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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.

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We are very appreciative to the reviewer for the careful read through of our manuscript, which enabled us to clarify some important points. We address the comments (bold) in a point-point fashion (normal) with associated changes made to the manuscript (italics).

In their study, the authors use a relatively new technique of combining satellite observations of cloud and carbon monoxide estimates from an off-line tracer transport model as a proxy for cloud-active aerosol to study the possible effects of pollution on low-level Arctic clouds. CO is used because it is itself unaffected by cloud processes. They find that the indirect effect (regression of CO) on cloud

optical depth is four times larger than for cloud droplet radius.

This is an interesting finding because cloud optical depth is suggested to be controlled by cloud droplet radius in the first aerosol indirect effect hypothesis. It would be in support of two recent studies suggesting that either dynamical cloud-radiative feedbacks (Garrett et al. 2009) or drizzle-aerosol effects (Mauritsen et al. 2010) might be more important, at least in the Arctic, than the first aerosol indirect effect suggested in earlier studies.

The paper is well-written, as I found only minor glitches pointed out below, albeit a bit long in my taste, while figures are in a good state. My major concerns with this study is the linkage from correlation to causality and the quality itself of the data analysis upon which the main conclusion of the study is drawn. How come the indirect effect estimates for LWP and cloud optical thickness is much larger when including all data as opposed to sub-dividing it into greybody and blackbody cases? See Figure 6 a-c). Note that the sum of cases in the two sub-divided categories is the same as when all cases are included.

I suspect part of the answer is that spurious correlations occur because large-LWP cases might come from certain regions or under certain meteorological conditions and low-LWP cases from others. That is, the high indirect effect estimate on cloud optical thickness when looking at all cases may be the result of processes which have little to do with pollution. If the authors would base their conclusion on Figures 6 b) and c), rather than a), they would say the enhancement is only a factor of two. If they had sub-divided the data further, the enhancement might have been even less.

This last point on the magnitude of the enhancement gets to a rather crucial question on how to analyze the data, one that we should have elaborated on further. Consider that in the limit of infinite sub-division, there could be no enhancement. That the magnitude of the enhancement is nearly identical for graybody and blackbody clouds

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suggests that, if a single mechanism is at play, it must apply over the full range of LWP. If so it would point more to drizzle-aerosol effects than cloud-radiative feedbacks, since the latter apply only to graybodies. To address this concern, we have added to the discussion, the following remark.

Note that any constraint on LWP is necessarily going to constrain the magnitude of any associated enhancement of IE_τ: the sum of the enhancement factors for graybody and blackbody clouds is equal to that for all clouds. What is implied, however, is that the magnitude of the enhancement is not specific to clouds of any particular thickness.

Thus, our results suggest that LWP in Arctic low-level liquid clouds is more sensitive to mid-latitude pollution plumes than is r_e. This is surprising, given that the most simple understanding of cloud physics is that values of LWP are determined primarily by thermodynamic constraints rather than aerosol concentrations. We cannot isolate an exact physical mechanism from the observations. One possibility, though, is that enhancement at low LWP may be indicative of a infrared radiative feedback process that accelerates cloud development when clouds are thin and polluted. More likely, given that the observed enhancement of IE_τ is not specific to a particular range of cloud LWP, is that suppression of warm rain and drizzle by pollution aerosol may lead to a long term thickening of liquid stratiform clouds.

Why do the authors use POLDER to derive cloud top height, when CALIOP is available in the A-train?

CALIOP has too limited a footprint and swath to be useful for our application. To match cloud fields to FLEXPART fields with sufficient statistical representativeness, POLDER is what is suitable. What we showed is that the CALIOP can be used to assess the validity of using POLDER derived cloud top pressure for low liquid clouds.

The reason for using POLDER rather than CALIOP data for matching cloud fields to FLEXPART data is that POLDER has a much larger footprint and swath, which provides much higher statistical representativeness.

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On several instances the authors discuss how MODIS cloud top heights have trouble due to surface temperature inversions. It is well-known that the lower troposphere exhibits a semi-permanent inversion, which is most likely due to transport of warm air from the south at mid-levels, while the surface or the boundary-layer clouds loose heat to space from below. This causes the atmosphere to be nearly the same absolute temperature in the lowermost 2-3 kilometers. See e.g. Kahl et al. (1996).

