

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

**Anonymous Referee #3**

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This paper describes a detailed chemical and microphysical model that calculates the composition of aircraft plumes and its interaction with the atmosphere. This is certainly one of the most comprehensive studies that accounts for many chemical reactions and interactions between chemistry and microphysics of particles. The results obtained confirm previous studies, like for example the overestimation of ozone production due to NO<sub>x</sub> emissions when instantaneous dilution is adopted in CTMs and the role of heterogeneous processes that convert part of the emitted NO<sub>x</sub> into HNO<sub>3</sub>. All those results are interesting but in my opinion there are several issues that should be further discussed. Follows my major points.

1/ In the introduction it is claimed that almost all the CTM use the instantaneous dilution (ID) approximation to account for aircraft emissions. The authors seem to ignore the

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many attempts of modelers to introduce plume processing in their large-scale model. The authors should refer to the review paper by Paoli et al. (2011) that gives a comprehensive review of the different approaches that have been followed to account for plume effects: Effective Emission Indexes, Emission Conversion Factors or Emission Reaction Rates. In the same paper are listed the CTM that use those parameterizations and the limitations of each approach.

2/ In contrast to the detailed chemistry and microphysics introduced in their model the authors have chosen a very simplified representation of the contrail and plume dynamics based on a simple diffusion model. This is a very crude approach that for instance ignores the details of contrail dynamics with the role of the crow instability, the formation of secondary vortices that maintain a significant fraction of the emissions at flight level with often persisting ice particles, and the complex nature of atmospheric turbulence and its interaction with radiation (e.g. Paoli et al., 2017). For instance wind shear and diffusion are considered as separate processes, although depending on the scale considered wind shear and diffusions are both the results of turbulence in a stratified atmosphere. Thus, it is very difficult to evaluate how the approximations made can influence the results of the model. Is it a balanced approach to introduce a detailed chemical and microphysics schemes with such a simplified dynamical scheme?

3/ Little is said on the validation of the model. Do we have measurements to confront to the model outputs? Can we constraint ozone formation rates and the conversion fraction of the emitted NO<sub>x</sub> to nitrogen reservoirs?

4/ In the conclusion it is concluded that plume effects are important and should be included in CTM. This is not new (see all the articles referenced by Paoli et al. 2011) and leaves open the difficulty to do that in a consistent manner with the chemistry and microphysics in place in those CTMs or GCMs. It is often because this consistency is difficult to preserve that the CTM and MCG modelers keep the ID approach despite its limitation.

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References :

Paoli, R., O. Thouron, D. Cariolle, M. Garcia and J. Escobar. Three-dimensional large-eddy simulations of the early phase of contrail-to-cirrus transition: effects of atmospheric turbulence and radiative transfert. *Meteorologische Zeitschrift*, Vol. 26, 6, 597-620, 2017. Paoli, R., D. Cariolle, R. Sausen. Review of effective emissions modeling and computation. *Geosci. Mod. Dev.*, 4, 643-667, 2011.

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