

Interactive comment on “Is there a trend in cirrus cloud cover due to aircraft traffic?” by F. Stordal et al.

Anonymous Referee #2

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general comments:

The manuscript deals with the question if air traffic leads to an increase in cirrus cloudiness, which is presently an important topic regarding the overall impact of aviation on climate. Up to now, only few estimates about the climate impact of contrail-cirrus exist.

The authors analyse 16 years of ISCCP data and correlate trends in cirrus coverage with air traffic density data. Their treatment of the observational data seems to be cautious and thoroughly: They discuss two different ISCCP data sets and their problems regarding the detection of optically thin cirrus in detail. They also discuss discontinuities in the data due to the contribution of different satellites and consequently focus on regions that are covered by only one single satellite.

In addition, in order to retrieve another cirrus cloud data base, they use their own anal-

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ysis method for deriving cirrus coverage for the METEOSAT region. For air traffic, they use a new air traffic inventory developed during the TRADEOFF project, which contains flown distances for the year 2000. I regard their data base as sound and state-of-the-art. The presentation is mostly clear and well-structured, but there are also several text passages that deserve a more precise reformulation or even reorganisation. (details see below). The manuscript contains enough original material to be worth for publication in ACP.

However, there are many uncertainties in the results, which in my opinion need to be addressed more clearly. My main concern is that their (in their own words) rather crude estimate about the global radiative forcing of aircraft-induced cirrus clouds could be interpreted too strongly. Hence, more sophisticated uncertainty estimates should be performed (details see below).

Especially statements in the abstract should be made very cautiously. My main reservation in this respect is the mentioning of a "best estimate" for contrail cirrus radiative forcing. This implies, as IPCC (1999) did refrain from giving a best estimate due to the lack of required criteria for doing so, that the authors assess the state of knowledge on contrail cirrus has been advanced by their study to such an extent that a "best estimate" is now possible. This is in conflict to the main text, which is honest enough to assess the derivation of the global RF as "crude". The minimum requirement of regarding their estimate a "best estimate" would be the derivation of upper and lower bounds, by using the alternative results for aircraft related cirrus increase (Boucher, 1999; Zerefos et al., 2003; Minnis et al., 2004) in their calculation. The same holds for the spread of results concerning the RF of linear contrails (Minnis et al., 1999; Myhre and Stordal, 2001; Marquart et al., 2003). My recommendation is to remove the term "best estimate" from the abstract, as it is not covered (not even mentioned) by the main text.

The comparison with other existing studies (Zerefos et al., 2004; Minnis et al., 2004) is too superficial. Since these papers address exactly the same question it appears to me very desirable to make a quite detailed comparison of methods and results in

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the discussion section. Having in mind the large uncertainties, the discussion section should also provide information about future research requirements.

Some figures need to be revised (see below). Figures 9, 10, 11, are very superficially discussed. If they contain cases that are not worth to be mentioned they should be left out.

Main specific comments:

(1) Introduction: in my opinion it would be worthwhile to explain the methodical differences to other papers dealing with aviation induced cirrus cloudiness (either here or in the Discussion section).

(2) ISCCP data: When ISCCP data are discussed, it is not made clear what the term "cirrus" means in relation to "high clouds". I understand that "high clouds" mean all clouds at the given altitude, but by which parameter are "cirrus" clouds distinguished ?

(3) p.6479, l.28/29: I do not see from the comparison of Figs. 1, 2, 3 that the NILU/METEOSAT results (Fig.3) are more similar to Fig.2 than Fig.1. It would be very helpful if these Figures had the same type of plot and color scale.

(4) p.6483, section 3.1: Region 3 (and its zero-correlation) should be discussed here since, similar to regions 2, 7 and 8, it is another region with high air traffic density.

(5) p.6484, l.15-20: The discussion of the temporal trends derived from regression should be extended: Why does the regression method indicate much weaker correlation than the "reference" method? What conclusions can be drawn from this finding?

(6) p.6485: Maybe, a more suitable title for section 4 would be "Global aircraft impact on cirrus cover"?

(7) p.6485, l.23-25: As far as I understand the global change in cirrus clouds between 1984-2000 is calculated by "slope x global air traffic density in 2000 (layer 17-19) x 2 x 16 years". I do not understand the factor 2 in this context. I do understand that

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the air traffic in 2000 is 2 times higher than 1984, but this does not explain the factor 2 in the above formula to me. Shouldn't instead of "global air traffic density in 2000 (layer 17-19) x 2" the mean air traffic density between 1984 and 2000 be used? In any case, the calculation of the radiative forcing estimate would profit from presentation of an equation, or more than one.