This is true. Temperature often even increases with height and a corresponding statement can actually be found in the paper: "Nonetheless, PO2 cloud top pressure from POLDER is preferred over MODIS cloud top pressure retrievals because the PO2 algorithm does not utilize infrared channels that require an assumed temperature profile. Further discussion on this point is described in section 3.3. We find that for low-level clouds in the Arctic, MODIS retrievals of cloud heights can be several kilometers too high."

Page 29115, line 19, I think it would be appropriate to cite Stevens and Feingold (2009) here.

Done

What is the rationale for removing CO after exactly 20 days? Might one not just as well use a general inert tracer?

A general inert tracer could have been used as well as CO which is actually treated as such. We use CO mainly because its sources are mainly combustion processes, which also generate CCN. The removal after 20 days has two reasons: 1) In a Lagrangian model, computation time increases with the number of particles tracked and, thus, not removing particles at all would have been computationally extremely expensive; 2) After 20 days, the contribution of individual CO sources is normally quite limited. Instead, the contribution of indvidual sources mixes into a CO background. Thus, while tracking the CO tracer longer would give higher CO concentrations overall, the spatial variability

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of CO would not change much. Since our analysis depends solely on CO variability (i.e., the capability of the model to resolve individual pollution plumes), disregarding the CO background does not affect our results. Thus, while the 20 days cut-off is somewhat arbitrary, the results are not sensitive to the exact choice of this value. This is further supported by the fact that the CCN lifetime is typically shorter than 20 days, i.e., variability in the background is even less important for CCN. The text has been revised to read

Unlike CCN, however, the χ_{CO} tracer is merely passive, and it is affected only by dilution. It does not interact with or influence clouds. In order to focus on the variability contributions associated with long-range transport to the Arctic, FLEXPART instantly removes all CO that has had twenty days atmospheric residence time.

Page 29121 line 29, the use of the SWIR abbreviation does not make sense here. Also, do you really mean 'near-infrared' instead of 'shortwave infrared'?

SWIR may be jargon, but it is how MODIS refers to the bands. There is no contradiction in refering to the infrared portion of the shortwave spectrum as a contrast to the visible portion. "infra" mean "below", so below-red.

Page 29122 line 13, replace 'and produces' with 'was set up to produce', unless this is fixed of course.

changed

Page 29125 line 1, I don't think clouds can lie. Maybe something is missing in this sentence.

The English is fine

Page 29127 line starting at 14 is very long and difficult to read.

Agreed. The text now reads

For the span of this study, we find that biomass burning is clearly affecting the com-

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position of the Arctic lower troposphere. Independent of potential temperature, it contributes to approximately half of the total FLEXPART CO concentrations when clouds are present.

Same page, line 21, I would suggest to insert 'roughly' before 'near

Near is fine

Page 29128 line 8, the sentence needs some rewriting.

The sentence now reads

Unfortunately, this remote region is lacking in studies of the emission characteristics of biomass burning CCN.

Page 29131 line 15, here and in several other places I get the impression that the authors interchange cloud optical thickness and LWP. Please check through the entire manuscript to ensure consistence.

The statement is accurate as is, however to add some clarification we have altered the sentence slightly to read

Thus, our results suggest that LWP in Arctic low-level liquid clouds is more sensitive to mid-latitude pollution plumes than is r_{e}, and therefore it is changes in LWP that are most important to the aerosol indirect effect.

Page 29131 line 24, I would cite Mauritsen et al. (2010) here.

Done

Page 29132 line 3, replace 'is' with 'could be'.

The whole sentence got massaged. It now reads

Thus, this feedback potentially could be contributing to our observation that cloud LWP is highly sensitivity to pollution, even if it would not explain the large enhancement factors (EF) that are observed at high temperatures (Figure 9)

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Page 29132 line 23, this sentence seems a bit too general to serve a purpose here, alternatively you need to cite some far older papers.

We added a reference to Shaw 1995, which provides an overview of the Arctic Haze problem

Page 29133 line 15, this sentence needs rewriting

"activating" was change to ", and they activate"

Page 29133 line 20, I would consider omitting 'strongly'.

Done

Page 29134 line 25, in the the first aerosol indirect effect hypothesis LWP is assumed to be unaffected by aerosol. Hence the last sentence here is self-contradicting.

The sentence seems fine as is, because we are referring to *enhancements* of the first indirect effect, i.e. a change in LWP that is in addition to what would be expected if LWP were fixed.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29113, 2010.