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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Space-based evaluation of interactions between pollution plumes and low-level Arctic clouds during the spring and summer of 2008" *by* K. Tietze et al.

Anonymous Referee #2

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This paper evaluates the relationship between cloud properties derived from satellite data and the concentration of a pollution tracer from a chemical transport model. This method provides some advantages to using satellites alone to constrain the aerosols and clouds. The authors succeed in presenting some interesting empirical relationships and the paper deserves publication based on this contribution. However, the discussion succeeds only in proposing plausible explanations for the observations without demonstrating their validity.

Major concerns:

1) The paper often appeals to cloud-top temperature and pressure values as tracers of

air mass transport. This assumption is central to the methodology of the paper, where sorting of the data by cloud-top temperature and pressure is employed as a means of controlling for coincidental variations of aerosol and cloud that are not related to aerosol-cloud interactions (p. 29119, lines 16-18), and is also central to the interpretation of the results, where cloud-top temperature and pressure are taken as indicators of air mass age and origin (e.g. p.29130, lines 14-16). Presumably this approach is hampered by the fact that clouds coincide with mixing processes, which would act to dilute the properties of the original air mass. The strength of the methodology and the interpretation of the results seem to rely quite heavily on how well these measurable cloud-top properties represent traceable air mass properties. Can the authors cite a study or provide and argument for why cloud-top temperature and pressure can be interpreted as representative of air mass properties that are conserved during transport? Is figure 8 really showing the properties of different isentropic surfaces as suggested on P. 29128, line 19?

2) A number of intriguing results are found when the dependence of the cloud optical thickness, cloud drop effective radius, and cloud liquid water path are evaluated for narrowly defined cloud-top pressure and cloud-top temperature bins. These include a reduced indirect effect at temperatures both less than and greater than 0 °C, and a weaker IE for "graybody" clouds than "blackbody" clouds. Curiously, wet deposition of aerosols is invoked to explain the reduced IE for both high temperatures and low temperatures. Unfortunately, wet deposition is not explicitly evaluated in the study, either in the observations or in the FLEXPART modeling, leaving the explanation for the temperature effects rather unconvincing. Given that the mechanisms invoked in the discussion were not explicitly tested in the analysis, I think the authors should be more cautious in assigning specific reasons for their observed IE values. Can other possible explanations be excluded? Could not lower values of IE coincide with weaker updrafts or shallower clouds?

3) I must be misunderstanding figures 6 and 9. It appears that in many cloud-top

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temperature bins the IE is greater for all samples regardless of LWP than it is for either samples with LWP<40 g mˆ-2 (labeled “graybody”) or for samples with LWP>40 g mˆ-2 (“labeled blackbody”). Are there additional samples included in the “all LWP” sample that are excluded from either the graybody or blackbody samples? Likewise, in figure 9, the ratio of the IE for cloud optical thickness to the IE for cloud drop effective radius is substantially greater for the all LWP samples than for either of the other two groups of samples. How can this be if the graybody and blackbody samples are simply a division of the same data as the all LWP group?

Minor concerns:

4) A feedback process related to the LWP is mentioned in the abstract and elsewhere, without clearly explaining what that feedback process is. Is this process the cause of the IE_LWP? Or is the IE_LWP better explained by a suppression of rainfall, as is typically argued?

5) The success of the FLEXPART model in predicting CO concentrations is mentioned only qualitatively (“agreed well”, P. 29123, line 5). Presumably the reference cited provides a quantitative assessment. Given that the explanation for the observed differences in IE is not explicitly tested in this study, one motivation for publishing the empirical results would be to provide a basis for a future comparison with model derived IE. But without some quantitative uncertainties, such as on the reliability of the FLEXPART CO, it will be difficult to evaluate how well a model calculation agrees with the observation. At the very least the paper should cite a quantitative uncertainty in the CO values from the literature. The results of this paper would be substantially more useful if the uncertainty in the FLEXPART CO could be translated into an uncertainty in the IE attributable to FLEXPART errors.

6) It is stated that FLEXPART grid boxes with less than 50% cloud coverage are excluded from the analysis (P. 29125, line 23). How much of the total cloud cover in the sample set is left out because of this constraint?

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Interactive Comment

7) Actual values of the probability densities are left out of figures 4 and 5. The qualitative points of the figures are conveyed, but they seem rather naked considering you went to the trouble of quantitatively processing a mountain of data.

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