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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 4 November 2004

This manuscript presents trends in cirrus cloudiness (from ISCCP and retrieved from METEOSAT data) in relation to air traffic. The evidences which are presented that air traffic has an impact on cirrus cover are rather weak and certainly not statistically significant in any sense. However since the manuscript is worded carefully (except maybe the abstract), I do not think it is a reason not to publish the study in Atmospheric Chemistry and Physics. I have however a number of major comments which need to be addressed and more work is needed before publication can be envisaged in ACP. Specifically, the trends need to be better computed, especially for the METEOSAT data, and the NAFC region needs to be looked at.

Major comments:

The abstract should be worded more carefully, more along the lines of the discussion

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section.

The cloud products which are used are not very suitable for detecting cirrus. Although this is stated in the manuscript, still it is a major shortcoming to this study. Even the ISCCP VIS/IR products will not detect thin cirrus in the case of multilayered clouds. The METEOSAT cirrus retrieval could have been very useful if it was performed at a high temporal (and spatial) resolution and correlated to air traffic at the same high temporal resolution. However with a temporal sampling of one image per day and 1 year every 5 years, I am not sure it brings much information as compared to ISCCP cloud products.

Throughout the manuscript the flight density is given in unit of km flown / km^3 volume. However this quantity should be given by some unit of time. If this is right, the correct unit should be used throughout the manuscript and captions. Moreover it is not clear for which base year is Figure 8.

I do not like so much the authors' first way to estimate trends. The fact that two 8-year periods have different average values does not mean that there is a trend. Regarding the second way, it is mentioned that the trend is calculated from monthly means. Wouldn't it make more sense to compute it from annual averages in order to get rid of the seasonal variability? (note that it is annual averages which are plotted in figure 6!).

I cannot agree with the trend estimation for the METEOSAT high clouds. It is dangerous to compute a trend from only 4 values, and even more dangerous the way it is done here. Let's take the following example of values of 50, 49, 52, and 50% in 1985, 1990, 1995, and 2000, respectively. The authors will estimate a trend of 0.1%/yr where it may simply be an oscillation or natural variability. The trends need to computed in a more robust way, preferably by using more years, and the statistical significance of the trends should be assessed.

It would be better to correlate the regional trends in cirrus cloud with the regional trends in air traffic rather than with the absolute value of air traffic. However I understand that the history of the air traffic data may be missing.

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Interactive Comment

I am surprised to see that the authors leave the North Atlantic Flight Corridor out of their study (see Figure 8) although it is a region of highest traffic over the ocean which has been looked at by previous investigators (Bakan et al., Annales Geophysicae, 12, 962-968, 1994; Boucher, 1999).

I do not understand the factor 2 in the back-of-the-enveloppe calculation done in section 4. I understand that the flight density of 1.2 km km^{-3} is for the year 2000, but it may not be the case.

Small and editorial comments:

p 6475, l 4: Boucher (1999) found a clear signal on cirrus cloud occurrence, but a less clear signal on cirrus cloud cover. This should be mentioned somehow.

p 6475, l 29: "three day periods"

p 6476, l 19: "Geostationary"

p 6478, l 10-11: It is rather that there is no data from geostationary satellites over the Indian Ocean.

p 6479, l 25-26: the authors are probably using data from different (successive) METEOSAT satellites, it is just that they are using only one at a time. So the sentence should be reworded. The authors justify the use of METEOSAT from the need to look at the NAFC with only one satellite, however they do not look at the NAFC since it does not correspond to any of the 11 regions of figure 8. As mentioned above, I would like to encourage the authors to look at the NAFC region.

p 6480, l 5: "8 year periods"

p 6483, l 9: I am not sure Lelieveld et al (2002) is very relevant for discussing trends in cloudiness over Central Africa.

p 6487, l 9: "in stead" should read "instead"

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p 6488, IPCC reports: please indicate number of pages of both reports and editors of IPCC (2001).

p 6488, l 3: Chagnon should read Changnon.

p 6489, l 1-8: please do not capitalicize all words of titles.

p 6492, figure 2: a color scale and a land mask are missing.

p 6493, figure 3: land contours are not very readable.

p 6498, figure 8: "voulme" should read "volume", "layer" should be "layers". More important for which year is this plot?

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