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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A twenty-year study on natural and manmade global interannual fluctuations of cirrus cloud cover" *by* K. Eleftheratos et al.

K. Eleftheratos et al.

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General: We would like to thank the reviewer for the useful comments and suggestions. All of them have been taken into account.

Specific:

Comment 1: The title reads more like taking 20 years to complete the study.

Response to Comment 1: To avoid any confusion, the title has been changed to "A study on natural and manmade global interannual fluctuations of cirrus cloud cover for the period 1984-2004".

Comment 2: The "Introduction" can be shortened, a few examples: a. The last few

sentences of the second paragraph sidetracks the focus; b. Again the second-to-last paragraph can discuss the usage of ISCCP data within the framework of the present study, rather than side track to the radiative effect. c. The last paragraph does not fit in here, and the usefulness from climate model evaluation is already in the "Summary and Conclusions" section.

Response to Comment 2: The "Introduction" has been shortened as follows: a. On Page 95 (lines 12-16), we have removed the last sentence "The solar and thermal infrared radiative properties of cirrus clouds depend on their composition and physical location in the atmosphere, but unfortunately the way which climate change affects cirrus clouds and the way with which changes in cirrus cloud properties affect climate variability are not well understood." b. On page 96 (line 27), we have removed the sentence "We know that cirrus is a radiatively important cloud type because it produces net warming." c. On Page 97, we have removed the last paragraph (lines 7-12).

Comment 3: Since the study focuses strictly on cloud "cover", the use of cirrus "properties" is not appropriate. Along this line, it is not clear whether the cirrus cloud cover here really means the "effective" cirrus cloud, i.e., considering both the cover and optical properties.

Response to Comment 3: We have made the appropriate corrections: On page 96 (line 29), we have replaced "type properties" with "cover". On Page 97 (line 3), we have replaced "field" with "cover". Now it is clear that the cirrus cloud cover considers only the cover and not the optical properties.

Comment 4: In the first two paragraphs of Section 2, more elaboration is needed on the use of optical thickness ($<$3.6) and cloud top pressure ($<$440 mb), and the choice of vertical wind and relative humidity at 300 hPa for the correlation study. For the latter, there is a need to explicitly discuss the differences in the variability at other levels.

Response to Comment 4: The use of optical thickness ($<$3.6) and cloud top pressure ($<$440 mb) for cirrus clouds is a matter of definition in the ISCCP satellite cloud dataset.

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To clarify this issue, in the first paragraph of Section 2, on page 97 (lines 22-23), we have added the following: "In the ISCCP D2 satellite cloud dataset, cirrus clouds are defined as those clouds with optical thickness less than 3.6 and cloud top pressure less than 440 mb." To clarify why we have chosen the level of 300 hPa for the analysis, in the second paragraph of Section 2, on page 98 (line 4), we have added the sentence: "The level of 300 hPa was selected in this study as a compromise for an upper tropospheric level for both the tropics and the middle and the high latitudes. However, we have looked also at 200 hPa for the tropics." On Page 102 (line 27), we have added the following sentence: "At the level of 200 hPa in the tropics and subtropics the respective correlation coefficients between cirrus clouds and vertical velocities are similar to those observed in Fig. 3a."

Comment 5: There is a lot of information in Section 3.1, but not quite sure about what they are intended for. There is no need for footnotes.

Response to Comment 5: The intention for the information in Section 3.1 was to give a short overview of the basic climatological characteristics of cirrus clouds from the ISCCP dataset on which we base all the analysis. To clarify this issue, on Page 99 (line 11), we have added the following sentence: "In Sect. 3.1 we provide a short overview of the basic climatological characteristics of cirrus clouds from the ISCCP dataset on which we base all the analysis." In Section 3.1 we have removed the footnotes.

Comment 6: The statements in Section 3.2, "VV300 and RH300 are not independent variables" raise more questions, see #4 above.

Response to Comment 6: We have removed the statements in Section 3.2, on page 103 (lines 10-15), in order not to make any confusion.

Comment 7: The discussion in Section 3.3 on the distinction between natural and manmade cirrus in the second-to-last paragraph is inadequate, leaving the impression of many uncertainties and speculations.

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Response to Comment 7: On Page 109, lines 21-26 have been complemented as follows: "As can be seen from Table 2b, there is a positive trend in CCC over northern Europe and the eastern part of NAFC, which in the long-term amounts to about +1.6% per decade. This trend has been calculated after removing from the time series of CCC the fraction of the natural variability that is explained by NAO (shown in Fig. 9b by the grey line). This trend is statistically significant at the 95% confidence level and compares well with the observed positive manmade trends in cirrus clouds over congested air traffic regions in Europe and the North Atlantic as have been evidenced from earlier studies (Zerefos et al 2003; Stordal et al., 2005; Stubenrauch and Schumann, 2005). On the other hand, over the other region which is influenced by NAO (25oN-40oN, 30oW-20oE), there is a negative trend in CCC of about -0.5% per decade, which follows the large-scale negative trends in CCC observed by ISCCP over most of the northern mid-latitudes. It should be noted that the respective trends in VV300 over the two studied areas are of the same sign (the order of +1.5 mPa/s per decade), which suggests that the differences in trends in CCC over the two regions cannot be attributed to differences in trends in vertical winds."

Comment 8: Section 4 can also be shortened by focusing on the issues relevant to the data sets used and key findings. The aspect of model evaluation in the last paragraph is important, but it needs to be more focused following the more concrete findings from the study.

Response to Comment 8: On Page 111, lines 18-29 have been replaced by the following: "After removing the fraction of the interannual variance that is explained by NAO, we find a significant increasing trend in CCC of about +1.6% per decade. This trend may be related to manmade cirrus contrails. On the other hand, over the other region which is influenced by NAO (25oN-40oN, 30oW-20oE), there is a negative trend in CCC which follows the general negative trends observed by ISCCP over most of the northern mid-latitudes. QBO and ENSO were not found to be significantly correlated with variations in cirrus clouds over the northern mid-latitudes." On Page 112 (lines 1-

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8), the aspect of the model evaluation has been improved: "The results of this work can be used to evaluate cirrus cloud cover and variability in climate model simulations. It is important for the climate models to get the cirrus covers right and this work can form the basis for an evaluation of both the mean cirrus cover and scales of variability but also can give modellers a chance to examine whether the models simulate the dependence of cirrus cover on basic dynamic and thermodynamic parameters. The global relationships between cirrus cloud cover and seasonal variability, ENSO and NAO derived in this paper can be tested in climate model simulations in order to quantify cirrus cloud dependencies in the model atmosphere. This would allow modellers to attribute model cirrus cloud deficiencies to either incorrect representation of atmospheric dynamics processes or to problems in the parameterization of cirrus cloud formation."

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 93, 2007.