(8) p.6485/6, section 4: An uncertainty estimate would be very appreciable in order to highlight the large present uncertainty: (a) Try to deduce a plausible "low" and "high" estimate for possible slopes (e.g., plausible slopes would be: slope in region with highest correlation vs. mean slope from 3 regions with the highest correlations vs. mean slope from regions with highest air traffic density (regions 2,3,7,8) (b) For the radiative forcing estimate, a lower and upper estimate should be provided in addition to the 0.12 W/m² from Myhre and Stordal (2001). E.g.: low estimate from Marquart et al. (2003), where 0.06% contrail cover gives 3.5 mW/m² (i.e. 0.06 W/m² per 1% cover); high estimate from IPCC (1999) (i.e., Minnis et al., 1999), where 0.09% contrail cover gives 17 mW/m² (i.e. 0.19 W/m² per 1% cover).

Minor specific comments:

(9) Introduction: Minnis et al. (2004) should be mentioned here.

(10) p.6475, l.18: delete "from"; It should be mentioned that the estimate of IPCC (1999) for contrail cirrus is very uncertain (not even a best estimate was provided).

(11) p.6475, l.19-20: delete this sentence or replace by something like "Locally, the radiative forcing may be much higher".

(12) p.6478, l.2: Is there a reference for the IPCC D2 data product?

(13) p.6478, l.16ff: ISCCP VIS/IR data set: I understand that this dataset is capable of better capturing thin cirrus clouds. Does the data set at the same time exclude or include other types of high clouds (convective, stratus)?

(14) p.6478, l.24: Please explain the purpose of calibrating the METEOSAT IR images

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to co-located AVHRR infrared channels.

(15) p.6479, l.29: Do you mean "ISCCP IR" instead of "ISCCP VIS/IR" here?

(16) p.6480, l.9/10: "Similar trends": similar to what?

(17) p.6480, 1st paragraph: It should be mentioned that the positive trend over Europe only shows up in the ISCCP VIS/IR dataset, while the trend over Europe is negative in ISCCP IR.

(18) p.6481, l.10-12: "Many main features are in common with the data from ISCCP IR(Figs. 4 and 5)....high values over Africa and Europe". Two statements are inconsistent here: 1. Fig.4 shows ISCCP VIS/IR and not ISCCP IR. 2. High values over Europe appear only in the ISCCP VIS/IR data set. Please clarify what you mean.

(19) p.6481, l.20: add "the years" before "1992 and 2000".

(20) p.6482, l.8/9: Is the term "Revenue Passenger Kilometers" common to everyone, without explanation or reference ?

(21) p.6484, l.26/27: "Our analysis shows..." I do not understand this sentence.

(22) p.6485, l.1: "less" than what?

(23) p.6485, l.2/3: "The variations in the slopes between the various cases to a large extent follow the variations in the correlation coefficients." Please give an interpretation of this finding.

(24) p.6486, section 4, last paragraph: I suggest moving this paragraph to the discussion section.

(25) p.6486, l.15: remove "Quite important"

(26) p.6487, l.6: replace "is 0.1 W/m^2 " by "was estimated to be 0.1 W/m^2 ".

(27) p.6487, l.6-13: These sentences should be moved to the uncertainty discussion I suggested above (7) for section 4.

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(28) p.6487, l.13-15: This sentence is quite difficult to understand. Please re-formulate.

(29) p.6487, l.20-22: This sentence more or less resembles the first sentence in the Discussion section. Please avoid this.

Figures:

(29) Figs. 1, 2, 3 should be the same type of plot and have the same color bar. This would make comparison much easier.

(30) Fig. 2: Map, axis labels and color bar are missing.

(31) Fig. 3: Map and labels are too faint.

(32) Fig. 6: Label size is too small.

(33) Fig. 7: The same plot type and the same map section as in Figs. 4 and 5 should be used to facilitate the comparison.

(34) Figs. 9-11: In my opinion, a zero-line would be helpful for a quick understanding of the figures.

(35) Fig. 11: Legend: What is NILU/METEOSAT To+10K?

Technical corrections:

(36) p.6474, l.4: replace "could" by "cloud"

(37) p.6475, l.21: "." is missing

(38) p.6447, l.3: replace "that" by "than"

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6473, 2004.

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