

Review of the manuscript “Understanding the Role of Contrails and Contrail Cirrus in Climate Change: A Global 2 Perspective” by Dharmendra Kumar Singh, Swarnali Sanyal, and Donald J. Wuebbles.

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I cannot recommend this paper for publication.

When I started to review this paper, I thought it is interesting to have. But during reading, I found very little constructive results or insights. Instead I more and more found many unqualified statements which brought me to the conclusion that this paper should better not be published.

I am sorry that I was less clear when commenting on the paper the first time during the discussion phase. I saw chances for an overview on the wide set of achievements of recent years and I saw chances for improvements. But now I see that the authors are driven by the aim to emphasize uncertainties above knowledge and progress.

The authors criticize the whole community; e.g.:

“Many global climate chemistry models used to study the physics and chemistry affecting the Earth’s climate system typically do not incorporate the impacts. Models that do account for contrails estimate contrail cirrus coverage based on simplified treatments of contrail aging and spreading mechanisms in ice-supersaturated regions (ISSRs) (Burkhardt et al., 2010; Burkhardt 86 and Kärcher, 2011; Bock and Burkhardt, 2016; Bier and Burkhardt, 2022; Chen and Gettelman 2013; Bickel et al., 2020; Schumann et al., 2015).

What scientific basis do the authors have. Do they ever have done work on contrail modelling? How can they be so negative without constructive suggestions? This is not what we need.

The paper is not scientifically open-minded and neutral but clearly biased to highlight all kinds of “uncertainties”. New findings are hardly acknowledged but cited with the comment that something might be missing.

The term “uncertainty” is used more than 60 times in this paper but nowhere clearly defined. It is a subjective measure without a quantitative meaning. It is used for statistical scatter as well as for incomplete understanding.

Here one example (lines 1508 ff): “Aircraft Plume Mixing: Uncertainty persists in referring to how ambient air mixes with the aircraft plume during contrail formation, comprising uncertainties associated with the rate and extent of mixing.”

How can one assess any scientific progress that may be made by further research in terms of the degree of reduction of uncertainty, when the term uncertainty is not a well-defined measurable quantity?

Nowhere the paper tries to quantify uncertainty. That would offer a clear chance for scientific assessment (e.g. statistical measures like correlation coefficients or mean biases or measures of significance).

Of course, the understanding of the physics behind a problem is often also incompletely understood, but this has to be explained scientifically at a high level of competence in each case.

For example, lines 538 ff discuss the importance of the ice crystal number (Ni) where Ni quantifies the concentration of ice crystals in the ambience (I would say, in ambient cirrus clouds). They have no quantitative argument but claim: “Key aspects such as contrail persistence, the extent of spreading, and the optical characteristics of contrails are closely tied to the ice crystal number (Ni). In essence, Ni serves as a fundamental factor in understanding and characterizing contrail behavior and its impact on the atmosphere. “ -

This comes without reference! My own research results tell me that contrails often form inside thin cirrus clouds and the interaction between the background cirrus and the contrails is weak as long as the ice particle concentration in the contrails is significantly higher than the ice particle concentration in the ambient cirrus - and that is often the case.

From the list of references and the style of arguments, I do not see that these authors have the competence to state such critics.

Another example, from page 42, line 1516: “Aerosols and Aircraft Emissions: Uncertainties persist in understanding the role of aerosols and particles from aircraft emissions in contrail formation and properties.

I must tell you: Uncertainties will exist also in 100 years from now, in particular (but not only) in respect to aerosol. If you perform more research, it is very likely that you detect new effects raising new questions and hence enhancing uncertainty. So, it cannot be the goal, to simply reduce uncertainties. This is a “mission impossible”.

Later on that page. The authors cite work of Schumann and Graf (2013, JGR) reporting on a fingerprint. The results are based on 8 years of satellite measurements over the North and South Atlantic and the result was used by IPCC (Boucher et al. (2013, IPCC, reference missing in this manuscript; together with those from Burkhardt and Kärcher, 2011) to assess the RF of contrails. Then instead of pointing out the innovative information content of this study, at least at that time, the authors conclude: “These findings resonate with our discussion on radiative transfer scheme uncertainties, as variations in how models handle radiative processes can significantly influence the estimated impact of contrails on Earth's energy balance.”

Instead the authors (rightfully) acknowledge progress in artificial intelligence (with reference to workshop reports, e.g. , line 1441: McCloskey et al. (2021) or line 1468: Siddiqui (2020) published in an undergraduate journal). Why don't they acknowledge remote sensing studies published in high quality journals similarly?

This paper does not acknowledge that it is common for environmental science that uncertainties exist but these acknowledged uncertainties do not exclude actions to mitigate environmental problems: (E.g., reduction of NOx and soot emission from engines or reduction of CO2 emissions by alternative fuels or improved aircraft/engines and operations).

Instead of a list of uncertainties, we need scientific advice of how to start practical activities, accompanied by careful scientific validation activities, to reduce the overall climate impact of aviation as quickly as possible. In view of the observed actual climate change and rather obvious effects from

contrails and CO₂, that is a very urgent issues and should not wait for decades of further research. Avoiding warming contrails can be a very cheap measure of mitigation (see.e.g., Frias et al., 2024).

Instead, this paper lists a set of arguments to retard mitigation.

The paper is not helpful for science. It is politically biased and I cannot recommend this paper for a scientific journal.

The comments above are incomplete. There are so many points to criticize or to suggest improvements that I cannot list all of them. On each page I read, I have several remarks. Instead, I commented more generally.

Minor points:

Fig 1 is often used to compare contrail and other non-CO₂ effects to CO₂ effects. However, this paper and most interpreters ignore the fact that the CO₂ effects shown in this figure result from the emission since the beginning of aviation, at least since 1940, while the contrail effect applies within hours, so practically momentarily.

Hence, Fig. 3 cannot be used when discussing the potential of various mitigation actions which require an assessment of future climate impacts, not of past.

Line 155: "soot emissions influence the meteorological factors..." that sounds wrong.

Chapter 2.1 is very badly organized. Many aspects are repeated at various places,. E.g., text in lines 298 to 310 (Contrails are created ..."), at least to a large extent, repeat statements made before.

Lines 13-15, Abstract.

The third sentence „While contrails ...life cycle“ is redundant, and should be deleted.

The presently fourth sentence uses the word “uncertainties” without any specification. It would be more reasonable to replace it by: “Despite extensive research, the relative importance of the climate effects of contrails compared to other aviation effects remain under debate.”