m is roughly the same as Γ_d).

p.8, l.11:

Could you describe in a few words what KPP is.

p.8:

The inclusion of ice aggregation seems very sophisticated (iterative determination of coalescene efficiency) compared to the treatment of other processes in the model. But more aggravating is the fact that the cited Beard & Ochs paper deals with precipitation drops and not ice crystals. Please refer to literature refering to aggregation, not coalescene of liquid cloud droplets. Section 4 of Sölch and Kärcher (2010) could be a good starting point to dive into the physics behind the aggregation efficiency.

p.9., l.21:

What specifications of the chosen aircraft and engine type appear in the model, and which ones matter in the end? Are your results only valid for this specific aircraft/engine combination or can your findings be generalised?

p.9:

Why is it important to evaluate the error ϵ_X after 24 hours, hence at the same time of the day as the initialisation was done? Wouldn't it be better to evaluate the error at a time where the spatial dimensions of the APCEMM-modelled plume and the BOX-modelled plume are similar? Refering to Figure 2, you state in the text that 1.2 kg or 0.2 kg ozone is produced, giving a fatcor 6 difference between the two modelling approaches. A variation of the evaluation time would dramatically change this factor. Using an earlier point in time (e.g. 2 hours earlier at time 6:00) the factor would be much higher. Could you make clearer the strategy behind your evaluation effort.

Remarks on section 3.5.1:

- Several choices of the background conditions are not reasonable at all. Section 2.2 of Kärcher et al. (2009) may help to choose more realistic cases.
- A 10 cm/s cooling over 24 hours translates into a lifting by 8 km and an adiabatic cooling by 80K. This is not realistic. Compared to this, the 0.1 K diurnal temperature variation can be safely neglected.

- Persistent contrail formation is likely to occur in a RH_i-range of $100\% - -140\%$ (above the upper limit, natural cirrus formation could not be neglected)
- Does the depth of supersatured layer remain constant over time? Given the prescribed uplift, the initially subsaturated layer above/below the supersatured layer would eventually become supersatured as well and the supersaturated layer would grow in size. If you used a time-constant 200m thick layer and included the radiation effect correctly, the contrail would move out of supersaturated layer into the drier air above. This leads to entrainment of dry air into the contrail which would then start to vanish.
- Given the quite thin supersaturated layer, the simulated contrail lifetimes of > 10h appear to be too large (in particular for $v_{UP} = 0$).
- You first make a link between in-situ loss and aggregation and few lines later you say in-situ loss is due to Ostwald ripening.
- line 21: Yes, it is usually warmer further down, but this is irrelevant here. Or do really want to say the ice crystal melt and become water droplets? What matters is that it is dry and the ice crystals sublimate and are lost eventually.
- If your contrail model produces reasonable results, could be checked by a comparison with the higher resolution model used in Unterstrasser & Gierens, 2010a,b. This comparion should be feasible to achieve with small extra effort as you anyway use mainly their definitions of contrail properties. It would only require to specify the same background conditions. This could show if your modeled contrail lifetimes and response to variations of RH_i or EI_{soot} are reasonable.

Table 8:

Wouldn't it be interesting and more insightful to compare all four simulations to the non-contrail simulation?

Technical corrections

p.3, l.14: the effects OF these

p.7, l.21/22: No complete sentence.

p.7, l.42: repetition of "a measure of local .."

p.11, l.24: please reformulate the sentence.

References

B. Kärcher, U. Burkhardt, S. Unterstrasser, and P. Minnis. Factors controlling contrail cirrus optical depth. *Atmos. Chem. Phys.*, 9(16):6229–6254, 2009. ISSN 1680-7316. URL http://www.atmos-chem-phys.net/9/6229/2009/.

R. Paoli and K. Shariff. Contrail modeling and simulation. *Annual Reviews in Fluid Mechanics*, 48(1):393–427, 2016. doi: 10.1146/annurev-fluid-010814-013619. URL http://dx.doi.org/10.1146/annurev-fluid-010814-0136190.

I. Sölch and B. Kärcher. A large-eddy model for cirrus clouds with explicit aerosol and ice microphysics and Lagrangian ice particle tracking. *Q. J. R. Meteorol. Soc.*, 136:2074–2093, 2010.

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