Anonymous Review

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Understanding the Role of Contrails and Contrail Cirrus in Climate Change: A Global Perspective

by Dharmendra Kumar Singh, Swarnali Sanyal, Donald J. Wuebbles

The topic of the paper is of high actual interest and well suited for ACP.

As stated in lines 100 ff, the paper aims to provide a comprehensive review of our understating of contrails… and to address the uncertainties….

Such a comprehensive review does not exist in the literature. It seems that the not all authors have extensive experiences in contrail research by themselves so many of their comments may raise critical discussion. The paper reads like a literature study in preparation of more specific own research by the authors on this topic.

But different from reviews by acknowledged experts, who like their own work most, this paper is rather unbiased in the selection of materials and assessments, and this is refreshing.

The coverage is quite good but can be improved. Here I have several additions and offer comments on detail, see below. The paper is (of course) mostly a collection of existing knowledge. The conclusions are not really surprisingly new. I found the comments on Machine Learning aspects particularly interesting (I think I should try to learn more about them). Section 7, “Research needs and gaps “ is worthwhile to have though not objective in all parts and partly hard to validate because of incomplete explanations or citations. I think it is worthwhile to have estimated numbers on the magnitudes of uncertainty but presently these number are not well justified.

The review misses many important publications. That should be improved.

The paper is partly too long to keep me interested in all details, collects lot of materials. Still the paper is worth to read but it needs amendments before it can be published.

Such a long paper is hard to review, and I can give only a few specific comments. I would formulate many parts differently.

Abstract.

Line 12: The sentence “contrail cirrus enhances the impact of natural clouds on the climate” is misleading, because I read it first as if contrails would have no effect without

natural cirrus. But that is not the case. Better: Contrail cirrus is similar to natural cirrus in having an impact on climate.

Then I would start a new sentence on the uncertainties – without “although” or “however” or “but”. Simply say that this paper discusses existing uncertainties

The statement “there are still unresolved questions” is trivial. There will be always unresolved questions. In fact, with further research more and new questions may likely arise. So, you should condition this sentence a bit more, like “presently the state of the art is insufficient to assess the magnitude of the climate impact of contrails, e.g. on surface temperature, and its error bounds with high confidence” – or similar.

Line 16: add “mixing” as one of the issues.

Line 56: this was known basically since long (see IPCC, 1999). Other reviews of relevance are, e.g. (Schumann, 1994; Sassen, 1997; Brasseur et al., 1998; Fahey and Schumann, 1999; Prather and Sausen, 1999; Sausen et al., 2005; Brasseur et al., 2016)

Line 68: what do you mean with atmospheric warming. It is correct for RF. But the ERF relates RF to the global mean surface temperature change.

In fact, perhaps you can discuss how reasonable it is to assess the impact of contrails in terms of global metrics. Contrails are local events, are short lived and have certainly a very small global mean surface temperature climate impact. Contrails warm mainly the upper troposphere, not so much the surface. Most contrails occur over continents with little chance or long-lives effects by heating the oceans. The regional effects are likely far larger than global effects and should be easier to be detected and quantified. And regional effects are of importance for the densely populated latitudes in the Northern hemisphere where most air traffic occurs.

Line 78: sentence(s) incomplete and unclear.

Line 81 is biased to one team and one model type only. Other should be included.

Line 85: there is far more literature on this aspect.

Fig 1 caption. “Current best estimate..” The estimate is already no longer the last one. Several studies have added new estimates. I would suggest: “Climate forcing estimate as published in 2021” or similar.

Fig 2. misses to mention the importance of mixing of the jet exhaust gases with ambient air. Mixing controls early or late contrail formation and contrail lifetime (Lewellen and Lewellen, 1996; Gerz and Ehret, 1997; Sussmann and Gierens, 1999; Lewellen, 2014; Paoli and Shariff, 2016; Schumann and Heymsfield, 2017).

Line 152. These 2 references do not cover this topic sufficiently. There are many more. I see that you cite them later. But this line is irritating. See line 178. See e.g. Sassen (1997).

Line 197. Further new reference: (Märkl et al., 2024) .

Line 246, Contrail formation was observed far earlier. See (aufm Kampe, 1942; 1943; Weickmann, 1945; Brewer, 1946; Schumann and Wendling, 1990; Schumann, 1994; Busen and Schumann, 1995).

Line 256. The visible constraint were introduced earlier: Appleman (1953).

Line268 and Fig 4: a similar figure with a contrail spiral was given earlier in Fig. 11.2 (Schumann, 2002).

Impressive contrail observations were also presented by Laken et al. (2012) and (Mannstein and Schumann, 2005).

Line 277: it is now pretty clear that the soot acts as condensation nucleus so that liquid droplets form which then freeze. The resultant ice particles likely do include the soot particles on which the water condensed. This does not exclude that some ice particle sublimate or that some soot remains dry throughout the contrail formation process, so that also dry soot is found inside contrails. See upcoming paper by Dischl et al. (Egusphere, to be published soon).

Lines 279 to 288 has been taken from Schumann and Heymsfield (2017), but the reference is missing.

Line 446: How do we know the current ranges of in-flight soot emission. Here you should cite papers by the team around Marc Stettler and Roger Teoh and colleagues (Teoh et al., 2019; Teoh et al., 2020a; Teoh et al., 2020b; Teoh et al., 2022a; Teoh et al., 2022b; Teoh et al., 2024a; Teoh et al., 2024b).

Lines 462 to 483: For the discussion of the impact of the initial contrail ice particle number concentration (and hence the soot emissions) on contrails the cross-section integrated optical extinction and hence on climate forcing you should cite details of the

study and explanation around Eq. (5) in Lewellen (2014). See also Section [17] in (Schumann et al., 2013).

