

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2022-169

Xavier Vancassel (Referee)

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Referee comment on "Aviation contrail climate effects in the North Atlantic from 2016 to 2021" by Roger Teoh et al., Atmos. Chem. Phys. Discuss.,  
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### General comment

This paper aims at addressing the radiative impact of contrails in the North Atlantic between 2016 and 2021. This is an important issue since contrails represent a non-CO<sub>2</sub> effect of potentially high magnitude. In order to select appropriate mitigation strategies, comprehensive knowledge of the most significant processes is needed. To this respect this paper provides an important contribution and I strongly recommend its publication in ACP.

The methodology is based on the use of several blocks:

- Air traffic information
- Meteorology data over a 6 years period
- Aircraft type and performance, mass, engine properties, emissions
- A contrail-cirrus prediction tool.

Each set of data required the use of various tools and methodologies, for instance to estimate aircraft engine emissions for cruise conditions from ICAO LTO dataset or to apply corrections to the ERA5 humidity fields. It looks to me that this represents a tremendous amount of work and the application of a strategy/methodology following already previously published work.

The paper is clear and very well written. It is an impressive work.

## Specific Comment

Line 245-250: The mean ice crystal radius is smaller in wintertime relative to the summer. Less condensable matter is available (RH<sub>i</sub> is lower in figure S14e) probably explains this point. Mentioning that contrails are formed at lower temperature ("lower temperature with less condensable water") is finally confusing since the effect of temperature is already included in the RH<sub>i</sub> which drives particle water uptake.

I probably missed a definition of the mean radius  $r_{ice}$ . Is it averaged along the contrail forming flight distance?

Supporting information line 260: there are quite a number of papers deriving saturation water vapour pressures. Has the choice of Sonntag (1994) been made for consistency with some other data, for instance in ERA5. Surely the choice of the parameterization used can modify RH<sub>i</sub> significantly and the predicted ISSR.

## Technical corrections

Line 51: Missing "." after "...Heymsfield, 2017)"

Line 78: There are actually two ICAO 2021 references in this paper and they should be called differently. One of them is misplaced in the References section (line 504).

Line 122: delete "and" after "ensembles)"

Line 226: Correction "than it would have been" instead of "than it would been"

Line 425 and 427 correct Schumann & Graf

Supporting Information Line 441 Table S5 caption : spelling « anonymised »