Page 9: The summaries of Urbanek, Kärcher and Wolf read like abstracts from these papers. The review does not really combine these result critically with related results from other studies. This is a serious deficiency of this review and makes me uncertain whether this is an acceptable review.

Lines 516 ff: Sedimentation. This section misses citations of related literature. See, e.g., Spichtinger and Gierens (2009)

Line 563 : 290 m/s, That is nearly speed of sound. I doubt that this value is correct (230 m/s would sound more reasonable).

Lines 574 to 588: difficult to read. I have not understood what you want to say.

Line 606, replace 2016 by 2017.

Lines 617: the LES is of course far more expensive than a plume model and hard to apply globally. We need improved plume models, e.g., based on LES. By the way: Much work in this respect has been done by Unterstrasser (2016).

Line 732. The comparisons have been done in several follow-up papers of that cited here.

Line 780: You refer the age of 450 s without comments. To me 450 s is far too long.

Line 83: neither 10 nor 7.5 um is correct. That value varies by large factors.

Line 528 see earlier work, e.g., Sassen (1997).

Line 876: This was first discussed by Meerkötter et al. (1999).

Line 1122, Overlap effects. See (Schumann et al., 2021; Teoh et al., 2024a)

Line 1130. Ettenreich observed in 1915, but published in 1919.

Line 1153: Which reference do you refer to with Schumann, 2016?

Line 1158: See the GAIA data set (Teoh et al., 2024b).

Line 1163. See also the development of open access performance codes: (Poll, 2018; Poll and Schumann, 2022; 2024).

Line 1252: Limited literature exists". See, e.g., measurements of ice particles by the Geophysica aircraft at about 20 km height above tropical convection by measuring its own contrail (Schumann et al., 2017).

The paper by Myhre (2009) is certainly outdated There are several recent studies citing this paper.

Line 1299 How do you come to the value 55 %? It seems too large to me.

Linem1346: Contrail effects on climate will never be "fully" understood.

But also many other things are incompletely understood and nevertheless practically addressed. (See, e.g., smog in cities, observed in the 1950's and soon thereafter regulated by US federal institutions (was it ERL, I forgot the name) and later by ICAO.

Line 1359 Again: ERF is an insufficient climate metric because it does not account for regional (northern mid-latitudes) phenomena and other climate changes besides mean surface temperature.

Line 1503: As said before "Uncertainties persist"´. I must tell you: uncertainties will be there also in 100 years from now. The existence of uncertainties is not a sufficient argument to start mitigation actions soon. We only have to make sure that the actions reduce and not increase the climate effect. (It is like building a bridge over a river. The only thing which counts is that the likelihood that the bridge fails is sufficiently small to be acceptable. That problem was solved at least 2000 years before today, in spite of very large uncertainties. I suggest to carefully rethink about talking about uncertainties.

Chapter 7 "Uncertainties and Research Gaps" is certainly an important part of the paper.

Here many questions arise.

You provide number of the uncertainties in percentages. How are these number derived? That is unclear in most case. Please explain. I assume you will note: there are not only uncertainties but also uncertainties of how large the uncertainties are.

The quotes percentage values add (linearly) up to a total of more than 100 %. Please discuss.

Line 1284: The reference to Myhre et al. (2009) in assessing radiative transfer schemes is outdated. There are several new discussions. The key parameter is the shortwave to longwave ratio in contrail radiative forcing. E.g. Newinger and Burkhardt (2012) used an outdated Radiation scheme which strongly underestimated the SW/LW ratio. See discussion in Schumann and Graf (2013) and the important poster publication of Ponater et al. (2013).

Line 1287: add Forster et al. (2012).

Line 1311: Why is the effect of lifetime of lesser importance? I would expect it is very important (Lewellen, 2014)

Line 1327: add: well assimilated observation are important to setup good initial conditions for accurate weather predictions (Bauer et al., 2015).

Line 1359: ERF is not a measure for climate change in general. It is a measure of the RF impact on global mean surface temperature change only.

The bi-model size distribution of soot??? I am not aware of this. Please provide a reference. See Fig. 4 in (Schumann et al., 2002).

Line 1402: The word “fully” excludes everything, but see (Teoh et al., 2020a; Teoh et al., 2020b; Molloy et al., 2022; Teoh et al., 2022a; Martin Frias et al., 2024; Teoh et al., 2024a).

Line 1413: replace “more” by “larger”

Please note the following comment on Sausen and similar studies. It makes no sense to avoid formation of all contrails. That would imply that you also avoid cooling contrails and it overburdens air traffic management. It is sufficient and far more effective to avoid the warming contrails and to allow for (even more) cooling contrails. I gave several references for this above, e.g., (Molloy et al., 2022; Martin Frias et al., 2024).

Line 1498: Please delete the word “valuable”. Such an applause should come from the readers not from the authors.

Line 1505. “Uncertainties persist” that has been said often enough and does not get more correct by repeating.

Line 1519: replace Berndt by Bernd.

Lien1524 replace “the” by “for ”

In looking to the list of references:

Please consider the important work of (Bickel et al., 2020; Ponater et al., 2021; Bickel, 2023)

Also (Duda et al., 2023).

Very important: (Iwabuchi et al., 2012)

and on ice supersaturation: (Ovarlez et al., 2000; Jensen et al., 2001; Ovarlez et al., 2002)

Line 1732 and elsewhere: Please list all co-authors.

Line 1916: What does the single “S” mean?

Line 1949: please set in capitals (here and elsewhere): MSG and SEVIRI

Line 1854 The method's name is RRUMS not drums.

Overall: A carefully revised version should become interesting, worthy to read, and acceptable.

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